



Of Words & Numbers. The Writing of Generality in the Emergence of Enumerative Geometry (1852-1893)

Michel Nicolas

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Université de Paris

École Doctorale 623 – Savoirs, Sciences, Éducation

Laboratoire UMR 7219 – SPHère (Sciences, Philosophie, Histoire)

Of Words and Numbers

The writing of generality in the emergence of enumerative geometry
(1852-1893)

par Nicolas MICHEL

Thèse de doctorat d'histoire et de philosophie des mathématiques

Dirigée par Karine CHEMLA
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Abstract:

This dissertation deals with the genesis and the reception of the “theory of characteristics”, a mathematical theory first formulated between 1864 and 1867 by French geometer Michel Chasles (1793-1880). This theory served to enumerate and construct, in a systematic and uniform manner, the conic sections satisfying five given geometrical conditions, without resorting to algebraic computations. This theory was for Chasles the culmination of a broader research programme, which he had undertaken throughout his lectures on Higher Geometry at the Sorbonne since 1846. Through his teaching, Chasles sought to reinvent the language and concepts of pure geometry, to provide it with a generality equivalent to that of algebra.

Chasles’ theory quickly circulated across mathematical Europe, and was reworked by many, including Hieronymus Zeuthen (1839-1920), Georges-Henri Halphen (1844-1889), Hermann Schubert (1848-1911), and Eduard Study (1862-1930). Amongst these readers, however, few shared Chasles’ epistemological stands; and many set out to rewrite the key concepts of the theory of characteristics with entirely new technical and notational means, and in the framework of other epistemological choices. From these simultaneous rewritings, a formula identified but not proven by Chasles emerged as problematic: it had been successively proven and refuted several times, and soon became the object of several controversies between 1867 and 1893. These controversies were more than technical disputes over a mathematical proposition: more profoundly, they revealed important disagreements over the epistemological value of generality in mathematics, over the way in which a general object is constructed in geometry, and over the textual practices through which said generality ought to be materialized on a page.

The first part of this thesis locates the genesis of this theory within the context of Chasles’ reflection on the history and philosophy of geometry, and in particular on the question of the generality of methods. Then, in a second part, we show that the reception of the theory of characteristics allows for a new perspective on the formation of modern mathematics, at the intersection of the cultural history of science and of the technical history of mathematical practices.

Keywords : History of mathematics, enumerative geometry, conic sections, generality, epistemological value, Chasles, Schubert, Halphen, Study

Résumé:

Cette thèse traite de la genèse et de la réception de la “théorie des caractéristiques”, une théorie mathématique formulée initialement entre 1864 et 1867 par le géomètre français Michel Chasles (1793-1880). Cette théorie visait à dénombrer et à construire, de manière systématique et uniforme, les sections coniques satisfaisant cinq conditions géométriques données, sans recourir au calcul algébrique. En cela, elle s’inscrivait pour Chasles dans un programme de recherche plus large, qu’il avait entrepris notamment au sein des cours de Géométrie Supérieure qu’il donnait à la Faculté de Paris depuis 1846. Par son enseignement, Chasles cherchait à réinventer le langage et les concepts de la géométrie pure pour lui donner une généralité équivalente à celle du calcul algébrique.

La théorie de Chasles connut une réception rapide à travers l’Europe mathématique, et elle fut retravaillée par, entre autres, Hieronymus Zeuthen (1839-1920), Georges-Henri Halphen (1844-1889), Hermann Schubert (1848-1911), et Eduard Study (1862-1930). Parmi ces lecteurs, toutefois, peu souscrivaient aux thèses philosophiques de Chasles ; et beaucoup décidèrent de réécrire les concepts clefs de la théorie des caractéristiques au travers de tout nouveaux outillages techniques et notationnels, et dans des cadres informés par de toutes autres positions épistémologiques. Au fil de ces réécritures concurrentes, une formule identifiée mais non démontrée par Chasles se révéla être problématique : elle fut successivement prouvée et réfutée à plusieurs reprises, et devint ainsi l’objet de plusieurs controverses entre 1867 et 1893. Ces controverses étaient bien plus que des disputes techniques autour d’un point de mathématiques: plus profondément, elles traduisaient des désaccords importants autour de la valeur épistémologique de la généralité en mathématiques, de la façon dont on peut fabriquer en géométrie un objet général, et des pratiques textuelles par lesquelles ladite généralité doit être couchée sur le papier.

Dans un premier temps, cette thèse situe la genèse de cette théorie dans le contexte de la réflexion de Chasles sur l’histoire et la philosophie de la géométrie, et notamment sur le thème de la généralité des méthodes. La seconde partie de cette thèse s’attache alors à montrer que la réception de la théorie des caractéristiques permet une nouvelle perspective sur la formation des mathématiques modernes, à la croisée de l’histoire culturelle des sciences et de l’histoire technique des pratiques mathématiques.

Mots-clés : Histoire des mathématiques, géométrie énumérative, sections coniques, généralité, valeur épistémologique, Chasles, Schubert, Halphen, Study

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Introduction

DIE ZAHL. — Die Erfindung der Gesetze der Zahlen ist auf Grund des ursprünglich schon herrschenden Irrthums gemacht, dass es mehrere gleiche Dinge gebe (aber thatsächlich giebt es nichts Gleiches), mindestens dass es Dinge gebe (aber es giebt kein “Ding”). Die Annahme der Vielheit setzt immer voraus, dass es Etwas gebe, das vielfach vorkommt: aber gerade hier schon waltet der Irrthum, schon da fingiren wir Wesen, Einheiten, die es nicht giebt.

F. Nietzsche, *Menschliches, Allzumenschliches*, 1886

The conflicted emergence of enumerative geometry

In 1914, at the end of a long and productive career, the Danish geometer and historian of mathematics Hieronymus Zeuthen (1839-1920) wrote a textbook on “enumerative methods”, a branch of geometry in whose emergence and development he had been a key actor. In the first chapter of this *Lehrbuch*, intertwined amidst technical discussions pertaining to the algebraic invariants of plane curves, multi-dimensional spaces, or even line-geometry, one finds a few other, more surprising paragraphs. The very first section of the book discusses “the meaning of numbers” (*‘Bedeutung der Anzahlen’*), whereas a few pages further, Zeuthen invites us to consider “the relativity of the concepts of the general and the particular¹” (*‘Relativität der Begriffe “allgemein” und “speziell”*). This meshing of the mathematical and the epistemological was not merely the reflection of an aging geometer, looking back upon decades of hard-earned results and knowledge. Rather, all of it was an integral and necessary part of Zeuthen’s definitive answer to a series of diffi-

¹ [Zeuthen, 1914a], pp.1-3; 9-12.

cult questions which had stirred up disputes throughout the early history of enumerative geometry.

Zeuthen’s involvement with enumerative methods runs throughout his entire career. Indeed, when he wrote his dissertation in 1865 for the University of Copenhagen, he had just come home from a scientific trip to Paris, where he had studied with the French geometer Michel Chasles (1793-1880). At the time of Zeuthen’s stay, Chasles was on the verge of a scientific breakthrough: building on the methods and concepts he had expounded through his lectures at the Faculté de Paris, he had obtained a general and uniform method for the enumeration and construction of all conic sections in the plane which simultaneously satisfy five given conditions. In February 1864, during one of the weekly meetings of the Paris Académie des Sciences, Chasles made public the scope of his discoveries. Over the following months, he came back frequently to the Académie to communicate through a series of short papers the technical details of his new theory, which he named the “theory of characteristics”.

As a historian of mathematics, widely praised for his erudition, Chasles was keenly aware that enumerative problems had a long history. The so-called Apollonius problem, which consists in the construction of the circles in the plane which satisfy three conditions of the form ‘passing through a given point’, ‘touching a given straight line’, or ‘being tangent to another given circle²’, was already discussed in Book 4 of Pappus’ *Collections* (4th century C.E.), and was solved anew by many representatives of the early-modern tradition of problem-solving³. At the École Polytechnique, where he had trained between 1812 and 1814, Chasles had been taught techniques to visualize and solve even more difficult contact problems, such as that of constructing the spheres touching four other given spheres⁴. More crucially, the enumeration of conics had been taken up by various other geometers in the 1850s, after Steiner’s (erroneous) enumeration in 1848 of the conics touching five given conics⁵.

However, Chasles’ new theory was incomparably more general than these past results in at least two ways. It enumerated conics satisfying any possible condition, far beyond conditions defined by lines, points or circles. Furthermore, Chasles’ enumerations dramatically extended the scope of what counted as a solution: points or lines at infinity,

²A circle being defined by its center and its radius, that is to say by the general equation $(X - a)^2 + (Y - b)^2 = r^2$, can satisfy three independent conditions, such as those indicated above, at the same time.

³ [Pappus, 2010], see pp.20-26 for the Greek text, pp.91-99 for an English translation, and pp.193-202 for a commentary. For a *longue-durée* study of the history of this problem, see [Boyé, 1998]. For early-modern approaches in the context of the tradition of problem-solving, see for instance [Bos, 2001], pp.110-112 where Van Roomen’s analysis is presented, or [Maronne, 2010], pp.543-548, for a discussion of how Pascal and Descartes solved it.

⁴This problem was the subject of a memoir by Charles Dupin in 1807, see [Belhoste, 2009].

⁵ [Steiner, 1848]. Steiner thought there were $6^5 = 7776$ such conics; however, Chasles’ theory refutes this number, and shows that the answer is in fact 3264.

imaginary elements, or degenerate curves were all legitimate figures to be counted.

The publication of this theory in 1864 was an immediate international success, both on an institutional and scientific level. Chasles would be awarded the Copley Medal by the Royal Society of London the very next year; and papers were published about his new discoveries over the next decade not only in French and Danish, but also in Italian, German, and English. However, despite the interest generated by the theory of characteristics, neither Chasles' methods nor his notations and concepts were preserved by the authors who took up his results. Chasles, a staunch defender of pure geometry in the face of what he perceived to be the hegemony of analytical methods, had based his theory on a technique for the writing of the “geometrical equations” of curves⁶. This technique, he thought, would provide a substitute to the technology of algebraic equations, which, for all their efficiency and computational power, always obfuscate reasoning and hide the real causes behind mathematical truths. However, while the theory of characteristics was met with praise and interest across mathematical Europe, its fate was not to be passively diffused in the way of hard-earned geometrical knowledge. Most of those who saw a worthwhile scientific endeavour in the theory of characteristics were not interested in this broader research programme, and immediately translated Chasles' methods back into the algebraic languages at their disposal. Very little of Chasles' publications remained stable throughout the ensuing series of rewritings this theory underwent: its notations, concepts, methods, but also its goals and its general architecture were all subject to change. More crucial, however, was an unexpected side-effect of these manifold variations.

A formula, which Chasles had observed across hundreds of examples but not proved, had been identified by some as crucial for the theory of conics at large: this formula gave the number of conics satisfying five given conditions as a simple function of four numbers defined by said conditions. In 1873, two proofs were independently produced by Alfred Clebsch⁷ (1833-1872) and Georges Halphen (1844-1889), using respectively the theory of ternary quadratic forms and the algebraic analysis of curves. Three years later, yet another algebraic proof was proposed by Ferdinand von Lindemann (1852-1939), and inserted in his 1876 edition of Clebsch's *Vorlesungen*. Halphen, whilst reading this last proof, was profoundly unsatisfied: he viewed it as at worst begging the question, at best a wholly unrigorous proof in which there remained logical gaps to fill. The shortcomings

⁶Chasles is thus often described as member of the so-called ‘synthetic tradition’, a label which designates those who defended ‘pure geometry’ against the perceived hegemony of analytical methods, that is to say methods based on algebraic or infinitesimal analysis and axes of coordinates. The boundary between analytic and synthetic geometries in the nineteenth century is a notoriously fleeting one, often to be negotiated on a case-by-case basis, see [Lorenat, 2016]. We shall discuss it in greater detail as regards the geometry of Michel Chasles in 1.1.

⁷Clebsch's proof was written up in May 1872, a few months before his death. It was published posthumously the following year.

he had observed in Lindemann's proof, he soon came to find in Clebsch's, and in his own. Eventually, he came to a shocking realization: not only were these proofs lacking, but the formula was in fact outright false! In September 1876, Halphen announced his discovery to the Association Française, as well as to the Paris Académie des Sciences, by means of a counter-example. A few months later, he announced an even stronger negative result: not only was Chasles' formula false, but his new theory showed in fact that no such simple formula could be generally true.

However, not all were convinced by the significance of Halphen's counter-examples. Among the dissenters was Hamburg-based mathematician Hermann Schubert (1848-1911). From 1874 onwards, Schubert had been devising a fruitful symbolical calculus, building on formal regularities he had observed in the results of his colleagues, and, crucially, on Chasles' formula – of which he had himself given yet another proof in the wake of Halphen's announcement. Schubert's calculus, and in particular his 1879 book *Kalkül der abzählenden Geometrie*, impressed many, if only by the sheer number of new and difficult results Schubert had been able to obtain with his idiosyncratic methods. To accept Halphen's sharp arguments against the perceived lack of rigor and analytical precision of his predecessors, for geometers at large, meant to agree that the numerous proofs of Chasles' formula produced by esteemed mathematicians were flawed, and to renounce the embarrassment of riches provided by Schubert's methods. In 1885, Felix Klein (1849-1925), then a professor in Leipzig, wished to see the matter resolved, and tasked one of his most promising students, Eduard Study (1862-1930), with writing his *Habilitationsschrift* on the subject. Study, against Halphen, found the theorem to be true, and gave a proof thereof. In subsequent years, he visited Paris, but failed to convince Halphen.

Zeuthen had been an actor of this entire episode. A student of Chasles, his dissertation proved crucial to the development of Schubert's calculus. A friend of Halphen's, with whom he had a sustained private correspondence throughout this historical episode, Zeuthen contributed to circulating and explaining the latter's refutation of Chasles' formula. A trusted colleague of Klein, he convinced the latter to stage a public debate with Study within the pages of the *Mathematische Annalen*, and was later invited to write the entry on enumerative methods for the *Enzyklopädie der mathematischen Wissenschaften*. Thus, Zeuthen's 1914 *Lehrbuch* was the scientific testament of a mathematician who had spent decades discussing and disputing the generality and validity of theory of characteristics, of Chasles' formula, and of Schubert's calculus. However, even by then, the validity of Chasles' formula had not been collectively decided by geometers.

These difficulties had arisen from the fact that there are many possible ways to define generality in geometry, and to inscribe it into equations or other textual practices. This is why Zeuthen, in his *Lehrbuch*, needed to weave together mathematics and epistemology.

To decide whether an enumerative result was correct or not, one had first to agree on what kind of generality was to be demanded from such results, and what the (geometrical) numbers it produced – the results of enumerative methods – were and ought to be. Indeed, while it appeared clearly to both Halphen and Study that the crux of the problem raised by Chasles’ theory was that of the nature of generality in geometry, they disagreed on almost everything else. Halphen viewed his own work as the uncovering of the false generality of the formulae of his predecessors through the expert use of analytical methods: from a skillful manipulation of analytic equations, he had found irregularities which undermined the formulae of Chasles’ and others. Study, in his defense of said formulae, proposed another account of generality in geometry; one that is not so vulnerable to the construction of counter-examples. For Study, generality was not the feature of a theorem or a method, but a mathematical construction, carried out by arbitrarily selecting a property of an object as the basis for a new definition. To save Chasles’ formula, Study explained, one simply has to choose a different property as the basis for a general concept of solution. Counter-examples, in this view, are simply the sign that the wrong property has been selected in view of the theorem which is to be preserved – for instance, because its simplicity makes a theory viable or worthwhile.

After Halphen’s untimely death in 1889, others would maintain the validity of his arguments against Chasles’ formula. This list includes Zeuthen himself, but also members of the so-called Italian school of algebraic geometry, such as Francesco Severi (1879-1961). In fact, even in recent decades, there have been geometers to defend this position⁸. Meanwhile, Chasles’ formula would be vindicated by the topological framework for algebraic geometry put forth in the 1920s by Bartel Van der Waerden (1903-1996). By then, however, the theory of characteristics had, by and large, lost its central importance for algebraic geometers at large. After Hilbert’s 1900 Paris address, a much more pressing concern had been identified in the justification of Schubert’s calculus. Much of this calculus, and of its notations, is predicated on results similar to Chasles’ formula: thus, the search for foundations for the former yielded facile proofs of the latter.

That it is possible to choose between two options with regard to the validity of Chasles’ formula is a banal fact of today’s mathematical life: it simply means that one can choose between two formalizations of the problems. Conversely, these 19th-century disputes often make little sense to modern-day mathematicians⁹. Furthermore, that generality in mathematics can be constructed by selecting statements that are viewed as valuable and

⁸ [Casas-Alvero & Xambò-Descamps, 1986].

⁹For instance, the American mathematician Steven Kleiman, a leading expert in contemporary enumerative geometry, viewed the Schubert-Halphen dispute as the conflict between “classical geometers [who] had a platonic view of figures like conics”, in contrast with modern geometers who know conics can and in fact ought to be constructed partly upon an arbitrary definition; see [Kleiman, 1976a], p.133.

taking them as axioms, is very much part of today’s ‘normal science’. Describing Noether’s theorem, which is an important result in algebraic geometry pertaining to the intersection theory of plane curves, the Indian-American mathematician Shreeram Abhyankar expressed in 1976 the following *dictum*¹⁰:

In other words, Noether’s theorem must be qualified by giving a “good” meaning to the phrase “H passes through..”. What meaning to give? ANSWER: That which will make Noether’s theorem true.

Indeed, the peculiar wisdom of geometrical algebraic geometry is the

Dictum. Study of simple cases gives rise to a nice succinct statement. Take it as an axiom that the statement is true most generally. Make it true by the provision that we learn to “count properly” the intervening quantities. Or better still, have faith that god (or, if you prefer, nature) has good meaning in mind, and march on!

These epistemic resources, however, were not as readily available to 19th-century authors such as Zeuthen: rather, they had to construct their own categories to make sense of the undecidedness of this result whilst still arguing for one of these two options. Thus, by retracing the conflicted emergence of enumerative geometry, and by focusing on the emergence of Chasles’ theory of characteristics and the ensuing debates, this dissertation examines the rise of modern mathematical practices of generality.

Generality: Concepts, Values, Practices

Generality has proven a particularly fruitful object of historical study in recent years, especially in the case of mathematics¹¹. In particular, generality has been studied as an epistemic and as an epistemological value, that is to say a norm for scientific practice whose observance conduces respectively to truth in whatever fashion possible, and to a specific desirable form of knowledge¹². The interest of studying such values is manifold. One reason of particular importance to us is that such values have been shown, in a wide variety of cases, to be co-constructed with specific textual practices. Because practitioners from a certain milieu value a certain kind of generality, they shape textual practices conducive to knowledge bearing this very generality. This is the case, for instance, of the mathematical texts composing the canon passed down in the “School of Mathematics”

¹⁰ [Abhyankar, 1976], p.413. I thank Sarah Griffith for drawing my attention to this text.

¹¹See for instance [Netz, 1999], pp.240-270, [Chemla, 1998], [Robadey, 2004], [Grattan-Guinness, 2011], [Acerbi, 2011], [Chemla *et al.*, 2016], [Vergnerie, 2017].

¹² [Chemla *et al.*, 2016], pp.7-8. Of course, as the authors insist, many values such as generality can lie on both sides of this distinction.

of the Chinese Imperial University starting from 7th century C.E., as analyzed by Karine Chemla¹³. A textual practice commonly found in these texts is the presentation of a paradigmatic example (with concrete values), followed by a proof of the correctness of the procedure. Through this latter proof, actors assess whether the procedure relied or not on singular features of the paradigmatic example: if it did, then modifications will often be brought to the procedure by subsequent scholars, aiming for ever greater generality. Similarly, different ideals of generality shaped the scientific work of enumerative geometers in different ways, and motivated the introduction of different textual practices. Conversely, textual practices help bring forth or transform epistemic values. For instance, we shall describe in this dissertation how the shaping of new notations for cross-ratios and homographic divisions, in a pedagogical context, brought Chasles to think anew his understanding of generality, and to link it with the notion of abstraction¹⁴.

However, as Zeuthen had noticed, generality served a more fundamental purpose in the context of enumerative geometry: it served as the basis for collective mathematical inquiry (and conversely, a disagreement as to what constitutes generality led to impossible dialogues between mathematicians). The many disputes over the formulae and numbers produced whilst enumerating geometrical figures had shown that one cannot count anything without first deciding what is to be counted, and what the meaning of the resulting numbers is. In other words, for enumerative geometry to emerge as a mathematical discipline, collectives of mathematicians had to first agree on the meaning of propositions such as “through five points goes one and only one conic section”. This proposition is, according to any modern-day textbook, false: one should add the requisit that no three of the five points be aligned. For 19th century geometers such as Chasles, it was not only true, but also one of the most fundamental of all properties pertaining to conics. Without such a proposition, no systematic enumeration of conics can be hoped for. And yet, Chasles, who wrote an entire treatise on the geometry of conics, cannot be accused of ignoring the specificities of the cases wherein three or four of the given points are aligned. Rather, because of his understanding of generality in geometry, he elected to disregard such ‘counter-examples’ and to handle propositions ‘generally true’. This convention, and many others, were necessary for the enumeration of geometrical figures; but it means that the propositions written in the development of this science are only true and meaningful

¹³ [Chemla, 2018].

¹⁴ Another related reason for the interest in epistemic values is that these shared practices they norm and shape have been shown to constitute powerful historiographical categories of the delineation and study of scientific cultures. It appears clearly that many of the practices of generality found in Ancient Chinese texts are totally different from that present in, for instance, Euclid’s *Elements*, documented in the works of Reviel Netz and Kenneth Manders (see [Manders, 2008]). Conversely, the focus on the value of generality and the textual practices associated to it allows for comparisons across geographical locales: for instance, the use of paradigmatic statement has been described in both Ancient Chinese sources, and the work on geodesics of Poincaré (see [Robadey, 2004]).

if read through the lens of a collectively-shared practice of generality.

As an epistemic basis for collective mathematics, generality thus played in the context of enumerative geometry a role analogous to that of objectivity in the natural sciences. Indeed, as historians of science have convincingly shown over the past decades, before scientists can collectively and critically produce knowledge, they must agree as to what constitutes an object of scientific enquiry, a ‘matter of fact¹⁵’. The mechanical recourse to instruments, supposed to erase the individual and the subject from the fact-producing part of scientific activity, or the transformation of repeated observations into spreadsheets of numbers, supposed to nullify the specificities which may arise in an individual experiment, are both activities which can be collectively agreed upon as adequate modes of objectivity on which scientific discourse can be based. Their justification as such is provided by a constellation of some of the aforementioned epistemic values; values such as neutrality, objectivity, or rigour. These values do not simply describe rules one should abide by in order to derive knowledge from, say, an experiment or a reasoning. They are historically-situated norms, which shape communally shared ways of acting qua scientists. They are embodied at a collective level through textual, physical, cognitive practices, such as note-taking, patient observation, data-compiling etc. These activities, which collectives agree to select as the basis for proper scientific practice, in turn serve to constitute a category of facts¹⁶:

All sciences must deal with the problem of selecting and constituting “working objects”, as opposed to the too plentiful and too various natural objects. Working objects can be atlas images, type specimens, or laboratory processes — any manageable, communal representative of the sector of nature under investigation. No science can do without such standardized working objects, for unrefined natural objects are too quirkily particular to cooperate in generalizations and comparisons [...] Even scientists working in solitude must regularize their objects. Collective empiricism, involving investigators dispersed over continents and generations, imposes still more urgently the need for common objects of inquiry.

Of course, these values and their physical embodiments are historical objects. Two simple examples suffice to hint at the profound transformations undergone by the value of objectivity. The first is that of the observation, classification, and representation of snowflakes. In 1755, after months of study, the Dutch physician John Nettis drew and classified the geometrical shapes of all possible snowflakes, as per his observations¹⁷. Strik-

¹⁵The seminal study in that direction is [Shapin & Schaffer, 1985].

¹⁶ [Daston & Galison, 2007], pp.19-22.

¹⁷ [Daston & Galison, 2007], pp.148-151.

ingly, all of his drawings displayed a perfect hexagonal symmetry. And yet, in a footnote to one of these figures, Nettis admitted that he had observed many an asymmetrical snowflake in the field, which he simply thought to be undeserving of representation and study. Asymmetrical snowflakes, for him, simply did not count as knowledge. Geometric regularity was constitutive of objectivity, and not the observation of a particular snowflake. By contrast, some two centuries later, the German physician Erwin Christeller would find pride in his ability to produce purely mechanical depictions of natural objects (in particular, tissue samples and other anatomic object). The irregularities in these depictions – such as the shape of an asymmetrical snowflake or a fracture in the photograph of an organ – were to be left in, as part of the objective result of the observation¹⁸. These different ways of conceiving objectivity, and thereby forming facts for scientific discourse, are related to different regulative ideals for scientific life. Nettis and Christeller, according to Daston and Galison, respectively exemplify the figures of the sage and of the hard worker, that is to say two different cultural forms of the natural scientist¹⁹.

In the case of mathematics, of course, it is not obvious at first what the import of these discussions of collective empiricism might be²⁰. However, as Zeuthen and his contemporaries had noticed, generality plays for enumerative geometry a similar role to the value of objectivity. Enumerative geometry, almost from the onset, was an international endeavour, in which mathematicians from very different scientific cultures, and with different areas of expertise, participated. Only by first collectively agreeing upon norms of generality could these geometers share an enumerative object. And, conversely, the disagreements on such norms, as exemplified by Halphen's and Study's incompatible understanding of what it means to enumerate generally, lead to impossible mathematical dialogue²¹.

Since generality is here taken to be the value that actually defines mathematical objectivity, what was at stake in the debates surrounding the theory of characteristics was in fact the cultural and scientific identity of the mathematician. The variation and succession of these identities cannot be borrowed from the history of the natural sciences. However, by adapting this framework to a more local scale, and by confronting it to a technical study of the mathematical and textual practices present in the historical record, this dissertation elucidates the multiple, conflictuous ways in which generality was conceived, written down, and embodied thus through the history of the theory of characteristics.

¹⁸ [Daston & Galison, 2007], pp.171-173.

¹⁹See the synthetic presentation in [Daston & Galison, 2007], p.371.

²⁰However, in specific contexts, some have argued that experimental life ought to be thought as an essential part of mathematical practice, see [Goldstein, 2008], [Dick, 2011], [Barany & MacKenzie, 2014].

²¹Of course, we are not here arguing that these modes of objectivity determined the truth-value of Chasles' formula in each individual case. Schubert and Study, for instance, both agreed that the formula was correct, but their mathematical practices could not be more different.

A Meandering Theorem

As the disputed status of Chasles' formula indicates, the historical episode we propose to study is not that of the emergence of a mathematical theory, if by theory one understands a collectively-shared set of epistemic norms, objects, and methods. By the late 1870s, one can already see labels such as “abzählende Geometrie” or “abzählende Methode” appear in the pages of, for instance, the *Jahrbuch über die Fortschritte der Mathematik*. These sections grouped mathematical papers which all, in some way, purported to enumerate geometrical figures satisfying certain conditions. However, this differentiation of a discipline, centered around guiding research questions (or a “problem constellation”²²) does not mean that there was any agreement whatsoever about how these questions should be tackled. In fact, beneath the rather superficial commonality of the guiding research question, there lay many an idea of what form this question should really adopt – for instance, should one ask for tables of numbers enumerating figures satisfying lists of problems, or should one ask for a few synthetic formulae?

While a systematic study of the apparition and transformation of such disciplinary labels (especially at an international level) would be interesting, this is not what we set out to provide here. Instead, our goal in this dissertation is epistemological: we set out to understand generality in geometry, how it was conceived, written down, and embodied. To that end, we focus on one strand of the composite that are 19th century enumerative methods: the shaping and circulation of Chasles' theory of characteristics. This choice means that several authors will not be discussed here, who otherwise would have to appear in a general history of enumerative geometry. This is the case, for instance, of the German mathematician Rudolf Sturm. Similarly, we shall ignore large sections of the body of work of some of the authors we do discuss: thus, Zeuthen's work on the enumeration of algebraic curves of genus p will not be treated here, because it would take us too far from the theory of characteristics and the enumeration of conics.

Our strategy, in this dissertation, is to focus on Chasles' theory of characteristics, and in particular the disputes it gave rise to. Even there, we make no claim to exhaustiveness: the criticisms of some actors which were not widely discussed will only be mentioned in passing. Instead, our strategy has been to focus on a few key actors of these disputes, of whose interventions we can produce “thick descriptions”. This means that, across this variety of ways of construing and enacting generality in geometry, we aim to “expose their normalness without reducing their particularity”²³. For instance, Schubert's symbolic

²²“Disciplines can be defined by *guiding research questions* rather than by subject areas. [...] The emergence of a discipline may be perceived, then, as a sequence of tentative answers. Ultimately, the formulation of new problems at the disciplinary boundaries calls for the establishment of new subdisciplines or disciplines – hence the cumulative nature of the development of science”, [Stichweh, 1992], pp.7-8.

²³ [Geertz, 1973], p.14.

practice is in many ways foreign to us. The results it leads to has been a constant source of wonder for mathematicians²⁴. And yet, by locating it, in all its technicality, within a certain cultural composite, one can restore the kind of rationality it belongs to.

Our periodization begins with Chasles' teaching at the Faculté de Paris, and ends with the year 1893. Indeed, we shall argue that the genesis of the theory of characteristics must be read against the background of a specific research programme which Chasles first developed through his pedagogical activities (even though he had laid out the foundations for it at an earlier stage, in his 1837 *Aperçu Historique*). In 1893, Zeuthen wrote his last reply to Study regarding the validity of Chasles' formula (and the theory of characteristics at large). At this point, Halphen and Chasles had passed away, and Schubert and Study would move on to other scientific endeavours. More profoundly, the theory of characteristics was no longer the cornerstone of enumerative methods. In 1900, in his Paris address, Hilbert identified Schubert's calculus and its justification as the more pressing matter; and despite the lack of a satisfactory answer, the question of Chasles' formula simply faded away.

How to (dis)count conics

To understand what the problematic formula was, and why its validity hinges upon what is taken to constitute generality in enumerative geometry, a brief technical exposition is required²⁵.

A first, putative definition of enumerative geometry could be the search for systematic methods for the enumeration of geometrical figures satisfying a maximal number of conditions in a (projective complex) space. One simple example of such a problem is the enumeration of conic sections, in the plane, passing through five given points. In this example, conic sections serve as the kind of figure to be enumerated. The plane specifies the space in which this enumeration is to be carried out, thus determining how many conditions one can demand from the figure at hand. Indeed, a general equation of the conic section is, in the plane (and using homogeneous coordinates)²⁶:

$$aX^2 + bY^2 + cZ^2 + 2dXY + 2eYZ + 2fXZ = 0$$

This equation has six coefficients, one of which at least is not zero. Dividing the equation by one such non-zero coefficient, one obtains another equation defining the very same

²⁴“Schubert does not explicitly offer such a parameter space, although he goes on, nonetheless, to give formula after formula in what can only be its intersection theory!”, [Roberts & Speiser, 1981], p.273.

²⁵An accessible introduction to these questions, from a modern perspective, is given in [Brugallé, 2008].

²⁶Other equations, for instance using homogeneous coordinates, can be given f

curve; therefore, a conic is defined in general by five independent coefficients.

Supposing a condition is an algebraic equation in the five coefficients of this general equation, then five (independent) conditions will determine a finite number of solutions, which can be theoretically computed via elimination. For instance, given five points $p_1 = (x_1, y_1, z_1), \dots, p_5 = (x_5, y_5, z_5)$, then the condition ‘passing through p_i ’ translates into the algebraic equation

$$ax_i^2 + by_i^2 + cz_i^2 + 2dx_iy_i + 2ey_iz_i + 2fx_iz_i = 0$$

For a conic to pass through all five points, its coefficients (a, b, c, d, e, f) must therefore solve the system

$$\begin{bmatrix} x_1^2 & y_1^2 & z_1^2 & 2x_1y_1 & 2y_1z_1 & 2x_1z_1 \\ x_2^2 & y_2^2 & z_2^2 & 2x_2y_2 & 2y_2z_2 & 2x_2z_2 \\ x_3^2 & y_3^2 & z_3^2 & 2x_3y_3 & 2y_3z_3 & 2x_3z_3 \\ x_4^2 & y_4^2 & z_4^2 & 2x_4y_4 & 2y_4z_4 & 2x_4z_4 \\ x_5^2 & y_5^2 & z_5^2 & 2x_5y_5 & 2y_5z_5 & 2x_5z_5 \end{bmatrix} \times \begin{bmatrix} a \\ b \\ c \\ d \\ e \\ f \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}$$

The set of solutions of the equation is a one-dimensional vector space, any non-zero vector of which represents the same conic (since they are all proportional). Thus, there is one and only one curve which passes through all five given points, provided that these be independent²⁷. Of course, for conditions more complex than ‘passing through a given point’, this rudimentary algebraic framework is not effective: computations are untractable, and even to form conditions in the coefficients of the Cartesian equation of a figure is not always possible.

Beyond its computational cost, this first approach runs into another, more substantial problem. It takes conics to be only a locus of points in the plane; a view from which tangency between a conic and a given curve can only be translated by the fact that they have a single (double) intersection point. However, in so doing, one will count every degenerate conic whose (Cartesian) equation is

$$(aX + bY + c)^2 = 0$$

²⁷If four of the five points p_1, \dots, p_5 are aligned, then the equations in the coefficients formed by the condition ‘passing through p_i ’ will not form a system of rank 5, and therefore an infinity of conics will satisfy all five conditions. These are, of course, all the degenerate conics composed of two straight lines, one of which is that line defined by the four aligned points. If only three points are aligned, then there is only one solution, but it is also degenerate; see [Eiden, 2009], pp.52-53. To spell a precise criterion for the independence of conditions beyond this simple case is beyond the scope of this introduction.

to be tangent to any given curve. These degenerate conics are double-lines (that is to say two coinciding lines), and they intersect every given curve at one double intersection point. And yet, they cannot satisfactorily be said to satisfy the contact condition ‘to be tangent to the given curve’. Incidentally, Steiner’s enumeration of the conics touching five given conics did count such solutions, and this is why his result is usually rejected in favour of Chasles’.

Chasles’ theory of characteristics avoids such pitfalls by introducing a dual description of conics as both loci of points, and envelopes of tangent lines. Furthermore, it avoids untractable computations, by means of a characterization of systems of conics via only two numbers. A system of conics (Z_1, Z_2, Z_3, Z_4) is defined as the collection of conics in the plane which satisfy four conditions Z_i . For any given system, Chasles defined two numbers μ, ν as the numbers of conics in the system respectively passing through a given point P , touching a given straight line L . That these two numbers do not depend on the choice for P and L is already a sign of the kind of generality that Chasles demands from his propositions: in modern mathematical parlance, this would require that one works in a complex, projective plane (or space). Chasles calls these numbers the “characteristics” of the system, and he writes $(Z_1, Z_2, Z_3, Z_4) \equiv (\mu, \nu)$. The justification for this terminology (and notation) is that Chasles had found a method for the systematic enumeration of conics in a system of characteristics (μ, ν) satisfying any fifth condition Z_5 . For several hundreds of examples of such a condition Z_5 , he had found this number to be of the form

$$\alpha\mu + \beta\nu$$

where α, β are integers depending only on Z_5 . Furthermore, provided that one has been able to compute the integers α_i, β_i relative to five given conditions Z_i , Chasles had given a procedure for the enumeration of the conics satisfying all five conditions as a function of these $2 \times 5 = 10$ integers.

The $\alpha\mu + \beta\nu$ formula captured the attention of many after Chasles’ initial salvo of publications. Supposing it to be true was not logically necessary to carry out his enumeration procedure; and, for Chasles, it was merely a formal regularity observed across the variety of conditions he had studied. For others, however, this observation would turn into a theorem in its own right. This theorem could be restated in a variety of ways, for instance as saying that the intersection of the collections of conics defined by respectively one and four conditions is given by a Bézout-like formula, that is to say as a simple algebraic combination of the numbers characterizing both collections (just as the number of intersections of two algebraic curves is given by the product of their degrees).

It turns out, as Halphen and others would find out, that this theorem can be criticized on the same grounds as Steiner’s enumeration of the 7776 conics touching five other

given conics. In other words, from the $\alpha\mu + \beta\nu$ conics enumerated by the theory of characteristics, some can be discounted on the grounds that they also are degenerate conics which do not satisfy the conditions at hand in a satisfactory manner. It was neither possible, nor desirable, for geometers of this period to discard degenerate conics altogether²⁸. Through a certain rewriting of what it means to satisfy a condition, and to be a degenerate conic within a system of conics, Halphen had established new standards for the geometrical significance of enumerative results, which Chasles' formula did not meet.

On the historiography of enumerative geometry

Enumerative geometry in the 19th century has been subject of scant attention by historians of mathematics in recent decades. In most general histories of mathematics, it is either dealt with in a few lines, or, more often than not, entirely absent²⁹. When it is mentioned, it is either in association with Chasles³⁰, or because of Hilbert's 1900 Paris address. In this well-known speech, Hilbert gave a list of 23 mathematical problems which he believed to be of great importance for the century to come. Hilbert's fifteenth problem consisted in the search for rigorous foundations for Schubert's *Kalkül*, which had already become the most visible representative of enumerative geometry³¹. Even in books focusing on the history of mathematics in the nineteenth-century, enumerative geometry is almost never mentioned³². In books devoted to the history of nineteenth-century geometry, the situation is only marginally better. There as well, enumerative geometry is often outright left out³³, or only mentioned in passing, as one of the research interests of Chasles' (an actor who is systematically mentioned, if rarely studied in his own right), with perhaps a nod to Schubert's later works³⁴.

By contrast, enumerative geometry features much more prominently in the writings of mathematicians (and especially, algebraic geometers) reflecting upon their own discipline. Julian Coolidge, who studied under Eduard Study (one of the important actors of the debates on the validity of Chasles' formula), wrote a *History of Geometrical Methods* in which an entire chapter is devoted to enumerative methods³⁵. Jean Dieudonné, in the first

²⁸In recent years, real enumerative geometry, wherein the benefits of working with an algebraically closed field are lost, has been the subject of important results; see in particular [Welschinger, 2005]. These results are far out of the scope of the present dissertation.

²⁹Such examples include [Kline, 1972], [Dahan-Dalmedico & Peiffer, 1986], [Cooke, 2005], [Katz, 2009].

³⁰See [Struik, 1948], pp.250-251.

³¹For instance, [Struik, 1948], p.285.

³²[Mehrtens *et al.*, 1981], [Gray, 2008]

³³[Nabonnand, 2006], [Bioesmat-Martagon, 2015].

³⁴This is the case in [Gray, 2007], p.68, for instance.

³⁵[Coolidge, 1940], pp.180-194.

volume of his course on algebraic geometry, gave a brief presentation of Chasles' theory of characteristics in the section on projective geometry, and discussed Schubert's enumerative geometry briefly in the chapter on algebraic structures³⁶. Interestingly enough, the titles of both books contain elements coming from the title of Chasles' 1837 *Aperçu Historique*. But the most sustained and thorough investigation of the history of enumerative methods has been that conducted by the American algebraic geometer Steven Kleiman. Kleiman contributed largely to enumerative geometry in the 1970s and 1980s, after the rise of intersection theory and Grothendieck's work in algebraic geometry. He published several papers on the history of enumerative geometry³⁷, and edited in 1979 a re-print version of Schubert's 1879 *Kalkül der abzählenden Geometrie*, writing a preface and adding in a list of Schubert's publications compiled by the German mathematician Werner Burau³⁸. Moreover, in his own mathematical works, Kleiman intertwined technical developments with historical discussions³⁹. In so doing, he shaped a specific format for his research papers which seems to have been adopted by others in the field (especially in American institutions with which he had frequent interactions⁴⁰). Of course, in these very useful books and papers produced by mathematicians, no effort is made to restore the notations, goals, or concepts of enumerative geometry as it was historically practiced. Instead, sense is made of past texts by means of contemporary mathematics. Thus, the history of enumerative geometry as scientific and textual practice remains by and large unwritten.

This brief survey is not to be read as a lament: after all, there is no reason a priori to expect enumerative geometry to be more relevant than any of the material included in these aforementioned books. Rather, this survey is an invitation to reflect on the reasons for this widespread lack of interest. One such reason seems to be that enumerative geometry sits uncomfortably within most of the narratives which typically dominate the historiography of nineteenth-century mathematics. For instance, enumerative geometry was – loosely speaking – part of projective geometry for several of the actors involved in this historical episode. Indeed, for the methods and concepts of enumerative geometry to make sense, one must accept points and lines at infinity to form legitimate solutions – otherwise, one should reject statements such as ‘there is one and only one point which lies on two given straight lines’, a fundamental proposition without which not much can be done in the way of geometrical enumerations. However, viewed as a part of the history of

³⁶ [Dieudonné, 1974], pp.39-41 and 129-132 respectively.

³⁷ [Kleiman, 1976a], [Kleiman, 1976b], [Kleiman, 1991].

³⁸ [Schubert, 1979].

³⁹For instance, [Fulton *et al.*, 1983].

⁴⁰For instance, see [Roberts & Speiser, 1981], [Aluffi, 1988]. Some of these historical discussions seem to be motivated by the notion that these geometers were proving the results obtained by unrigorous methods about a century prior, especially in the context of Schubert's calculus. A broader study of this collective endeavour, at the junction of algebraic geometry and historical reflections, would be very interesting.

projective geometry, enumerative geometry is not of particular interest. In fact, by viewing enumerative geometry as a remote province of projective geometry, some have read Schubert's principle of conservation of numbers as a particular case of Poncelet's principle of continuity, thereby obfuscating the specificity of Schubert's symbolic approach and his proto-formalist philosophy of numbers⁴¹. Similarly, while the history of enumerative geometry intersects that of algebraic geometry (for instance because it is where the principle of correspondence⁴² was first developed), the theory of characteristics was first constructed by mathematicians who wanted nothing to do with algebraic methods. Only through a radical recasting of Chasles' theory could one fit it in a history of algebraic geometry. In historical discourses focused on an object and not a discipline, such as histories of space or of axioms, enumerative geometry is a marginal item, much less salient than other domains of mathematical inquiry of the same period. The fact that Chasles' theory of characteristics was subject to many rewritings, thereby being pulled toward a variety of other mathematical theories, is a key factor of this uneasy state of enumerative geometry with regard to the historiographical canon.

However, precisely because it sits at the intersection of so many contemporary developments, enumerative geometry is particularly interesting in order to study the transformation of mathematics in the second half of the 19th century as a cultural and scientific practice. In this dissertation, we will locate the emergence of the theory of characteristics against the backdrop of the defense and renewal of pure geometry proposed by Chasles and De Jonquières. In so doing, we will add to recent analyses on the distinction between synthetic and analytic geometries⁴³. Furthermore, by discussing the algebraic translations undergone by this theory in the hands of Clebsch, Lindemann, and Halphen, we shall make tangential contacts with the history of algebraic geometry, and in particular of the theory of invariants⁴⁴. Lastly, by situating Schubert and Study within the broader context of the transformation of the cultural status of mathematics at the turn of the century, we shall highlight the relevance of enumerative geometry for the history of mathematical modernism⁴⁵.

⁴¹See for instance [Zeuthen, 1905], p.270.

⁴²On the principle of correspondence, see 3.1.3, 4.2.2.

⁴³[Gray, 2007], [Nabonnand, 2011], [Lorenat, 2016]. Chasles, while often relegated to a second role in these narratives, has been the subject of growing interest in recent years, see in particular [Chemla, 2016], [Smadja, 2016]. These studies focus mostly on his historiographical work. Of his later, mathematical work, not much has been studied in detail.

⁴⁴[Gray, 1989], [Lê, 2015]

⁴⁵[Mehrtens, 1990], [Epple, 1997], [Gray, 2008], [Corry, Forthcoming].

Chapter Outline

The first third of this dissertation focuses on Chasles' geometrical practice prior to his involvement with enumerative questions. More precisely, the first chapter presents Chasles' historiography and epistemology of geometry relying on his early work (and, in particular, on his famous 1837 book on the history of geometrical methods). The second chapter then turns to Chasles' course of higher geometry given between 1846 and 1852 at the Faculté de Paris. Special attention is paid to the notations developed therein, and their use for the writing of abstract geometrical propositions. Lastly, chapter three shows how these notations (and, more largely, the content of this course) were mobilized between 1853 and 1860 to form the basis of a literary technology Chasles would call “geometrical equations”. Alongside the naval-officer Ernest de Jonquières, who had attended some of the aforementioned lectures, Chasles would reactivate the early-modern tradition of equation-solving within his new notational and conceptual framework, in a way that allows for the writing of general equations of curves, devoid of auxiliary quantities such as algebraic variables or axes of coordinates.

The second part of this dissertation deals with Chasles' and De Jonquières' enumerative theories, which they developed between 1861 and 1867 after publications by Steiner and Bischoff had circulated to France and Italy. These enumerative theories, this dissertation argues, must be read as the introduction of these early enumerative efforts into the new framework of geometrical equations. Chapter four thus contains a description of the circulation of enumerative problems from Steiner to Chasles, as well as a presentation of Chasles' theory of characteristics in its technical details. In chapter 5, we turn to the controversy which ensued thereafter between Chasles and De Jonquières. While their controversy began as a regular priority quarrel, it soon turned into a full-fledged scientific disagreement bearing on the epistemic differences between algebra and geometry. The notion of “solutions étrangères”, which had arisen toward the end of the 18th century as part of a reflection on the modes of generality of algebraic and geometrical propositions, would be reactivated to describe certain degenerate solutions to enumerative problems.

Lastly, the third and final part of this dissertation is devoted to the transformative circulation of Chasles' theory of characteristics between 1865 and 1893. More precisely, in chapter 6, we survey early rewritings of this theory, and in particular algebraic rewritings thereof. We show how one of these rewritings, namely that of the French artillery officer Georges Halphen, led to a refutation of the $\alpha\mu + \beta\nu$ formula at the heart of Chasles' theory. In chapter 7, we present the genesis of Schubert's *abzählende Geometrie* between 1874 and 1880, with a special emphasis on the notational innovations developed therein. Much of the notations and architecture of Schubert's theory is predicated on the validity of Chasles' formula (and other analogous results). The final and eighth chapter of this dissertation

surveys the debates which arose from the obvious contradiction between Halphen's and Schubert's enumerative theories. These debates are then framed as part of a broader cultural phenomenon, namely the modernist transformation of mathematics.

Chasles' *Aperçu Historique*: The Epistemic Portrait of the Geometer

Introduction

Toward the end of his landmark 1837 *Aperçu historique sur l'origine et le développement des méthodes en Géométrie* (to which we shall refer only as *Aperçu Historique* in what follows), the French mathematician Michel Chasles (1793-1880) expressed an optimistic outlook on the state of and perspectives for pure geometry¹:

Aujourd'hui, chacun peut se présenter, prendre une vérité quelconque connue, et la soumettre aux divers principes généraux de transformation; il en retirera d'autres vérités, différentes ou plus générales ; et celles-ci seront susceptibles de pareilles opérations ; de sorte qu'on pourra multiplier, presque à l'infini, le nombre des vérités nouvelles déduites de la première. [...] Peut donc qui voudra, dans l'état actuel de la science, généraliser et créer en Géométrie ; le génie n'est plus indispensable pour ajouter une pierre à l'édifice.

The *Aperçu Historique* presents itself as a historical account of the development of geometrical methods, largely polarized by the rivalry between pure geometry and analytical geometry². This rivalry, in Chasles' narrative, mostly revolves around the relative generality of both approaches. The conclusion of Chasles' historical account was that at

¹"Today, anyone can come up, pick any known truth, and submit it to the various general principles of transformation; they will thus gain other truths, different or more general; and the latter will be susceptible to similar operations; so that one will be able to multiply, almost to infinity, the number of new truths deduced from the first one. [...] In the present state of science, whoever wants to generalize and create in Geometry, can; genius is no longer required to add a brick to the edifice", [Chasles, 1837], pp.268-269. All translations in this dissertation are mine, unless otherwise noted.

²Analytical methods, here, refer in first approximation to methods relying on axes of coordinates, algebraic or infinitesimal computations etc. Chasles sometimes refers to pure geometry as synthesis, or synthetic geometry. However, he overall rejects this label, for reasons which we shall explain later.

the turn of the century, savants such as Gaspard Monge (1746-1818) and Lazare Carnot (1753-1823) had laid out the foundations for a renewal of the methods of pure geometry, thereby allowing the latter to finally regain the means to rival the analytical methods which had dominated mathematics in the wake of Descartes' 1637 *Géométrie*³. Moreover, beyond catching up with the generality of analytical methods, Chasles depicted pure geometry as retaining a series of epistemic advantages, in that the knowledge it provides is particularly simple and intuitive.

Geometrical methods, Chasles concluded at the end of his historical analysis, can be used effortlessly, and by anyone. This contrasts starkly with Chasles' assessment of analytical methods which, while also general, require shrewdness, talent, or subtleness⁴. It might sometimes be the case that analysis yields geometrical truths at a faster rate than geometrical methods could (although Chasles never fully admits this possibility), but the latter would still be valuable for it illuminates the problem at hand so fully that it provides a knowledge of a different nature. The sign of a complete knowledge of a certain theory, Chasles thus explained, was that one could explain it entirely to any passer-by on the street⁵. Thus, generality, simplicity, and intuitiveness are to be understood as both epistemic and epistemological values for Chasles⁶. This means that they serve both as norms of scientific practice conducive to truth – by following general methods, one can transform a proposition into a more general one, thereby adding to our knowledge – but also to truth *in a desirable form*.

This constellation of values, thus, allows Chasles to construct two opposite portraits of the analyst and of the geometer: the ingenious and swift *calculateur*, expert producer and wielder of a cryptic knowledge, *versus* the enlightened geometer, effortlessly and methodically combining truths, able to pause at any given time to explain to the layman in the street whatever proposition he is contemplating. Of course, these two portraits are not pure creations of Chasles': they reflect the preoccupations of a specific scientific *milieu*. In particular, both Chasles' recourse to historical epistemology, and his insistence on the importance of general, transmittable knowledge, are key components of a scientific culture shaped and shared at the École Polytechnique.

³In this chapter, we shall not attempt to discuss the validity or merits of Chasles' historical account per se, as our interests lie rather in how it played a role in his mathematical practice.

⁴For instance, see Chasles' comparison of various approaches to spherical geometry which concludes with "la Géométrie pure offre souvent une voie plus facile et plus expéditive que le calcul le plus ingénieux et le plus subtil", [Chasles, 1837], p.238. Many other passages in the book echo this thesis.

⁵"Nous ajouterons, avec l'un des géomètres modernes qui ont le plus médité sur la philosophie des mathématiques, 'qu'on ne peut se flatter d'avoir le dernier mot d'une théorie, tant qu'on ne peut pas l'expliquer en peu de paroles à un passant dans la rue'", [Chasles, 1837], p.115. Chasles is in fact quoting Gergonne through the intermediary of Quêtelet here. On the circulation of this quote from Gergonne to Chasles, and later on to Hilbert, see [Barrow-Green & Siegmund-Schultze, 2016].

⁶We borrow this distinction from [Chemla *et al.*, 2016], pp.8-9.

In this chapter, we locate Chasles in the institutional and scientific context of this school, and analyze the portrait of the ideal geometer which he constructed in his *Aperçu Historique*. In the two following chapters, we shall turn to the manners in which this normative ideal structured Chasles' mathematical practice.

1.1 Situating Michel Chasles

1.1.1 Some biographical elements

Chasles was born in 1793, the son of a wealthy and well-established merchant near Chartres. At the peak of Republican fervor, he was given the name Floréal after a month of the Revolutionary calendar. By 1809, this attempt at decimalizing dates had largely failed, and he had his name changed to Michel. At about the same time, Chasles was sent to Paris to study at the Lycée Impérial (today's Lycée Louis-le-Grand), where he excelled. In 1812, he was admitted to the École Polytechnique; and two years later, an Imperial decree mobilized the students of this school in the context of the War of the Sixth Coalition. Chasles and his classmates took part in the Battle of Paris in 1814, after which they were sent home and the school closed down for several weeks. The military



Michel Chasles (1793-1880)

defeat, and the ensuing economic losses, meant that many of Chasles' cohort were not able to obtain the positions they had hoped for upon entering the École Polytechnique. Chasles, according to a few biographers, gave up on such a position so that one of his less wealthy friends could have it, and instead went on to work as a stockbroker in Paris, and then in various European cities⁷.

⁷A few biographies of Chasles were published toward the end of his life and in the years following his death, see in particular [Boudin, 1869], [Bertrand, 1892].

Chasles had published a few papers on geometry while a student at the École Polytechnique, but he would not publicly participate in any scientific endeavour between 1814 and 1827. Very little is known of Chasles' life during this period, except for the fact that he was rather unfortunate in his financial dealings, and, by the late 1820s, had to return to Chartres and to mathematics. In 1827, his name begins appearing in mathematical journals such as the *Nouvelles Annales*; and his publications mostly deal with the geometry of conics, borrowing from the work of other pure geometers such as Hachette and Poncelet.

Chasles' rise to fame came with his winning entry for a concours organized in 1829 by the Académie de Bruxelles. Candidates were asked to present the methods of modern geometry, and in particular that of reciprocal polars. Chasles' entry became the basis for a book, which would only be published in 1837 partly due to the Belgian Revolution between 1830-1831. We shall turn to the content of this book in the second section of this chapter (1.2). Throughout the 1830s, Chasles also published a few memoirs on the attraction of ellipsoids, which were well received and would circulate widely⁸.

In 1841, after the death of Félix Savary, Chasles was hired to teach the 'Cours de Machines' at the École Polytechnique. Whilst Chasles did not relish the prospect of teaching this subject, nor that of moving back to Paris at the age of almost fifty, he was nonetheless convinced to do it. This move proved judicious, as Chasles managed to navigate Parisian scientific institutions extremely well. By 1846, after some lobbying by Poinot, a chair of higher geometry was created specially for Chasles at the Faculté de Paris (i.e. the Sorbonne), which he then occupied until his death in 1880 (although after 1868, his teaching duties were increasingly covered by Pierre-Ossian Bonnet, then by Gaston Darboux⁹). Furthermore, while Chasles had been a corresponding member of the Académie des Sciences since 1839, he would become a full member only in 1851.

Chasles' scientific output accelerated once he moved back to Paris, and even more once he obtained his tenure at the Faculté de Paris¹⁰. In particular, he would become a regular speaker during the weekly meetings of the Académie des Sciences; and the *Comptes-Rendus de l'Académie des Sciences* would quickly become Chasles' favored outlet for scientific publications¹¹. In fact, most of Chasles' communications to the Paris Académie would borrow from the content of his lectures given the same year at the Faculté de Paris.

⁸ [Michel, 2020]. At several occasions in this chapter, we shall use this case-study as an example of Chasles' historiographical and epistemological theses; the reader can report to this paper for more details each time.

⁹ [Croizat, 2016], p.107.

¹⁰ A partial list of Chasles' publications can be found in the inventory of his personal library, see [Boulland & Claudin, 1881], pp.173-179.

¹¹ In this regard, Chasles was no different from the majority of his peers from the Académie. We shall discuss Chasles' publishing strategies in 5.1.3.

Chasles' lectures were designed for advanced students, and the material he lectured on was very much his own research. These lectures would form the basis of two treatises: in 1852, Chasles published a *Traité de Géométrie Supérieure* (which will be the focus of chapter 2); and in 1865, he published a *Traité des Sections Coniques*. This latter treatise was initially intended to be the first of two volumes. The second volume, of which no trace exists, was to focus on the enumerative methods which we shall discuss in chapter 4, according to contemporary accounts. That Chasles' teaching must be viewed as an integral part of his scientific activity (and not merely as a transmission of knowledge) will be key to our analysis of his notational and textual practices, which were largely created in a pedagogical context.

Chasles' research interests were diverse: they encompass for instance, the history of mathematics (including from non-Western traditions), kinematics, and of course the geometry of curves and surfaces. However, a constant theme across his scientific output is the defense of pure geometry, whether to investigate motions or curves, or its historical development. For this reason, descriptions of Chasles' work have often placed him as part of a larger French tradition of synthetic geometry, to which we now turn.

1.1.2 A French tradition of pure geometry?

Much has been written on the renewal of pure (or synthetic) Geometry in early 19th century France. This historical episode is one which took place during the two or three first decades of the nineteenth-century, and it saw an effort from various geometers to broaden the scope of geometrical methods by introducing concepts such as transformations and projections, often in order to enable geometry to compete with analysis. The main actors of this historical narrative are typically considered to be Gaspard Monge and Lazare Carnot, but also Charles Dupin (1784-1873), Jean-Victor Poncelet (1788-1867), Charles Brianchon (1783-1864), and of course Chasles. In first approximation, we shall refer to this collective as the “French tradition of pure geometry”, a label whose relevance we shall discuss shortly.

Already at the end of the nineteenth century, mythical retellings would present this historical episode as the birth of a new branch of mathematics, namely projective geometry. A key text responsible for this historiographical narrative is Klein's famous *Erlanger Programm*¹². This French tradition, Klein suggested, “provided a sound foundation for that distinction between properties of position and metrical properties¹³”. By subordinating the study of metrical properties (such as lengths and areas of figures) to the study of

¹² [Klein, 1872]. This text was first delivered as a speech, then published in various languages over the subsequent years. On the difference between these various versions, see [Rowe, 1983]. On the moderate influence of this text in its first years of circulation, see [Hawkins, 1984].

¹³ [Klein, 1872], p.12. Translation by M.W.Haskell, in [Haskell, 1893], p.222.

properties which are not altered by projection (such as the fact that two figures intersect, or are in certain specific configurations), Poncelet, Chasles, and their colleagues allegedly discovered a new geometry, which would later be made entirely free from metrical concepts (such as lengths or angles) by German geometers such as Von Staudt. Klein's historical account served to underscore the profound unity underlying the various, divided geometries which had arisen over the past decades¹⁴: for him, the divide between analytic and synthetic geometries had been bridged through a redefinition of the very subject-matter of geometry. This reading has proven influential: for instance, the American philosopher Ernest Nagel borrowed largely from it in his account of the development of 19th-century geometry as the progressive introduction of a non-quantitative subject-matter for geometry¹⁵. However, recent scholarship has definitively put the lie to this reading, and revealed it as decidedly anachronistic. None of the authors of this French tradition suggested an exclusion of metric properties from geometry¹⁶. Furthermore, their criticism of analysis did not prevent them from relying more or less frequently on "mixed methods", that is to say on a combination of the analytical and the purely geometrical¹⁷.

More recently, this tradition of French synthetic geometry has been the subject of historical attention from a variety of perspectives. Some have proposed accounts of the mathematical innovations – be it in terms of concepts, methods, or results – at the heart of this episode, emphasizing the role of descriptive geometry, the study of projections, the principle of duality, and Poncelet's theory of poles and polars¹⁸. Others have put this renewal of geometrical methods in perspective with the emergence of new teaching institutions¹⁹, such as the École Polytechnique or the École de l'an III. These institutions, where Monge's teaching was influential for years even after his dismissal, were places for the circulation not merely of his geometrical knowledge, but also for a certain way of understanding and practicing science. For instance, the older members of this French geometrical tradition have been described as enacting practices of generality more largely shaped and shared at the École Polytechnique²⁰.

Our purpose in the following pages is not to give a comprehensive overview of this

¹⁴For a more nuanced presentation of Klein's thesis and its evolution over time, see [Lorenat, 2015a], pp.32-37.

¹⁵ [Nagel, 1939].

¹⁶ [Chemla, 2016], p.66.

¹⁷ [Lorenat, 2015a], p.284.

¹⁸ [Gray, 2007], pp.1-78; [Nabonnand, 2006], pp.11-78; [Nabonnand, 2011].

¹⁹ [Daston, 1986], [Belhoste & Taton, 1992], [Belhoste, 1998]. Daston emphasizes the philosophical continuities between the empiricism of Locke and D'Alembert, and the epistemology of geometry passed down by Monge in his teaching for engineers. Belhoste emphasizes the transformation of the mathematical practice (and understanding thereof) of actors such as Lagrange upon receiving teaching duties, and having to adapt their mathematical knowledge for the transmission to a non-expert audience.

²⁰ [Chemla, 1998], [Chemla, 2016]. On the Polytechnique ideal for generality in mathematics more broadly, see [Wang, 2017], pp.118-122.

tradition, or even of its historiography. Instead, we wish to identify some of its epistemic characteristics, in order to measure Chasles' adherence to or departure from them. Before we do that, however, we must first ponder how this collective can even be delineated.

From a mere glance at their dates of birth and death, the actors we have listed above can already be divided into at least two distinct generations. Monge and Carnot were both savants of the Ancien Régime, who witnessed (and in fact, actively participated in) the French Revolution as adults. Monge had worked as an instructor at the École de Mézières, teaching experimental physics and technical drawing to military engineers-to-be²¹. Carnot actually trained at the École de Mézières while Monge worked there, graduating in 1773 to become a lieutenant in the French army, where he continued his study of (applied) mathematics²². After the Revolution, Carnot served as a general under Napoleon Bonaparte, and later served as Minister of the Interior. Monge, on the other hand, played a key role in the creation of new pedagogical institutions. In particular, he was an influential member of the Committee which presided over the creation and definition of the École Polytechnique, where he also served as a professor of descriptive geometry for over a decade²³.

On the other hand, Dupin, Brianchon, Poncelet, and Chasles were born either a few years before or after the Révolution. They all trained at the École Polytechnique (being admitted in, respectively, 1801, 1803, 1807, and 1812); however, only the first two were able to attend Monge's lectures. Indeed, during his first year at the École Polytechnique in 1807, Poncelet was taught by Hachette²⁴, as Monge only taught second-year students. In 1808, Poncelet became ill, and had to repeat the first year. The following year, Monge himself would cease his teaching activities due to an illness of his own²⁵. This means that another division must be introduced between the members of this tradition. Indeed, while Dupin and Brianchon would strongly identify as disciples of Monge²⁶, this would not exactly be the case for the other two. The introduction to Poncelet's 1822 *Traité des Propriétés Projectives*, for instance, opens on an evocation of Monge's lessons, and identifies "Monge's disciples" as a collective with which Poncelet maintains a certain

²¹On engineer schools in Ancien Régime France, and especially artillery schools, see [Alder, 1997], pp.56-85. On Monge's scientific career, see [Taton, 1951]. On the global circulation of Monge's teaching, see [Barbin et al., 2019].

²²On Carnot's political and scientific life, see [Gillispie, 1971].

²³ [Belhoste, 2003], pp.76-91. Descriptive geometry is the art of representing three-dimensional figures in only two dimensions.

²⁴Jean Hachette (1769-1834) had worked at the École de Mézières in the months before the French Revolution, and had become Monge's assistant at the École Polytechnique at its creation in 1794. He would eventually teach descriptive geometry at the Faculté de Paris and at the École Normale.

²⁵ [Belhoste, 1998], pp.2-3. Monge's influence at the École Polytechnique weaned off quickly in the aftermath of his disease and withdrawal from teaching activities, see [Belhoste, 2003], pp.200-211.

²⁶See for instance the dedication of Dupin's *Développements de Géométrie* to Monge, [Dupin, 1813], pp.v-vi.

distance²⁷. While the works produced by Monge and his students are an important starting point for the renewal of pure geometry, they remain insufficient in that regard. Poncelet does not present his treatise as a continuation, but rather as a departure from them²⁸ and about the same thing can be said of Chasles' *Aperçu Historique*²⁹.

In a well-known paper on the so-called “Chicago school of algebra”, the historian Karen H. Parshall surveyed a few definitions for the historiographical category of a ‘research school’ in general, and eventually proposed one with the specific case of mathematics in mind³⁰. A collective of actors, she suggests, can be said to form a school if it’s centered around a charismatic leader, who advocates a fundamental idea which he passes along to students he trains more or less closely³¹. This original leader, Parshall explains, can be replaced by a successor who is recognized as such by other members of the school. What is passed down through teaching is of course not just mathematical knowledge, but a selection of questions, as well as a certain ideal for what form the answers to these questions ought to take.

With this definition in mind, can one speak of a Monge school of geometry? Monge was certainly a charismatic figure, whose aura subsisted for years after he had had to abandon his teaching duties at the École Polytechnique. He trained many students there, and was replaced by Hachette, who by all accounts was his faithful successor. Furthermore, a broad consensus that geometrical methods must be renewed in order to elevate them to the heights achieved by analytical methods over the course of the past century was shared amongst these mathematicians, alongside a group of epistemic virtues such as facility, intuitiveness, and generality. The remaining question is whether Monge’s fundamental questions were really pursued by these students.

In his course on descriptive geometry given at the École Normale de l’an III, Monge put this mathematical discipline at the center of a broader republican project: to educate the nation, and thereby to boost national industry, such was the purpose of this course in the newly-created school³². The fundamental objective of descriptive geometry, therefore, was to provide engineers and workers with systematic methods to represent on the page spatial objects and their relative positions. What was at stake in this new science was at least two-fold: first, it had to be a representational technique accessible to a large number of citizens (and not only to trained mathematicians), hence the recourse to non-analytical

²⁷“Les ouvrages mêmes de MONGE, ceux de ses Élèves [...] ont prouvé que la Géométrie descriptive [...] peut se suffire à elle-même. [...] Cependant, il reste encore quelque chose à faire”, [Poncelet, 1865], p.ix.

²⁸ [Poncelet, 1865], p.ix.

²⁹ [Chasles, 1837], p.191.

³⁰ [Parshall, 2004].

³¹A fourth criterion regarding publications is included in her definition, which is omitted here because it is not entirely suitable for the description of early 19th-century scientific practices.

³² [Monge, 1799], pp.1-4.

methods, which Monge was very comfortable using in other contexts³³. Second, it had to allow engineers to exert control at a distance over the design and production of technical artifacts: as such, descriptive geometry quickly became a “language of command³⁴”, whose mastery granted greater social mobility and autonomy within the French industry. Monge’s understanding of the need for a purely geometrical approach to certain theoretical problems, and his insistence on the generality and simplicity of this approach, was therefore enshrined in a larger, extra-mathematical project.

Some of Monge’s students shared this vision: in his 1813 *Développements de Géométrie*, Dupin set out to show how descriptive geometry could be used to investigate the curvature of surfaces, and thereby be applied to practical matters such as the stability of ships or optics³⁵. However, this concern would disappear with Chasles. While he would occasionally mention such applications, with which he was clearly familiar as a ‘polytechnicien’, a large portion of Chasles’ scientific output lies in the realm of pure mathematics. This departure from Monge’s concerns corresponds to a shift in professional careers: unlike Dupin and Brianchon, Chasles eventually obtained a position as a professor of higher geometry at the Faculté de Paris, and did not pursue a career at the crossroads of engineering, military duties, and administration, as most of the other members of this collective did.

Thus, after savants such as Monge and Carnot, a set of epistemic virtues and epistemological concerns, shaped against the backdrop of late-Enlightenment engineering culture, would serve as identifiers for a collective of geometers all throughout the first half of the 19th century³⁶. However, their signification evolved with the sociology of their bearers. Thus, if one wishes to speak of a “Monge school of geometry”, Chasles and Poncelet can only be described as members of it in a weak sense. Furthermore, the bulk of Chasles’ scientific output was produced decades after the main texts around which Monge’s school was structured. Thus, an ideal for generality forged in the context of Republican science was transmitted to and transformed by Chasles, in the context of pure mathematics.

1.1.3 Generality between universality and uniformity

The transmission of this ideal can be located within the École Polytechnique. This school was where Chasles, like Poncelet, encountered Monge’s teaching through the intermediary of Hachette. Not only did he learn descriptive geometry there, but he was also in contact with a normative ideal regarding the form which scientific knowledge ought to take, one

³³Monge wrote a book on analytical geometry, which would be republished several times throughout the 19th century; [Monge, 1807].

³⁴ [Alder, 1997], p.74.

³⁵ [Dupin, 1813], pp.vii-xii.

³⁶On the importance of such identifiers for mathematical collectives in the larger context of synthetic geometry, see [Lorenat, 2015a], p.7.

which Monge had largely contributed to.

Indeed, the knowledge dispensed at the École Polytechnique was geared toward a specific aim, and as such, was structured according to a specific set of epistemic norms³⁷:

The École Polytechnique confronted its creators with a unique historical challenge. How were they to train engineers to build fortifications, bridges and roads, and to turn out mining and shipbuilding engineers, geographer engineers and all types of engineers capable of working effectively in all technical domains in the space of just three years? In order to handle such a daunting task and to mould the figure of the engineer-*polytechnique* in such a short time, there was only one real solution as far as the founders were concerned: to provide future engineers with **universal scientific knowledge, tools and methods**, which due to their being as **universal** as possible, were the only **principles** applicable to an (infinite) range of practical circumstances.

Descriptive geometry was one such method. One can see why generality was a cardinal epistemic value for the first generations of polytechniciens: the social and political status of the knowledge they were to master and employ throughout their careers ought to be universally applicable.

But the kind of generality sought for mathematical knowledge was not limited to universality: rather, its universality had to be readily available to practitioners all over the country, in a diverse array of industries and administrations. As such, the knowledge of this technocratic class had to be applicable systematically, through the use of rational linguistic rules. This is why analysis became such a crucial part of the Polytechnique curriculum: mathematical analysis was more than the namesake for a set of tools and methods, such as differential equations or infinite series, which *polytechniciens* learnt how to use and apply to a variety of physical and mathematical problems, ranging from mechanics to the theory of heat. More broadly, analysis referred, after Condillac, to a general method which, in the face of a given theory, “was supposed to determine the creation of a uniform and natural system of signs, [so that] the rules of their combination correspond to the rules of combination of the elements the signs refer to³⁸”. Similarly, in the context of early 19th-century engineering, descriptive geometry and technical drawing thus became more than “neutral conduits of information”, but rather a “vernacular of industry”, in keeping with “the central tenet of the Enlightenment [...] that progress in the mechanical arts depended on finding a uniform and precise language for that subject³⁹”.

While Chasles or Poncelet had their own reasons for rejecting, or at least criticizing,

³⁷ [Belhoste & Chatzis, 2007], p.217. Emphasis mine.

³⁸ [Belhoste, 2003], pp.232-235.

³⁹ [Alder, 1997], p.316.

the mathematical methods attached to the term analysis, they shared to a large extent a respect for the general scientific practice that had come to be understood under it - in other words, they were part of that same epistemic culture, to which belong many of the most vocal early proponents of mathematical analysis, such as Lagrange, Laplace, or Fourier⁴⁰. Chasles' plea for geometrical methods which can be applied "without genius" responds to this analytical ideal, whilst deplacing it onto the terrain of pure geometry. Similarly, Poncelet would call for the renewal of the language of pure geometry, which he assessed must be brought closer to that of analysis for general and uniform geometrical reasoning to be possible⁴¹.

Thus, Chasles' epistemology and practice of geometry must be viewed against the backdrop of an epistemic value of generality between universality and uniformity, and which originated in late-Enlightenment engineer culture. We now turn to his first book on the history of geometry, which we read as an epistemic portrait of the ideal geometer; and we flesh out the notion of generality in geometry which he had come to place at the center of this portrait.

1.2 The virtuous practice of geometry in Chasles' *Aperçu Historique*

1.2.1 History as a means for an epistemological end

In 1829, the Académie Royale des Sciences de Belgique proposed a prize for the best essay on the topic of "the philosophical examination of various geometrical methods used in recent geometry, and, in particular, of the method of reciprocal polars". Chasles' winning entry was to be immediately published, but some political turmoil caused by the Belgian Revolution put this project to a temporary halt⁴². As he was finally allowed to send a manuscript to press in 1837, Chasles had more than doubled the size of his dissertation. On top of the two memoirs on the principles of homography and duality which the Belgian Academy had rewarded, he had added a detailed historical study of the development of geometry, from the classical works of Thales and Pythagoras to the recent discoveries of Dupin and Poncelet; as well as 34 notes which go in depth into some technical, historical, or philosophical details evoked during the historical account itself⁴³. The rewritten manuscript was then entitled *Aperçu Historique*, thus reflecting what had

⁴⁰ [Wang, 2017].

⁴¹ [Poncelet, 1865], p.35.

⁴² [Quetelet, 1872], pp.36-37.

⁴³The historical study itself covers about 270 pages, while the notes run slightly under 300 pages. The two memoirs, together, make up for 275 pages.

become the focal point of Chasles' presentation⁴⁴.

Despite its title, this book should not be regarded as a purely historical account of the development of a certain science. Chasles used historical analysis as a means to an epistemological end. More precisely, through the study of past methods and theories, Chasles aimed to understand what enabled geometers to develop ever more general methods and results, and to further these investigations on that very same path. In so doing, Chasles set out to show that pure geometry had finally obtained the means to rival analysis through a process of generalization, which had culminated in the last decades. As Chasles put it in the introduction of his book⁴⁵,

Nous avons eu en vue surtout, en retraçant la marche de la Géométrie, et en présentant l'état de ses découvertes et de ses doctrines récentes, de montrer, par quelques exemples, que le caractère de ces doctrines est d'apporter, dans toutes les parties de la science de l'étendue, une facilité nouvelle et les moyens d'arriver à une généralisation, jusqu'ici inconnue, de toutes les vérités géométriques.

Such a project very much bears the mark of a certain scientific culture, in which Chasles took part through his studies. The influential teaching of analysis at the École Polytechnique by Lagrange had also placed historical considerations at the center of the very understanding of what constituted the strengths and advantages of the methods and disciplines being taught⁴⁶. For instance, in the very first pages of his *Traité de Mécanique Analytique*, Lagrange sets out to “reduce this science to general formulae”, and to “unite the different principles found until now under a single viewpoint⁴⁷”. To that end, in both parts of his book (respectively dealing with statics and dynamics), the first section was devoted to a historical overview of the principles found in past centuries, while the second section would unite them within a single general formula. This formula could then be rewritten in (or applied to) a variety of cases, thereby allowing for a systematic resolution of all mechanical problems. History thus played an instrumental as well as a pedagogical role in Lagrange's mechanics, and served to put on display the perfection reached by analytical methods, which reduce to two principles, and two formulas, the sum of all mechanical knowledge acquired throughout the centuries. While Chasles and Lagrange

⁴⁴A thorough analysis of Chasles' historiography is given in [Chemla, 2016], from which this section borrows largely.

⁴⁵“While recounting the march of Geometry, and while presenting the state of its discoveries and recent doctrines, we mainly set our sights on showing, by a few examples, that the hallmark of these doctrines is to bring, in all parts of the science of extension, a new simplicity and the means for a generalization, until then unknown, of all geometrical truths”, [Chasles, 1837], p.2.

⁴⁶See [Wang, 2017], pp.57-84., for an analysis of the pedagogical uses of the history of analysis in its teaching at the École Polytechnique.

⁴⁷ [Lagrange, 1811], p.i.

had reached very different conclusions as to what tools are best suited for the development of mechanics, and in particular the merits of analytical methods, the parallelism in the organization of their books, and their shared way of producing and reflecting on mathematical knowledge, must be emphasized here. In fact, this mathematical practice can be detected in many other works from that period and that *milieu*, such as Lacroix's 1797 *Traité du calcul différentiel*, Delambre's 1814 *Astronomie théorique et pratique* or Fourier's 1822 *Théorie analytique de la chaleur*⁴⁸.

Chasles' history of geometry takes the form of a succession in five stages ('*époques*'), each marking a decisive turn in the sort of geometrical methods available to mathematicians, and characterized by an ever-greater generality. Chasles' initial project when expanding on his prize-winning dissertation was to include a *exposition dogmatique* of some of these modern geometrical theories⁴⁹, including a theory of second-degree surfaces which eventually became the basis of his work on the attraction of ellipsoids⁵⁰. This abandoned project betrays the influence of another fellow Polytechnicien, on whom this perceived interest of returning to the historical development of sciences as a means to track what allowed for their perfecting also left a mark. Indeed, in his *Leçons de Philosophie Positive*, briefly quoted in Chasles' *Aperçu Historique*⁵¹, French philosopher Auguste Comte resorted to a similar instrumental use of the history of science. Comte famously claimed that "one can only completely know a science once one knows its history⁵²", as only history can reveal how scientific knowledge was formed, how generality was reached, how founding principles for a given science were discovered. This historical study, however, can and must be supplemented in Comte's view by an exposition of the "dogmatic march" of said science, so that the historical account becomes intelligible. Chasles' project, and his use of the term 'dogmatic', indicate that he shared Comte's positive view on the need for the joint study of a science and of its historical epistemology⁵³.

⁴⁸See for instance the preface in [Lacroix, 1797], pp.iii-xxix, where Lacroix explains that "the same discoveries being made by several geometers with very different viewpoints, therefrom many methods have resulted, among which one must make a choice, or which one must present in an order which puts on display the relations they have with one another; furthermore, it is no less necessary to give to all of those, so to speak, a uniform hue", p.iii. Similar ideas can be found in the *Discours préliminaire* in [Fourier, 1822], pp.i-xxii.

⁴⁹[Chasles, 1837], p.254. This project was eventually dropped, but gave Chasles the initial content for the redaction of several notes. To an extent, his lectures at the Faculté de Paris would allow Chasles to realize this project, and his 1852 *Traité de Géométrie Supérieure* would be framed as such an exposition dogmatique. We shall come back to these lectures in the next chapter.

⁵⁰[Chasles, 1846].

⁵¹[Chasles, 1837], p.415. Furthermore, Note V (pp.288-290) is directed against the definition of geometry as the science of the measurement of extension, which Comte had reproduced in his 10th *Leçon*. Whether Chasles was arguing with Comte directly, or other authors who also used this definition, is not made explicit in this text.

⁵²[Comte, 1830], Deuxième Leçon, p.82

⁵³In fact, Comte's tenth to fourteenth *Leçons* are devoted to the history and philosophy of geometry. Some of Comte's theses, for instance regarding the limitation of Ancient geometry, are quite close to

However, unlike the sort of narratives for the development of sciences suggested by Comte's well-known theory of the "three stages", Chasles allows for parallel developments to take place⁵⁴. For instance, he claims that both Descartes' and Desargues' geometries display levels of generality which, in their respective fields of application, were previously unattainable, while being contemporary. And yet, these geometries appear in different "époques". Despite this fact, Chasles' historical account can be regarded as narrating the progress of geometry towards generality, and stops at a point where various powerful methods have been found, which still remain to be unified under a minimal set of principles.

1.2.2 A tale of two generalities

The historical epistemologies of Lagrange, Comte, and Chasles, are all polarized by the value of generality. For Lagrange, this generality was attained through the unification of historical results within a single formula and principle; for Comte, history served the identification of the general and fundamental principles of a given science. Similarly, the *Aperçu Historique* opens with what Karine Chemla called a "diagnosis about the limits of ancient geometry⁵⁵"; and these limits consist mainly in a lack of generality that can be found in both the mathematical statements and the geometrical methods used by ancient Greek geometers. While they can certainly be linked, these two limits must first be separated and explained in their own rights⁵⁶.

The first of these limits is that certain theorems, or propositions, are redundant for the modern geometer. For instance, in the seventh book of Pappus' *Collection*, 43 lemmas are given, many of which, according to Chasles, "express a single theorem⁵⁷". The multiplicity of these lemmas follows from the fact that ancient geometers had to give different proofs for different configurations of the points involved in the theorem. For Chasles, all of

Chasles' own views.

⁵⁴Comte viewed the historical development of our methods of cognitive inquiry as passing through three stages, namely the theological, metaphysical, and positive stage. In the first stage, natural phenomena are explained through a recourse to the divine or the supernatural. In the second stage, phenomena are explained through abstract concepts. In the positive or scientific stage, explanation of natural phenomena derives from observation and experiment. Contrary to common belief, Comte does not view these three stages as ruling the development of the belief-system of an entire society, but rather of the methods used in a specific science. In particular, he allows for the possibility that two sciences, at one given time, be at different developmental stages; see [Schmaus, 1982].

⁵⁵ [Chemla, 2016], p.50.

⁵⁶Note that this criticism does not apply equally to the whole of Ancient geometry. For instance, Apollonius seems somewhat immune to it, as Chasles reads his *Conics* as bearing the mark of what would become the foundation of Descartes' analysis; namely that a single property between two magnitudes on a conic serves as a unifying notion on which the whole theory is built. See [Chasles, 1837], pp.17-18.

⁵⁷"Cette analyse des quarante-trois lemmes de Pappus nous paraît pouvoir en faire saisir l'esprit général et en faciliter la lecture. On y voit que plusieurs propositions expriment un même théorème", [Chasles, 1837], p.41.

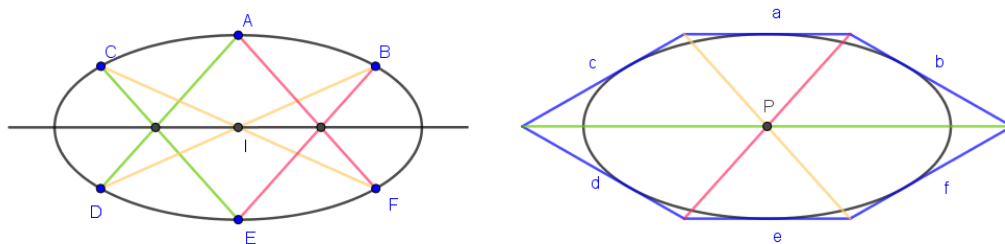
these propositions pertain to ratios between magnitudes formed by six points on a line, but some of them correspond to cases where, for instance, two of these points coincide. In each of these cases, a different proposition and a different proof are needed, which modern geometry can unite within a single statement (and, thus, a single proof). In this manner, for instance, Chasles explains that “through the consideration of negative and positive quantities, under a single statement, one theorem can present several cases⁵⁸”. For instance, in analytical geometry, denoting a segment via a letter x , one can write equations in which x appears which remain valid even when the endpoints defining the segment trade places – in which case, the number x becomes negative when it was previously positive, and conversely⁵⁹. The formula thus expresses one theorem, to which correspond many cases – namely, the many possible configurations for the objects whose measurements are denoted by symbols appearing in the formula.

Another cause for redundancy in the propositions of Ancient Greek geometry is the absence of general principles of “transformation”. In other words, there are many propositions in the Ancient corpus which are only transformed versions of one another. This can be the case of propositions which can be derived from another by duality or homography, for instance. Analytical geometry, but also modern pure geometry, is in possession of principles which link together such propositions, and eventually reduce them to a single theorem. One such example is, of course, the unity behind Pascal’s and Brianchon’s theorem which respectively express a property of six points on a conic, and six tangents to a conic. Pascal’s theorem states that the opposite sides of an hexagram inscribed in a conic intersect at three aligned points. Conversely, Brianchon’s theorem states that the three (principal) diagonals of an hexagram circumscribed around a conic intersect at a single point (see fig. below). In this sense, each is the dual version of the other⁶⁰; and what appears to us as one theorem, was for Ancient geometers many distinct and unrelated propositions. In this sense, their propositions lack generality. The importance for Chasles of the development of an adequate language for geometry must be stressed here. Not only is generality linked with the capacity of grouping *statements* (as opposed to truths, results, or formulae), but it is through the development of a certain symbolic or linguistic

⁵⁸“C’est un des grands avantages de la Géométrie moderne sur l’ancienne, de pouvoir, par la considération des quantités positives ou négatives, comprendre, sous un même énoncé, les cas divers que peut présenter un même théorème, par la diversité de positions relatives des différentes parties d’une figure”, [Chasles, 1837], pp.41-42.

⁵⁹In 2.1.3, we shall present Chasles’ geometrical introduction of negative numbers, which does not rely on algebraic variables and coordinate systems. Note that the question of the introduction of negative numbers in geometry was an old one, which had given rise to much debate among 18th-century mathematicians such as Fontenelle, Clairaut, D’Alembert, and even Carnot; see [Schubring, 2005], pp.102-113. We shall come back to this issue in 5.2.3, when discussing the concept of *solutions étrangères*.

⁶⁰Note that even their statements, that is to say their wordings, are dual of one another: it suffices to systematically swap the words ‘points’ and ‘lines’, ‘aligned’ and ‘concurrent’ etc. to transform one statement into the other one.



Pascal's and Brianchon's Theorems

grasp on geometrical statements, that the latter have been made more general. In return, Chasles claims, a large amount of lesser results can be easily deduced from truly general statements. One such example is Pascal's mystical hexagram, from which he was able to deduce 400 corollaries. This is so precisely because these corollaries all “**express** a certain property of six points on a conic⁶¹”. Similarly, this is why Descartes' geometry is so powerful, according to Chasles: because “it establishes, through a single formula, general properties of entire families of curves; so that one could not in this way discover some property of a curve without it immediately yielding similar or analogous properties in an infinity of other lines⁶²”. Obtaining general propositions was not just a matter of mathematical progress, it involved the search for modes of expressing truths in a general manner. This was not an original concern of Chasles': Poncelet also stressed “the necessity to generalize the language of geometry⁶³”, while Gergonne, as an editor (and without the consent of his authors) reorganized dual propositions on the page to make their unity as apparent as possible⁶⁴.

Besides this linguistic transformation, for Chasles, geometrical modernity necessitated a break from the reliance on concrete diagrams. While no concern for the rigour of diagrammatic reasoning would be expressed by Chasles, his concern was rather that diagrams are an impediment for truly general methods. In Chasles' historical narrative, Monge is credited with freeing pure geometry from the slew of diagrams⁶⁵:

⁶¹ [Chasles, 1837], p.73. Emphasis mine.

⁶² “La Géométrie de Descartes, [...], se distingue encore de la Géométrie ancienne sous un rapport particulier, qui mérite d'être remarqué; c'est qu'elle établit, par une seule formule, des propriétés générales de familles entières de courbes; de sorte que l'on ne saurait découvrir par cette voie quelque propriété d'une courbe, qu'elle ne fasse aussitôt connaître des propriétés semblables ou analogues dans une infinité d'autres lignes”, [Chasles, 1837], p.95.

⁶³ “Si, après les travaux géométriques des savants illustres qui composent la moderne École, on peut encore former l'espoir de faire faire quelques progrès vraiment utiles à la science de l'étendue, ce ne peut être évidemment qu'en suivant de près leurs traces, qu'en cherchant sans cesse à généraliser le langage et les conceptions de la Géométrie”, [Poncelet, 1865], p.xvi.

⁶⁴ [Gray, 2007], pp.55-62; [Lorenat, 2015b].

⁶⁵ “This useful influence of descriptive Geometry extended naturally just as well to our style and language in mathematics, which it made easier and clearer, by freeing the complications brought about by

Cette influence utile de la Géométrie descriptive s'étendit naturellement aussi sur notre style et notre langage en mathématiques, qu'elle rendit plus aisés et plus lucides, en les affranchissant de cette complication de figures dont l'usage distrahit de l'attention qu'on doit au fond des idées, et entrave l'imagination et la parole. La Géométrie descriptive, en un mot, fut propre à fortifier et à développer notre puissance de conception; à donner plus de netteté et de sûreté à notre jugement; de précision et de clarté à notre langage.

A second criticism expressed by Chasles towards Ancient geometry is the lack of systematicity in the application of its methods⁶⁶. For instance, Archimedes' method of exhaustion, while quite general with regards to the variety of figures to which it can be applied, is no match for modern integral methods as far as the systematicity of the method is concerned. For Chasles⁶⁷,

La méthode d'exhaustion, qui reposait sur une idée mère tout à fait générale, n'ôta point à la Géométrie son caractère d'étroitesse et de spécialité, parce que cette conception y manquant de moyens généraux d'application, devenait, dans chaque cas particulier, une question toute nouvelle, qui ne trouvait de ressources que dans les propriétés individuelles de la figure à laquelle on l'appliquait.

This method requires a renewed effort every time it is applied to a new figure, and thus does not follow systematically from the application of a certain set of rules. Consequently, new insights were required for each of its application. The generality of a method is here linked to its systematicity, and even to the effortlessness or simplicity of its application. By contrast, Newton's methods of fluxions would provide an "uniform analytical process"⁶⁸ for the obtention of tangents to a given curve. When applying the same method to a different figure or a different curve, one only has to change a few words and expressions.

Indeed, to measure the generality of a past method, Chasles often employs what we call a "stability criterion": if a proof or a method is such that one can substitute in it all occurrences of a figure by a more general one (or by another one of the same kind), and still have a valid proof for the new figure, then the method is general. This is

figures, whose use distracts from the attention we owe essentially to ideas, and which hinders imagination and speech. In a nutshell, descriptive Geometry was adequate for fortifying and developing our powers of conception; to give our faculty of judgement more sharpness and certainty; our language more precision and clarity", [Chasles, 1837], p.190.

⁶⁶This notion of generality can be compared to Steiner's "Systematizität"; see [Lorenat, 2015a], p.206.

⁶⁷"The method of exhaustion, which rested on a completely general main idea, did not deprive Geometry of its character of narrow-mindedness and specialization, since this conception, lacking general means of application, became, in each particular case, a wholly new question, which found resources only in the individual properties of the figure to which it was applied", [Chasles, 1837] p.52.

⁶⁸ [Chasles, 1837], p.59.

particularly striking in Chasles' assessment of the generality of MacLaurin's results on the attraction of ellipsoids. In his 1742 *Treatise on Fluxions*, MacLaurin had studied the relationship between the attractions exerted on one same point by two confocal ellipsoids. MacLaurin had shown that these attractions, for two confocal ellipsoids of revolution, had the same direction and the same ratio as their volumes. Then, according to D'Alembert, Lagrange, and Legendre, he had only stated an analogous theorem for general ellipsoids. Chasles, however, disagreed with this assessment. MacLaurin had prefaced this second theorem with the expression "in the same manner"; and, according to Chasles, one simply had to "repeat the reasonings that MacLaurin had given a few lines before, without changing, adding, or subtracting a single word", except of course for the figures at play⁶⁹. Because Chasles understands the generality of a method or a proof-strategy in a particular manner, he draws different historiographical conclusions with respect to the merits of past mathematical texts than others. More largely, this ideal of a proof apparatus in which one can simply change a few words and retain a stable structure is one that would largely guide his later mathematical practice, as we shall see in particular in section 4.3.

Therefore, the generality that Chasles finds lacking in Ancient geometry, and whose emergence he wishes to describe and prolonge, is at least twofold. It is both at the level of statements and of methods. In the first case, it refers to abstraction of a proposition, that is to say, that it measures how it can be unfolded into a variety of concrete cases. In the second case, generality is to be read as the systematicity of a method, that is to say, that it measures how it can effortlessly be applied to a variety of figures. These two generalities are obviously related: in particular, they both yield easy, effortless inferential practices. A general theorem yields immediately many simpler propositions; while a general method can yield without much effort many results when applied to various figures. Indeed, as we shall now see, the equivalence of the values of simplicity and generality is a cornerstone of Chasles' epistemology of geometry.

1.2.3 From generality to simplicity

At several occasions in the *Aperçu Historique*, Chasles claims that the most general principles are, by nature, the simplest⁷⁰. To this claim, Chasles gave a theoretical, *a priori*

⁶⁹"D'Alembert, et ensuite Lagrange et Legendre, avaient pensé que Mac-Laurin n'avait fait qu'énoncer son théorème, sans en donner la démonstration; c'était une erreur de la part de ces trois illustres géomètres, car cette démonstration est identiquement la même que celle du cas qui avait précédé, et l'auteur dès lors devait se borner, comme il a fait, à ces seuls mots: *l'on prouvera de la même manière*, etc.; et ne pas répéter des raisonnements qu'il venait de faire quelques lignes plus haut, et auxquels il n'y avait à changer, ni ajouter ou retrancher aucun mot", [Chasles, 1837], pp.168-169. For the original text by MacLaurin, see [MacLaurin, 1742], p.131.

⁷⁰Of course, this is by no means a unique claim to make. For instance, Grothendieck famously equated generality or abstraction to "childish simplicity", see [McLarty, 2003]. However, not only is the epistemological construction of this equivalence different in Chasles and Grothendieck, but, more crucially, so

justification⁷¹:

Les principes les plus généraux, c'est-à-dire qui s'étendent sur le plus grand nombre de faits particuliers, doivent être dégagés des diverses circonstances qui semblaient donner un caractère distinctif et différent à chacun de ces faits particuliers, considéré isolément, avant qu'on eût découvert leur lien et leur origine commune : s'ils étaient compliqués de toutes ces circonstances ou propriétés particulières, ils en porteraient l'empreinte dans tous leurs corollaires, et ne donneraient lieu, généralement, qu'à des vérités excessivement embarrassées et compliquées elles-mêmes.

Here, Chasles uses the term 'principle' rather ambiguously; he is likely not referring to principles such as that of duality or continuity, but rather to these central propositions which, like Pascal's mystical hexagram, easily yield a large number of truths⁷². Pascal's hexagram, thus, is said by Chasles to express a general property of six points on a conic. By transforming it (by duality, by homography, by choosing a specific configuration etc.), one can derive from it many properties of six points on a conic. In that sense, these principles are general propositions which, through transformations, can represent a variety of cases: in this sense, they are propositions of a new kind, absent from Ancient geometry⁷³.

In so doing, these principles serve to regroup what previously were thought to be separate propositions, for instance putting one unique theorem in lieu of the various cases of a configuration. Proving a unique theorem is not only simpler in that sense that it requires a single effort, but, more crucially, it is simpler because it requires that a chain of deductions be established from the common origins of all these separate propositions. For instance, Pappus' 43 lemmas are all one property of a concept which had emerged in the nineteenth century, namely that of the involution⁷⁴: modern geometry, built around

is the cultural and institutional context in which it is mobilized.

⁷¹"The most general principles, that is to say those that extended their domain to the largest number of particular facts, are necessarily free from the various circumstances which seemed to give a distinct and different character to each of these particular facts when conceived in isolation, prior to the discovery of their common link and origin: if they were complicated by all these particular circumstances and properties, they would bear the mark of these particularities in all of their corollaries, and would, in general, only give rise to truths which excessively embarrassed and complicated themselves", [Chasles, 1837], p.116.

⁷²The ambiguity of the word 'principle' in Chasles' writings was already noted in [Chemla, 2016], pp.59-60.

⁷³"On observe que ces divers théorèmes principaux exprimaient, chacun, une certaine propriété de six points situés sur une conique: cela explique comment Pascal avait pu les déduire de son hexagramme mystique, qui était lui-même une propriété générale de ces six points. Mais chacun de ces théorèmes avait pris une forme différente, qui le rendait propre à des usages particuliers, comprenant un nombre immense de propriétés des coniques. C'est cet art infiniment utile de déduire d'un seul principe un grand nombre de vérités, dont les écrits des Anciens ne nous offrent point d'exemples, qui fait l'avantage de nos méthodes sur les leurs", [Chasles, 1837], p.73.

⁷⁴We shall discuss the notions of cross-ratio and involution in the next chapter, see 2.2.2.

this notion, is thus able to explain these 43 lemmas as a single property of six points on a straight line. In return, each of these lemmas is one particular case of a more general truth. More generally, diagram-based inferences, for Chasles, always run the risk of relying on specific properties of the diagram. Chasles worries not about the dangers of intuition, or that such reliance can lead to false deductions; but rather that the use of diagrams introduce in a proof arguments that are not necessary in the general case, thereby obfuscating the real reasons for the validity of the proposition.

To obtain general propositions and methods, thus, one must search for the adequate theoretical settings within which a figure or a question can be studied. The importance of the concept of involution for the general and simple treatment of Pappus' lemmas is similar to the treatment given by Chasles to many past geometrical results: for instance, MacLaurin's work on the attraction of ellipsoids is shown to be a particular form of a general theory of second-order surfaces⁷⁵. Chasles' concept of simplicity can then be conceived in at least two ways. A simple theory is centered around the right principle, that general proposition from which all others derive: in this sense, the simplicity of modern geometry contrasts with the multiplicity of ancient methods. In a different but related sense, the simplicity of a theory means that all the propositions composing it can be obtained effortlessly, by simple transformations of some initial truth. Here, simplicity contrasts with the difficulty of geometrical inquiry without departing from first principles.

The strong equivalence between generality and simplicity, for Chasles, means that geometrical practice evolved alongside the generality of its propositions and methods. In ancient times, Chasles explains, inferential practices went from particular cases to particular cases. The ancient geometers "went from some very simple proposition to another one of the same kind, but slightly more general, and then to that other one similarly more extended; so that the proof of even a somewhat general proposition first required the proof of many of its particular cases⁷⁶". The portrait of modern geometrical life is totally different: modern geometers first strive to establish the most general propositions possible, and only then they apply these general propositions to particular cases in a downward motion.

In this regard, Chasles depicts a practice of generality that belongs to the *milieu* which we described earlier: in both parts of his *Mécanique Analytique*, Lagrange first gave a general formula, whose transformations then could be applied to a variety of cases. More generally, the emphasis on the need to first expound general principles in view

⁷⁵ [Chasles, 1837], pp.384-399.

⁷⁶"C'était là la marche habituelle et nécessaire du géomètre qui s'élevait de quelque proposition très-facile à une proposition du même genre, mais un peu plus générale, et de celle-ci à une autre aussi plus étendue; de sorte que la démonstration d'une proposition tant soit peu générale exigeait celle de plusieurs de ses cas particuliers", [Chasles, 1837], p.185.

of their ulterior applications mirrors the pedagogical ideal which had presided over the creation of engineer schools such as the École Polytechnique.

1.2.4 Natural methods & human conceptions

There remains one crucial question to address: why is it always possible, when conducting geometrical research to find such general principles as Pascal's mystical hexagram? Why are we to expect, whilst studying some other figure or problem such as cubics or some mechanical motion, that there is a fundamental principle whose transformations will easily yield the properties we seek? Chasles answers these questions with a crucial decree of faith: nature provides the germs for simple methods, which therefore must be general. Conversely, by following natural methods, one will walk upon simple paths, thereby reaching generality. In fact, the latter view is one that Chasles attributes to Tschirnhaus, whilst most likely quoting directly from Fontenelle's *éloge* of the latter⁷⁷:

Le but constant de Tschirnhausen, dans ses diverses spéculations géométriques, était de rendre la Géométrie plus aisée, persuadé que les véritables méthodes sont faciles; que les plus ingénieuses ne sont point les vraies, dès qu'elles sont trop composées, et que la nature doit fournir quelque chose de plus simple.

The lines directly following this quotation, but also various other passages of the *Aperçu Historique*, show that Chasles indeed made this goal his own⁷⁸.

The notion of naturalness pervades throughout Chasles' entire book: the propositions which result from transformations of Pascal's hexagram are said to “derive naturally⁷⁹” from it, while Huygens' theory of the evolute of a curve “lead naturally to the knowledge of central forces in circular motions⁸⁰”. More profoundly, it underlies Chasles' diagnosis of the relative strengths of (modern) pure geometry and analysis.

Analytical methods, here, refer to the use of coordinate systems to form equations, and of algebraic and infinitesimal computations to carry out transformations on said

⁷⁷“Tschirnhaus' constant goal, in his various geometrical speculations, was to make Geometry easier, as he was persuaded that the true methods are easy; that the most ingenious are not the true ones, as soon as they are too complex, and that nature must provide something simple”, [Chasles, 1837], p.114. This sentence is almost given *verbatim* in [Fontenelle, 1733], p.122. Chasles quotes neither Tschirnhaus nor Fontenelle in the *Aperçu Historique*.

⁷⁸In fact, earlier in the *Aperçu Historique*, Chasles had quoted a similar thesis by way of Descartes' fourth rule in the *Regulae ad directionem ingenii*: “Je me persuade que certains germes primitifs des vérités que la nature a déposés dans l'intelligence humaine [...] avaient, dans cette simple et naïve antiquité, tant de vigueur et de force, que les hommes éclairés de cette lumière de raison qui leur faisait préférer la vertu aux plaisirs, l'honnête à l'utile [...] s'étaient fait des idées vraies et de la philosophie et des mathématiques, quoiqu'ils ne pussent pas encore pousser ces sciences jusqu'à la perfection”, [Chasles, 1837], p.29. As we shall see in section 2.1.1, the term ‘germe’, especially in the context of a discussion of Ancient geometry, is one that Chasles very much made his own too.

⁷⁹ [Chasles, 1837], p.81.

⁸⁰ [Chasles, 1837], p.104.

equations⁸¹. It must be first stressed that Chasles was absolutely not opposed to the practice or to the teaching of analytical methods. He never doubted the validity of analytical proofs, nor did he express worries regarding the so-called ‘metaphysics of the calculus’⁸². In fact, Chasles lauded in many instances the sheer efficiency and speed of analytical methods, as well as the generality provided by the “universal instrument”⁸³ that Descartes’ equations form.

And yet, for Chasles, what makes analysis so powerful is also its main limitation. “The main advantage of Analysis”, Chasles assesses, is that “it has the marvelous privilege of neglecting the intermediary propositions which [Geometry] always require”⁸⁴. Indeed, the path taken by the analyst from one geometrical proposition to another is one which proceeds by the introduction of some “auxiliary and artificial system of coordinates”⁸⁵. The axes one introduces to write equations and carry out algebraic computations, or the infinitesimal volumes which one introduces to compute the attraction of a body, are both expressed in terms of objects totally foreign to the figures under study. This introduction of artificial quantities is a powerful device, because it allows for the direct obtention (or computation) of some geometrical proposition; but it also obfuscates the natural paths which link propositions together.

This lack of clarity, Chasles reckons, has real consequences. For instance, in his work on the attraction of ellipsoids, Legendre introduced infinitesimal volumes which could have been interpreted geometrically: they are, Chasles would show, infinitely small cones in a special position with respect to the ellipsoid. Observing this geometrical configuration, in return, would put Chasles on the path for a more general theory of level-surfaces, wherefrom the attraction of much more general bodies can be obtained easily. Thus,

⁸¹In the first lecture on higher geometry he ever gave at the Faculté de Paris, Chasles defined analytical geometry as “the art of representing lines and curved surfaces via algebraic equations”, [Chasles, 1847], p.18. We shall come back to Chasles’ discussion of the history of the categories of analysis and synthesis in section 2.1.1.

⁸²Poncelet did not doubt the validity of analytical methods either. In fact, Monge and Poncelet both wrote a treatise on analytical geometry. In that sense, one cannot adequately characterize the French tradition of geometry as a “more rigorous alternative to the analytic approaches which had typified 18th century French mathematics”, as is claimed in [Daston, 1986], p.269. In fact, when Chasles mentions the value of rigor, it is sometimes in negative terms. For instance, the rigor of Ancient Greek geometers prevented them from uncovering the general principles underlying some of their propositions: “on ne peut se dissimuler qu’on doit à ce relâchement de la rigueur des Anciens, les progrès immenses que les Modernes ont faits dans la Géométrie”, [Chasles, 1837], p.359.

⁸³ [Chasles, 1837], p.118. This description of Descartes’ *Géométrie* would be repeated by Chasles across many papers, including those which would form the basis of his enumerative theory of conics. On the renewed comparison between pure geometry and algebra that Chasles would construct in this context, see section 4.2.4.

⁸⁴“C’est en cela, soit dit en passant, que nous paraît consister le principal avantage de l’Analyse moderne sur la Géométrie. La première de ces deux méthodes a le merveilleux privilège de négliger les propositions intermédiaires dont la seconde a toujours besoin”, [Chasles, 1837], p.114.

⁸⁵ [Chasles, 1837], p.119.

because it obfuscates the chains of truths linking propositions, analysis could not identify the real significance of these surfaces⁸⁶:

Déjà, dans les plus savantes recherches physico-mathématiques, l'Analyse a dévoilé la présence de ces surfaces; mais le plus souvent on a regardé une si heureuse circonstance comme fortuite et secondaire, sans songer qu'au contraire elle pouvait se rattacher directement à la cause première du phénomène, et même être prise pour l'origine réelle, et non pas accidentelle, de toutes les circonstances qu'il peut offrir.

Thus, the recourse to efficient but extrinsic methods may lead to a lack of clarity; thereby impeding the discovery of the natural setting, the fundamental principle around which to center the geometrical investigation. This, in turn, results in a mathematical practice which is blind to possible generalizations.

The geometer does not have access to such artificial auxiliaries; instead, they must always proceed from “direct and natural methods⁸⁷”, relying only on “the attentive consideration of things and the succession of ideas⁸⁸”. Indeed, when investigating the properties of a certain figure, they will have to go over the entire chain of truths, moving from proposition to proposition. Because of this forced slower pace, a more thorough grasp of the ways in which propositions are linked together is gained: this is how, for instance, Chasles can describe Pascal's theorem as a “center⁸⁹”, a nodal point in the theory of conics. The knowledge provided by geometrical methods, therefore, is not only that of the truth of a certain proposition; but it is also the knowledge of its position within the natural theory to which it belongs⁹⁰. For this reason, it is a knowledge that is more useful. Knowing its position makes it more evident when a certain proposition is to be employed, but also how it derives from first principles, and how it can be generalized further.

What Chasles is constructing here is not just a general epistemology of geometrical knowledge, but also a new model for the construction and teaching of mathematical theories; one which he would apply in his own teaching. For instance, in his 1852 *Traité de Géométrie Supérieure*, Chasles wrote⁹¹:

⁸⁶“Already, in the most savant physical-mathematical investigations, Analysis has uncovered the presence of these surfaces; but most often such a happy circumstance was viewed as contingent and secondary, and no attention was paid on the contrary to the possibility that this circumstance could tie into the first cause of the phenomena, and even be taken to be the real (and not accidental) origin of all the circumstances it can offer”, [Chasles, 1837], p.251.

⁸⁷ [Chasles, 1837], p.252.

⁸⁸“La Géométrie [...] ne puise ses inspirations que dans la considération attentive des choses et dans l'enchaînement des idées”, [Chasles, 1847], p.39.

⁸⁹ [Chasles, 1837], p.81.

⁹⁰By stressing that mathematical practice is not purely about producing new results, but more largely about producing understanding, the 20th-century geometer William Thurston frequently (and inadvertently) echoed these key points of Chasles' epistemology; see in particular [Thurston, 1994].

⁹¹“This march was all the more necessary because, in general, it is not enough to know that a proposition

Cette marche était d'autant plus nécessaire, qu'en général il ne suffit pas de savoir qu'une proposition est vraie pour qu'on puisse en faire un usage utile en Mathématiques; il faut encore connaître toutes ses dépendances avec les diverses propositions qui se rattachent au même sujet. Quand cet enchaînement est mis à nu, tout devient facile, et il est même rare que l'on ne puisse pas démontrer une proposition de bien des manières, car on y arrive par toutes celles qui la touchent de quelque côté. C'est là un criterium qui permet d'apprécier jusqu'à quel point on a pénétré dans le sujet que l'on traite, et combien il peut encore laisser à désirer.

1.2.5 Geometry without the burden of proof

The opposition between analysis and geometry is conceived of by Chasles as one between “human conceptions” and “natural methods⁹²”. Mobilizing these human conceptions require “subtleness and ingenuity⁹³”; they are only worthwhile in the hands of “savants”. These skills are rewarded with the ability to move directly from an hypothesis to almost any theorem quickly and efficiently, thereby jumping over the tight web of propositions which naturally link said hypothesis and theorem. By contrast, natural methods require no genius whatsoever; they are direct and simple, and the knowledge they yield is ready to be communicated to the ‘man in the street’. To use these methods is to move effortlessly within the web of propositions. Indeed, Chasles describes at various points of the *Aperçu Historique* the work of the analyst as being painful⁹⁴, difficult, demanding; thereby displaying the unfettered motions of the geometer.

What results from these contrasted portraits is a new picture of mathematical life, that is to say a regulative ideal for what a mathematician ought to be and do. Chasles' ideal geometer is almost entirely free of the burden of proof⁹⁵: instead, his task is to generalize concepts and methods, thereby bringing clarity and completeness to mathe-

is true for it to be useful in Mathematics; it is also necessary to know all its dependencies with the various propositions which are related to the same subject. When this succession is laid bare, everything becomes easy, and it is even rare that one cannot prove a proposition in many ways, for it is proven by all the propositions that touch it in some way. This is a criterium which makes it possible to appreciate how far one has penetrated into the subject one is dealing with, and how much it may still leave something to be desired”, [Chasles, 1852], p.435. Through these lectures, Chasles transmitted this epistemic ideal to his students. This will be apparent in the case of De Jonquières, whose work on the generation of curves we discuss in section 3.2; but it is also true of lesser-known authors, see for instance the introduction of [Serret, 1869].

⁹² [Chasles, 1837], p.114.

⁹³ [Chasles, 1837], p.238.

⁹⁴“Les calculs et transformations savantes et pénibles de l'analyse”, [Chasles, 1837], p.251. In fact, geometrical practice is equally painful when done in the manner of the Ancients, that is to say without the help of general principles, see [Chasles, 1837], pp.39-40.

⁹⁵A similar observation is made in [Chemla, 2016], p.60.

mathematical theories and centering them around the most fundamental principles. Geometers, for Chasles, ought to strive not necessarily to prove theorems, or to compute magnitudes, but rather “to search for the supreme laws⁹⁶” behind geometrical truths.

In other words, mathematical life for Chasles is not primarily aimed toward the production of truths, but rather toward the refining of acquired truths, by means of the search for the most natural theoretical setting within which it can be inserted. That such is the normative ideal for mathematical practice conceived by Chasles is made explicit in the *Aperçu Historique*, wherein two ‘rules’ are laid out in this sense⁹⁷. The first rule is that a geometer ought to always strive for greater generality for each of the propositions they have obtained. The second rule is that, when proving a proposition, one should always strive to find a proof that locates said proposition within the “doctrine to which it belongs naturally”. Proving is not merely about certifying the truth of a statement; it is a task which ought to produce a broader knowledge of the place of said statement within its natural theoretical setting. In turn, that these two tasks have been fulfilled is both demonstrated and rewarded by the fact that a large number of truths will derive easily from the proposition thus adequately generalized and proven. In fact, this profusion of simple consequences is, for Chasles, “the mark of a truly perfect science”.

We shall see in the following chapters that, to a large extent, this ideal ruled Chasles’ own scientific practice, in at least two ways. The first is that Chasles, at many points of his careers, set out to ‘re-prove’ old theorems, relying on methods and theories which he deemed more natural for the problem at hand. This is the main motivation for his own work on the attraction of ellipsoids, after the works of MacLaurin, Legendre, and Poisson. More largely, as we shall see in 3.1, Chasles would often blend historiographical and mathematical reflections in papers which revive old problems to provide them with new, more general answers: crucially, this will be the case of the early-modern tradition of problem-solving and of the generation of curves.

The second way is that mathematical propositions which some would immediately

⁹⁶“Il est toujours utile de contempler les vérités géométriques dans leur plus grande étendue, dans leur plus grande généralité, dans leur plus grande approximation, pour ainsi dire, des lois suprêmes, dont la recherche doit être l’objet constant des efforts des géomètres”, [Chasles, 1837], p.240.

⁹⁷“Generalize more and more particular propositions, in order to attain, step by step, what is most general; which will always be, at the same time, the most natural and the simplest.

Within the proof of a theorem or the solution of a problem, never be satisfied by an initial result which would be enough in a particular case viewed independently of its place within a general system in science; but be satisfied by a proof or solution only when its simplicity, or its intuitive deduction from some known theory, will prove that you have attached the question to the very doctrine it naturally depends on.

To indicate a way to recognize whether the practice of these two rules has led to the desired goal, that is to say whether we have marched on the true roads of definitive truth, and reached its source, we believe that, in each theory, there must always be, and we must always be able to recognize, some principal truth from which all others easily follow, as simple transformations or natural corollaries; and that this fulfilled condition only will be the mark of the true perfection of a science”, [Chasles, 1837], p.115.

identify as theorems in need of a proof would have a very different status for Chasles. In section 4.2.3, we shall encounter an enumerative result which, for Chasles, would be the source of a quasi-infinite number of other, simpler truths. This profusion of simpler truths would serve as a sure sign that a natural and perfectly general method had been reached, and Chasles would be largely disinterested in other mathematicians' attempts at proving this original truth. Thus, this epistemic portrait of geometry results in a different identification of what makes a theorem (and what sort of epistemic work is required for a theorem to be accepted) than many other 19th-century actors would accept.

Conclusion

Through an intricate blend of historiographical, epistemological, and mathematical analyses of past geometrical texts, Chasles' *Aperçu Historique* produces an epistemic portrait of the ideal geometer. We have shown that this ideal portrait functions as a “moral economy” for scientific life, that is to say a “web of affect-saturated values that stand and function in well-defined relationship to one another⁹⁸”, namely the values of simplicity, generality, and naturalness, to which correspond affects such as effortlessness or painfulness. In the following chapter, we shall focus on Chasles' teaching of higher geometry at the Faculté de Paris. In so doing, we shall see that this epistemic portrait would have important consequences for the choice of mathematical problems, but also the construction of notations and other textual devices. While we have so far presented generality for Chasles as an epistemological value and an epistemic virtue, we now approach it as a practice.

⁹⁸ [Daston, 1995], p.4.

“In a single stroke of the quill”: on Chasles’ rewriting of pure geometry

Introduction

While the *Aperçu Historique* gathered an impressive collection of Chasles’ historical, epistemological and mathematical insights, it remained an unfinished work. The recourse to historical studies was for Chasles a means for an epistemological end: looking back at the development of geometrical methods was supposed to provide insights into how pure geometry could be made more general, and founded anew upon a minimal collection of principles. The *Aperçu Historique* was, from the onset, meant to be supplemented by what Chasles called a “dogmatic exposition”, borrowing a term from the positivist philosophy of Auguste Comte¹, a friend and classmate of Chasles’ at the Ecole Polytechnique. Comte distinguished between two modes of exposition of a science: the historical one, and the dogmatic one. While both are necessary in the proper teaching of a science, according to Comte, the latter brings something that the former lacks in that it allows “new conceptions to present past discoveries under a more direct point of view²”. Similarly, Chasles expressed in the *Aperçu Historique* the necessity, through such a dogmatic exposition, to “coordinate these partial and isolated truths [which rational Geometry until then had produced], to make them all derive from only a few of them, taken amongst the most general³”, that is to say to subsume the various historical discoveries of the past under a unified, more direct point of view.

Such was the task that Chasles undertook as soon as a chair of *Géométrie Supérieure* (*Higher Geometry*, in what follows) was specially created for him at the Faculté de Paris

¹ [Comte, 1830], pp.77-83.

² [Comte, 1830], p.78.

³ [Chasles, 1837], p.234.

in 1846, thanks to the intense lobbying of his institutional and philosophical ally, the mathematician Louis Poinot⁴. Through his teaching, of which several traces have been preserved⁵, Chasles set out to unify and systematize the sheer bulk of modern geometrical methods, whose virtues he had extolled in the *Aperçu Historique*. In this chapter, we show how this project entailed the constant reshaping of new expressive tools, or “literary technologies”, which Chasles used to define geometrical figures and transformations thereof in what he viewed as a perfectly general and abstract manner. We borrow the term of “literary technology” from the influential studies of Boyle’s description of experiments involving his air-pump, by Steven Shapin and Simon Schaffer⁶. By this term, they referred to a specific form of textual production with the aim to communicate a phenomenon produced by the air-pump to those who could not directly witness the experiment. Obviously, the case of mathematics does not involve experiments, and it is not clear at first what witnessing could possibly mean for mathematical proofs and results, and what exactly it is that words should replace. For us, literary technologies will refer to all sorts of expressive resources (diagrams, symbols, but also textual forms) that allow for a mathematical proposition to capture a certain level of generality.

In his 1999 book *The Shaping of Deduction*, Reviel Netz analyzed the interrelated functions of diagrams and highly-structured textual forms within the deductive practice of Ancient Greek geometers. Not only are diagrams and texts both necessary to fully understand the proofs and statements of Greek Geometry, Netz showed; but there also is a complex interplay between them that serves to ensure the generality of the knowledge thus acquired. How can one ensure that the proof of the Pythagorean theorem, as given in Euclid’s *Elements* is not solely valid for the diagram being drawn in front of us, especially if we cannot do without this diagram? Netz suggests that a certain *repeatability* of the cognitive and material moves of the proof, which involves both text and diagram, is key to answering this question⁷. By restoring the cognitive and material practice that allows Greek proofs to express general truths, Netz’s book can be described as a detailed study

⁴See [Boudin, 1869], p.6. Chasles later acknowledged this debt in [Chasles, 1870], pp.219-220. The philosophical and epistemological proximity between Chasles and Poinot is evident in the *Aperçu Historique*, see for instance [Chasles, 1837], pp.415-416, 614-615. Comte also praised Poinot’s work on mechanics in terms close to Chasles’, see [Comte, 1830], pp.611-616, 710-714.

⁵The opening lecture of the year 1846-1847 has been published in two slightly different versions, while an unpublished, handwritten draft of the opening lecture of the year 1847-1848 can be found in Chasles’ scientific archives at the Paris Académie des Sciences, 35J/11. This document will be transcribed and analyzed in a forthcoming joint paper with Ivahn Smadja. Chasles also published two treatises, and a plethora of short papers, which all seem to match the yearly evolution of his lectures, both in choice of topic and in style.

⁶[Shapin, 1984], [Shapin & Schaffer, 1985].

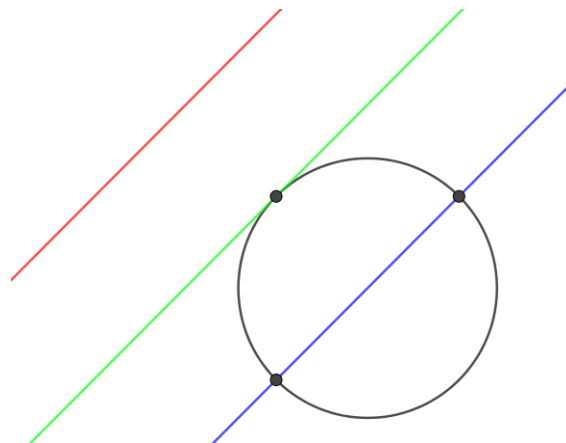
⁷[Netz, 1999]. For the co-dependance of diagrams and texts in Greek geometry, see pp.12-67, for the shaping of generality in these proofs, see pp.240-270. A convincing comparison of Netz’s book and the historiographical approach of Shapin and Schaffer – against Netz’s explicit rejection of this interpretation in [Netz, 1999], p.3 – can be found in Bruno Latour’s insightful review, [Latour, 2003].

of a “literary technology” carefully crafted by a community of mathematicians, and whose mode of functioning is so intricate that it is partially “invisible” to its users⁸.

The mathematical issue at stake was different for the geometers of Chasles’ times: the generality they wanted to capture with their geometrical statements and proofs was on another level entirely from that of rigid diagrams. Their rival, namely Algebra (or, more broadly, what they construed as “analytical methods”), had brought to the fore new geometrical entities, such as imaginary points, infinitely-near points or lines, points at infinity etc. For these new defenders of pure geometry, generality was not something to achieve through rigorous use of diagrams, or careful axiomatic practices: it was currently the preserve of analytical methods, and something to recapture for geometry through new methods and expressive resources. When an algebraist wrote the equation of a circle and of a line, in the same system of coordinates, their equations automatically gave rise to two intersection points, possibly imaginary. Take, for instance, a circle of center $(0, 0)$ and of radius 1. Its equation will be

$$x^2 + y^2 = 1$$

Any line of equation $y = ax + b$ will yield two intersection points, obtained by solving the equation $(1 + a^2)x^2 + 2abx = 1 - b^2$, which has two (potentially complex) roots. With analytical methods, it was possible to handle two intersection points of a circle and a line in complete generality, and to express propositions about them. To echo Chasles’ laudative assessment of Descartes’ contribution to the history of geometry, thanks to algebraic equations, the whole variety of possible cases is “subdued in a single stroke of the quill⁹”. A diagram representing this configuration, however, will necessarily distinguish between three apparently different cases: the straight line can cross the circle at two points, it can be tangent to the circle, or it can be completely exterior to it (see fig. below).



This was for Chasles a serious problem: among other things, it showed that relying on

⁸ [Netz, 1999], p.2.

⁹“Descartes a soumis, d’un trait de plume, l’innombrable famille des courbes géométriques [à la Géométrie des formes et des situations]”, [Chasles, 1837], p.143.

diagrams was to be proscribed, because it hinders the geometer’s ability to think generally. When we think with diagrams, we immediately distinguish amongst seemingly separate cases which, in fact, are one¹⁰. Algebra shows one way to conduct proofs generally, but with heavy epistemological costs. The problem therefore is: how should pure Geometry be advanced or renewed in a way that allows it to express the same level of generality? How should one talk and write about the circle and the line, without the help of Algebra, but also without having to distinguish between these three cases?

In this chapter, we explore how Chasles confronted this problem when having to design a systematic teaching of geometry to advanced students. This task, we show, involved a renewal of notations for some fundamental geometrical entities, such as segments and angles. This renewal was largely informed by novel historiographical theses that Chasles drew from his work on the history of algebra, and of his constant interest in Ancient Greek analytical geometry. Indeed, this chapter argues that Chasles started to view his geometrical research as the modernization of the Ancient Greeks’ “geometrical analysis”, which he did by reinventing a language for the uniform expression of complicated geometrical configurations. This language was meant to import the epistemic benefits of Algebra’s unknowns and variables, but without resorting to artificial and extrinsic systems of coordinates. In so doing, we show, Chasles crafted a literary technology aimed at expressing geometrical truths in a manner that was perfectly general and abstract, whilst still direct and intuitive.

2.1 Geometry made methodical and abstract

In this section, we focus on Chasles’ teaching and exposition of geometry as it can be found in his 1852 *Traité de Géométrie Supérieure*, that is to say the first book to grow out of his teaching at the Faculté de Paris. We focus on the renewal of Chasles’ historiographical and epistemological theses on the status of algebra, abstraction, and symbols in the development of modern geometry, which we then tie to the new definitional and notational approaches employed in said *Treatise*.

2.1.1 Geometrical analysis without algebra

On December 22nd 1846, Chasles gave the inaugural lecture of his newly founded chair of Higher Geometry, the content of which would be published first in Liouville’s Journal¹¹,

¹⁰At least, these three cases are one from a certain perspective on generality in geometry. For that reason, the literary technologies constructed by Descartes or Chasles *construct* generality as much as they *express* it.

¹¹ [Chasles, 1847].

then at the beginning of his 1852 *Traité de Géométrie Supérieure*¹², the first book to grow out of his teaching. These two texts differ only slightly: the latter is generally more detailed than the former, which is probably closer to the actual lecture given that day. For our purpose here, we will only refer to the second, more detailed version of the text, as our focus is on the mathematical, epistemological, and historical conceptions that led to the structure of the *Traité* than on the actual teaching professed by Chasles, especially in the early years.

The inaugural lecture starts with a description of the circumstances and motivations for the creation of this chair. Chasles explains that this teaching was supposed to fill a gap in the cursus available to French students, and to catch up on recent changes within higher education in Germany and England. Indeed, continues Chasles, while elementary geometry was taught to most students in French universities, more recent methods and results were only introduced via the teaching of analytical methods, that is to say through the algebraic manipulations of quantities expressed with respect to a certain choice of coordinates. Whether Chasles' diagnosis of the state of French higher education – and in particular of the teaching of mathematics in French universities and schools – is correct is of little relevance to us here. More important to us is that this justification of the creation of the chair allows Chasles to explain what exactly is needed to remediate the supposedly undesirable state of pure geometry in France.

Chasles quickly points out at the beginning of this lecture that a course in higher geometry cannot be limited to the study of great works of the past, whether it be that of the Ancient Greeks or of important 17th- and 18th-century authors such as Newton or MacLaurin. The reason why that cannot be, however, is perhaps not that which one might expect. It is not that their results have fallen into obsolescence, either because of their relative triviality compared to the intricate theorems and formulas that newer theories discovered or because they contained mistakes, imprecisions, or a lack of rigor. On the contrary, the results these works contain are still worthy of admiration for Chasles¹³ and can even convince the attentive reader that Geometry has plenty to offer in a variety of questions¹⁴. However, what they ultimately lack is a unified presentation as a “body of doctrine”, and as a “set of methods”. Propositions such as those that Chasles read in

¹² [Chasles, 1852], pp.xxxv-lxxxiii. In the second edition of this treatise, published in 1880, the *Discours* is reproduced at the end of the volume.

¹³ [Chasles, 1852], p.xxxvii.

¹⁴This claim is recurrent throughout all of Chasles' historical works. An important example for Chasles is the relevance of MacLaurin's work on the attraction of ellipsoids, see [Chasles, 1837], pp.164-170.

the few extant accounts of Euclid’s *Porisms*¹⁵ are the “germs”¹⁶ of some of the theories at the bedrock of modern Geometry, but in their original settings they were not fully developed yet, insofar as they were not part of a general doctrine. This lack of generality, for Chasles, is measured not so much by the extension of these theorems of the past, but by the absence of any systematic method of deduction. Recall Chasles’ pious wish at the end of the *Aperçu Historique* that modern geometers, finally equipped with the proper methods and principles, be able to mechanically combine truths and multiply them to no end, and this without any genius¹⁷. To help these germs flourish and bloom into fully-formed bodies of doctrine, such is the stated goal of Chasles’ teaching of higher geometry.

To do so, Chasles explains, one must first seek the germs from past and modern mathematical texts, and second bind them together within a coordinated and logical chain of deductions, that is say a unified and methodical framework¹⁸. Note that the relationship between ancient and modern mathematical texts goes both ways for Chasles: ancient texts yield results that form the basis on which to build general theories, while modern theories shed a new light on past texts and reveal their full meaning¹⁹. Consequently, Chasles devoted the bulk of his inaugural lecture to an analysis of the contributions to geometry made by mathematicians from “Ancient Greece, the Middle Ages and the Modern Era, and the 19th century” - with a heavy focus on French authors for the most recent times. This historical narrative in three stages echoes that already expressed in the *Aperçu Historique*, discussed in the previous chapter. However, some major changes in focus, and a reevaluation of the relative importance of certain authors are to be noticed. In particular, whereas Gaspard Monge was presented in the *Aperçu* as the central actor of the renewal of pure geometry at the onset of the 19th century, he is now joined by Lazare Carnot, an author who was only placed among the cohort of Monge’s followers in 1837. While it is not our aim here to discuss in full detail the variations in Chasles’ historiography of geometry, some of the broad historical and epistemological theses expressed in this inaugural lecture illuminate the general project conducted through this course, and the corresponding treatise. Consequently, we now turn to the content of this lecture - whose

¹⁵Euclid’s lost *Porisms* are a text of great importance for Chasles’ interpretation of Greek geometry, from the 1837 *Aperçu* to his later studies. In 1860, he published a reconstitution of these lost texts, based on Pappus’ commentary and his own modern geometrical methods. We will come back to these texts later in this chapter.

¹⁶The metaphor of the germ is omnipresent throughout Chasles’ writings. See [Chasles, 1860], p.13, but also [Chasles, 1837] in many instances. Geometrical theories, for Chasles, grow naturally and effortlessly from proper concepts and methods; they are not merely the additive results of series of deductions and computations.

¹⁷ [Chasles, 1837], p.269

¹⁸“Ces théories et ces méthodes une fois déterminées, il faudra les coordonner entre elles, et les soumettre à l’enchaînement logique”, [Chasles, 1852], p.xxxviii.

¹⁹For more on this view of Chasles’, see [Chasles, 1860], p.13.

historical accuracy we do not pretend to evaluate.

In the first section of this lecture, Chasles opens with a discussion of Ancient Greek geometry. Unlike the first section of the *Aperçu*, his focus is on the modes of reasoning that one can find in these authors rather than on listing individuals and their respective contributions to the advancement of science. Greek geometry is then said to be composed of three parts, namely elementary geometry (also known as the *Eléments*), practical geometry (*Geodésique*), and higher geometry (*Lieu Résolu*). As it is thought to be the part of Greek geometry which enables geometers to actually solve problems and prove propositions, the latter is of particular interest to Chasles, who then proceeds to describe it in more detail, despite the relative lack of extant sources. Chasles gathers the corpus forming this part of Ancient Geometry from Pappus' *Collections*, and in particular Book VII²⁰. Among this corpus one can find Euclid's *Data*, and perhaps more importantly for what follows, his lost *Porisms*, as well as texts by Apollonius, Aristaeus, and Eratosthenes. Note that the Greek word standing for what Chasles calls "lieu résolu" is *Αναλυόμενος*, which modern-day commentators translate rather as "Domain of Analysis"²¹. Chasles adds that this domain of geometry has been called "geometrical analysis" since the modern Era²², despite the fact that the word "analysis" had a precise meaning for Ancient geometers, and in particular Pappus²³. This original meaning, says Chasles, is not to be mistaken for what was understood as "analytical geometry" at the time of his lectures²⁴. Consequently, Chasles gives his own translation of the beginning of Book VII, where the terms "analysis" and "synthesis" are defined by Pappus²⁵:

Le *Lieu résolu* est une matière à l'usage de ceux qui, possédant les *Eléments*, veulent acquérir en Géométrie l'art de résoudre les problèmes : c'est là son utilité. [...] On y procède par voie de *résolution* et de *composition*.

La *résolution* est une méthode par laquelle, en partant de la chose que l'on cherche et que l'on suppose déjà connue, on arrive, par suite de conséquences, à une conclusion sur laquelle on s'appuie pour remonter, par voie de *composition*, à la chose cherchée. En effet, dans la *résolution*, nous regardons comme fait ce que nous cherchons, et nous examinons ce qui découle de ce point de départ, et même ce qui peut en être l'antécédent, jusqu'à ce que nous arrivions par le raisonnement à quelque vérité déjà connue ou mise au nombre des principes.

²⁰For a critical edition and translation of this text, see [Pappus, 1986].

²¹[Pappus, 1986], p.82 *et passim*.

²²[Chasles, 1852], p.xxxviii.

²³For a more recent appraisal of what analysis meant for Greek geometers, a good starting point is [Sidoli & Saito, 2012].

²⁴For Chasles, what 19th-century mathematicians called "analyse" is "the use of *algebraic calculus*", while "synthèse" refers to "the sole consideration of the properties of figures, with the help of natural reasoning", see [Chasles, 1852], p.xli.

²⁵For a modern translation in English, see [Pappus, 1986], pp.82-84.

Cette marche constitue le procédé qu’on appelle *Analyse*, comme qui dirait *solution en sens inverse*.

Au contraire, dans la *composition*, nous partons de cette vérité à laquelle nous sommes parvenus, comme dernière conséquence, dans la *résolution* ; et, en suivant dans le raisonnement une marche inverse de la première, c’est-à-dire en prenant toujours pour antécédent ce qui, dans le premier cas, était conséquent, et réciproquement, nous parvenons enfin à la chose cherchée. Cette marche constitue le procédé qu’on nomme *synthèse*.

To this distinction in the logical structure between the methods of analysis and synthesis corresponds a distinction in their respective roles in mathematical practice. Analysis, Chasles claims, can only be useful when solving problems or proving previously-found propositions, as it requires a known thing to start with. This known thing can either be the proposition to be proved, from which the mathematician attempts to go back to some already proved proposition, or the solution of the problem, which is viewed as a known thing, and on which the mathematician reasons so as to discover its properties. Therefore, only when solving a problem does analysis function as a “method of invention”, and even then, only in a rather weak sense. Synthesis, on the other hand, is deemed by Chasles to be the “[sole] method of invention” capable of accruing our knowledge of a given science. This may seem rather surprising: it goes against many past discussions of Ancient Greek geometry, such as Descartes’ famous conjecture that the parts missing from the Greek corpus were those that allowed past mathematicians to actually discover new results, as opposed to the rather sterile teachings of landmark synthetic treatises, among which of course stand Euclid’s *Elements*²⁶. However, such opinions had already been expressed by actors crucial to the renewal of geometrical methods in the first decades of the nineteenth-century. Joseph Diez Gergonne, editor of the *Annales de Mathématiques Pures et Appliquées* from 1810 to 1832, through which he promoted (and altered) the new geometrical methods of the likes of Poncelet and Plücker²⁷, had penned in 1817 a philosophical essay on Analysis and Synthesis in mathematics²⁸. Arguing against the dogma of Analysis promoted by the Condillac-inspired *sect of Idéologues*, Gergonne contended that “Synthesis, even more so than Analysis, is a method of invention²⁹”. For Chasles,

²⁶See [Descartes, 1998], p.87-89.

²⁷ [Lorenat, 2015b].

²⁸ [Gergonne, 1816-1817]. For a detailed study of this paper, as well as of an unpublished, longer manuscript by Gergonne on this topic, see [Dahan-Dalmedico, 1986].

²⁹ [Gergonne, 1816-1817], p.354. Note, however, that Gergonne uses this comparison to rethink the relation between Algebra and Analysis, and, eventually, to promote the generality of Algebra against the limitations of synthetic geometry, understood as Euclidean geometry. Chasles’ goal is different: he wants to show that pure Geometry can capture, through the help of appropriate methods and concepts, the same level of Generality, and that the apparent shortcomings of pure Geometry are not an intrinsic flaw of synthetic methods.

therefore, these two modes of reasoning are thus depicted as perfectly complementary: synthesis is also the adequate mode of exposition of the elements of a given science, and of its principles, while analysis suits the exposition of problem-solving methods³⁰.

Nonetheless, as Chasles notes, scarce are the extant texts that display the “Ancients’ geometrical analysis” in action. Most of the texts listed by Pappus were lost or only partially recovered. Amongst these, Euclid’s *Porisms* particularly caught Chasles’ attention. While only a few paragraphs by Pappus and Proclus have survived through the centuries to tell us of these porisms, several mathematicians ventured to guess the geometrical content of these texts. One of them, Robert Simson, was particularly successful in this endeavor. Chasles claims that Simson was able not only to recover the very definition of the term “porism”, but also the “peculiar form” of the propositions thus named, by explaining 6 or 7 statements out of the 30 that Pappus transmitted to us, in rather obscure and laconic terms³¹. Simson’s work is yet to be built upon, Chasles adds, as he failed to explain “the reason for the eminent utility [of these propositions] in the Geometer’s craft”. Despite its limitations, Chasles found Simson’s work to be sufficient evidence to assert that Euclid had reached propositions which would not be out of place in a modern textbook, in large part due to their “peculiar form”. While Chasles did not make explicit this form in his opening lecture, he had previously defined porisms in the *Aperçu Historique* in the following manner³²:

Le porisme est une proposition dans laquelle on annonce pouvoir déterminer et où l’on détermine effectivement certaines choses ayant une relation indiquée avec des choses fixes et connues, et avec d’autres choses variables à l’infini: celles-ci étant liées entre elles par une ou plusieurs relations connues, qui établissent la loi de variation à laquelle elles sont soumises.

In Chasles’ understanding of Greek geometrical practice, porisms are an intermediary form of propositions between theorems and problems. Theorems are propositions where one has to prove a known and stated fact, whereas problems are propositions where one must discover an unknown thing. Porisms, however, are propositions where one must simultaneously prove a stated truth and discover the nature (“manière d’être”) of some things mentioned in the statement of this truth³³. In 1846, Chasles was not in possession of a complete interpretation of these texts: in the second half of this chapter,

³⁰ [Chasles, 1852], p.xlii.

³¹ [Chasles, 1852], p.xliv.

³² “The porism is a proposition wherein we announce that we can determine, and we effectively determine, certain things having a stated relation with fixed and known things, and with some other variable things at infinity: these being linked to one another by one or several known relations, which establish the law of variation to which they are submitted”, [Chasles, 1837], p.13.

³³ [Chasles, 1860], pp.32-36.

we will discuss his 1860 extensive “divination” of the meaning of these lost texts. In 1837 already, however, he understood them to be the basis for a geometrical analysis with a power comparable to that of algebra: porisms, Chasles said then, are “in Euclid’s mind, in a way, *equations* of curves³⁴”. From this comparison, Chasles concluded that the Ancients’ analysis must serve as the basis on which to build a new higher geometry: the form of their propositions was to be restored within unified and systematic exposition³⁵. Notably, Chasles’ higher geometry is not synthetic: it is self-avowedly analytical, in the historical sense of the term. However, it is an analytical geometry without Algebra.

2.1.2 Concrete and abstract geometry

Chasles’ historical narrative continues as follows: as Western science went into decline and medieval obscurity, the Arabs kept mathematics alive by translating and copying a vast number of texts. In so doing, they acted as a melting pot for Greek and Indian sources alike. While the former displayed a certain character of abstraction and was devoid of any form of calculation, Indian mathematics revolved mainly around Algebra and computations³⁶. As Western scholars regained access to Classical geometrical texts through the Arabs during the Renaissance, they started using algebra to solve geometrical problems. Nonetheless, this algebraic practice was still imperfect, claims Chasles: in the works of the so-called Italian algebraists (Scipion Ferro, Cardano, etc.), the only purpose for the use of signs is to represent the unknown whose value is sought. Thus, the algebraic operations performed by these mathematicians only apply to concrete numbers. Unlike them, according to Chasles, Vieta created a “science of symbols³⁷”, the “logistique spéceuse”, which soon became “l’instrument propre de la marche *analytique*”. In so doing, he had made algebra – and its application to geometrical problems – abstract.

A large part of Chasles’ argument here aims at reiterating criticisms he had made in 1841 against the historical theses of Italian mathematician (and *académicien*) Guglielmo

³⁴ [Chasles, 1837], p.276. In a review of Chasles’ 1860 book on Euclid’s lost *Porisms*, Chasles’ pupil De Jonquières wrote: “Cette forme de théorèmes non complets, c’est-à-dire débarrassées de déterminations parfois compliquées et sans utilité, tend à devenir le caractère le plus général des propositions dans les mathématiques actuelles ; [...] il y a, à cet égard, une analogie incontestable, qu’on était loin de soupçonner, entre les Porismes d’Euclide et la plupart de nos propositions modernes ; [...] cette modification *dans la forme des énoncés* est, à elle seule, un progrès réel ; car la science y trouve un degré de simplicité et d’abstraction qui facilite le raisonnement et la combinaison des vérités mathématiques entre elles ; et ainsi [...] elle prouve chez Euclide, non point un caprice inutile, mais au contraire une rare sagacité et une profonde intelligence des besoins de la science”, [de Jonquières, 1861a], p.6. Underlining in the original text.

³⁵ [Chasles, 1852], p.xlvii.

³⁶For a broader-scope history of the distinction between Greek and Indian mathematics in nineteenth-century historiography of mathematics, see [Smadja, 2015].

³⁷ [Chasles, 1852], p.liv.

Libri³⁸. The latter, in his 1838 *Histoire des sciences mathématiques en Italie de la Renaissance au XVIII^e siècle*, had placed an emphasis on Fibonacci’s innovations whilst recounting the history of algebra. In a passage quoted extensively by Chasles amidst their controversy, he had stated that Fibonacci “denoted [quantities] by a single letter, on which he then carried out *algebraic operations* as if they were abstract quantities, *absolutely in the same way as it is done today*³⁹”.

To that claim, Chasles replied that it rested on a confusion between two very different things: “reasoning on letters”, and “computing on or represented by letters⁴⁰”. What Fibonacci did is not properly called algebraic operations, Chasles contended, but merely a reasoning on letters. Reasoning on letters was not even an innovation, as the texts of Euclid, Diophantus, or Stifel show. A simple example allows Chasles to reject any claim of an invention of algebra by Fibonacci: when the latter had to express the identity which we now write $a^2 + b^2 = ab(\frac{a}{b} + \frac{b}{a})$, he introduced two auxiliary letters d and g to represent the product of the two given quantities a, b and the sum of their ratio, effectively writing $a^2 + b^2 = dg$, where d, g are as described above. This, according to Chasles, proves that Fibonacci merely reasoned on letters but could not compute with them. Only at the end of the 12th century did mathematicians such as Jordanus Nemorarius create some of the first algebraic notations, as he got rid of the need for introducing letters other than those that represent the given data of the problem⁴¹. However, all this was still a far cry from Vieta’s true innovation, which Chasles describes as follows⁴²:

Ce n’est donc pas là ce qu’on attribue à Viète; ce n’est ni d’avoir raisonné sur des symboles abstraits, ni même d’avoir exprimé les *inconnues* d’une question par des lettres, comme le dit M.Libri. Mais on lui attribue d’avoir substitué, dans le *calcul algébrique*, des lettres aux quantités *connues* qui, jusque-là,

³⁸ [Chasles, 1841c], [Chasles, 1841b], [Chasles, 1841a]. While Chasles’ historical take seems partly motivated by nationalist interests and institutional strategies, and as such (as is the case with a large part of his historiography) could be deconstructed on such grounds, it remains relevant for discussing the epistemological themes underlying the construction of his Higher Geometry. Therefore, the chauvinistic accents of this controversy will not be discussed here, nor will the sociological background of Libri’s uneasy institutional seating in the Parisian mathematical society. See [Ehrhardt, 2011] for more on this last point.

³⁹See [Libri, 1838], vol.2. The part on Fibonacci covers pp.20-44, and the passage quoted by Chasles can be found pp.33-34.

⁴⁰“Le raisonnement sur des lettres, et le calcul exécuté ou figuré sur des lettres”, [Chasles, 1841c], p.742.

⁴¹Very little is known of the life of Jordanus Nemorarius, and even his dates of birth and death are subject to much debate. For instance, in the second volume of his *Vorlesungen über Geschichte der Mathematik*, and *pace* Chasles, Moritz Cantor presents him as a 13th-century, Germanic author [Cantor, 1900], pp.53-59.

⁴² [Chasles, 1841c], p.747. Underlining in the original text. Chasles also wrote extensively on the history of algebra in India, and put forth several influential conjectures, both on the circulation of algebraic knowledge and on the connection between algebraic procedures and geometrical constructions. See [Smadja, 2016], especially pp.258-282.

avaient été exprimées en *nombres*; c’est donc d’avoir, le premier, figuré des *calculs virtuels* avec des *lettres*, calculs qu’on ne savait faire qu’avec des *nombres*; c’est d’avoir créé les *expressions* et les *formules algébriques* et cet art des *transformations* qui équivalent à de longs et pénibles raisonnements auxquels l’esprit humain ne pourrait suffire, enfin c’est d’avoir créé ce qu’on appelle aujourd’hui l’*algèbre* ou *calcul des symboles*.

One of Chasles’ main sources and allies in the fight for the rehabilitation of Vieta’s achievements, and in arguing for the Frenchman’s priority with regards to the creation of algebra, is English historian Henry Hallam (1777-1859), and in particular his *Introduction to the Literature of Europe*, published in four volumes starting in 1837. In the second volume of said *Introduction*, published in 1839, Hallam devoted his eighth and final chapter to what he called the *History of Physical and Miscellaneous Literature from 1550 to 1600*. This chapter opens with a discussion of the progress of algebra in this period⁴³. Hallam, a well-connected Whig liberal, was also a fellow of the Royal Society. It comes therefore as no surprise to see him quote Peacock’s definition of algebra as “the science of general reasoning by symbolical language⁴⁴” in this section, which he borrows from the famous 1830 *A Treatise on Algebra* by Peacock⁴⁵. In this much discussed text, as well as in a later *Report on the Recent Progress and Present State of certain Branches of Analysis*⁴⁶, Peacock responded to long-lasting discussions regarding the epistemic status of negative numbers, proposed a redefinition of algebra as a science, and of its relation to Arithmetic⁴⁷. In particular, Peacock argued against the idea that algebra was a *generalisation* of arithmetic, by noting that is impossible to derive the general laws of algebra, such as the subtraction operation, from the principles of arithmetic. In other words, from the principles that rule the subtraction of a smaller number from a larger number, it is impossible to derive the algebraic laws that rule the sign ‘-’ in general. For Peacock, algebra is rather to be thought of as an *abstraction* from arithmetic, with the latter serving merely as a “science of suggestion⁴⁸”: if two arithmetical expressions (or, to write like

⁴³ [Hallam, 1839-1840], p.274-280.

⁴⁴ [Hallam, 1839-1840], p.277

⁴⁵ [Peacock, 1830], p.1.

⁴⁶ [Peacock, 1834].

⁴⁷For a detailed analysis of Peacock’s conception of Algebra, and its diffusion in British mathematical circles, see [Pycior, 1981]. For a suggestive analysis of conflicting attitudes towards these conceptions of algebra in sociological terms, comparing political stances (Whig reformers such as Peacock *versus* protestant conservatives such as Hamilton) and epistemologies of Algebra, see [Bloor, 1981]. Peacock’s conception of Algebra, through the mediation of Hermann Hankel, would prove extremely influential for the philosophy of mathematics of Hermann Schubert, to which we shall turn in section 8.2.4.

⁴⁸ [Peacock, 1834], p.198. The question of the arbitrariness of formulas and algebraic forms arose in this context, culminating with the discovery of Hamilton’s quaternions. Geometers such as Chasles did not seem to consider this line of thought as relevant for their own subject, as the necessities of the extension seem to prevail over the indetermination of symbolic scriptures. However, as we shall see in chapters 4

Peacock, “forms”) are equivalent, then they will remain equivalent when read as purely symbolic expressions, that is to say algebraic expressions. This does not mean that there is an intrinsic necessity for these forms to remain equivalent. On the contrary, the laws of symbolical algebra are formed in this manner because arithmetical algebra is chosen to be a “subordinate science” of the former⁴⁹.

This new conception of Algebra was not only appealing to Chasles because it could be levied in a polemic against Libri, or for its purely philosophical merits. In fact, Peacock’s theses, or at least Chasles’ reading and understanding thereof, echoed strongly ideas already expressed in the *Aperçu Historique*. Through Hallam, Chasles got from Peacock the idea of distinguishing between a concrete science of numbers, and an abstract science of symbols, in a way that mirrors the distinction between a concrete and an abstract geometry. Whilst discussing Archimedes’ method of exhaustion, for instance, Chasles wrote⁵⁰:

La méthode d’exhaustion, qui reposait sur une idée mère tout à fait générale, n’ôta point à la Géométrie son caractère d’étroitesse et de spécialité, parce que cette conception y manquant de moyens généraux d’application, devenait, dans chaque cas particulier, une question toute nouvelle, qui ne trouvait de ressources que dans les propriétés individuelles de la figure à laquelle on l’appliquait. [...]

Ces considérations, qui tendent à faire ressortir la différence du *spécial* au *général*, du *concret* à l’*abstrait*, qui distingue la Géométrie jusqu’au XVe siècle, de la Géométrie postérieure, nous portent à regarder cette première époque comme formant les *préliminaires* de la science.

Concrete geometry, for Chasles, is that which is not founded upon general principles. Archimede’s method of exhaustion is one such example: while general in that it can be applied to a wide range of cases, it requires a special construction for each figure. The geometer must work with the figure before their very eyes, and the reasoning does not operate at an abstract level. In Chasles’ historical reconstruction, to this concrete geometry of diagrams succeeded an abstract geometry. Crucial to these transitions, for Chasles, were the works of Vieta and Kepler. Interestingly enough, Chasles’ narrative for the rise of abstract geometry was not one of pure cumulative progress. He notes that, “for Greek geometry to become perfect, it had to go through a state of alteration, which made this

and 7, similar concerns appear when certain characteristic properties, used to create general objects, are shown to cause unwanted results to hold. While the introduction of arbitrariness or conventionality in geometry is usually more associated with non-Euclidian geometries, it actually played a key role in the debates surrounding Chasles’ enumerative geometry.

⁴⁹ [Peacock, 1834], p.200.

⁵⁰ [Chasles, 1837], p.52. Underlining in the original text.

science lose its abstraction and its generality, and to descend to the rank of concrete and numerical operations⁵¹”, and refers to medieval Arab geometry (in particular, Mohammed Ben Musa), and that of the Italian algebraists such as Fibonacci. The algebra of concrete numbers (which Peacock called arithmetical algebra) had been used by these authors to represent magnitudes and carry out computations without referring to diagrams, explained Chasles, thereby doing away with the generality of Greek proofs. By inventing literal algebra, Vieta was able to preserve the import of literal modes of computation and reasoning, and to elevate them to a higher level of abstraction. Through the use of this “universal instrument”, the geometer is able to reason on the intersection of a circle and a line in an abstract fashion, that is to say unfettered by the requirement of separating between each possible concrete case.

In 1837, Chasles had devoted but a few lines to Vieta’s algebra, which he merely described as a “complement to Plato’s analytical method⁵²”. In 1846, he would make this connection between the Ancients’ analysis and Vieta’s algebra more explicit. By introducing indistinctly known and unknown quantities into an abstract symbolism, Vieta had created an instrument to make this geometrical science of the Ancients uniform and systematic. This is not to say that Chasles attributes to Vieta the creation of “analytical geometry”, a term whose polysemy he denounced. For Chasles, this creation has to be attributed to Descartes, who used Vieta’s instrument to represent lines and curves. In fact, to mark the difference between the two, Chasles suggests a distinction between “analytical geometry” and the Ancients’ “geometrical analysis”. This is crucial, as “L’*Analyse géométrique* des anciens [doit] être la base de notre Géométrie supérieure⁵³”.

Of course, Chasles was not the first French mathematician and historian of mathematics who had commented on Vieta’s works, and the break they represent with past algebraic practices. Famously, Lagrange had already done so in his *Traité de la Résolution des Équations Numériques*⁵⁴. For Lagrange, the difference between arithmetic and algebra was that “the latter’s operations do not provide individual values for the quantities that are being sought, like those of arithmetical operations or geometrical constructions, but merely represent the operations, either arithmetical or geometrical, which have to be carried out on the first quantities to obtain the values being sought”. Vieta is then being credited as having shown that geometrical constructions can be put under the perview

⁵¹ [Chasles, 1837], p.542.

⁵² [Chasles, 1837], p.52. Vieta’s work on triangles is described in much greater detail, and disappears completely in later historical narratives of Chasles’, thus showing once more the evolution in historical and epistemological concerns.

⁵³ [Chasles, 1852], p.xlviii. Underlining in original text.

⁵⁴ [Lagrange, 1826], pp. xxi-xxiii. For more on Lagrange as a historian of mathematics, and on the role of historical studies in his teaching and practice of algebra, see [Wang, 2017], pp.57-84, [Richards, 2006], pp.706-711.

of algebra in the same way as arithmetical operations can. For Lagrange, algebra is a system of operations putting in relation known and unknown quantities, reigning over several fields of application such as geometry and arithmetic.

On the contrary, through this 1846 opening lecture, Chasles presented Vieta's achievement as the development of a tool for the abstract and easy expression and proving of propositions of higher geometry, or geometrical analysis. Chasles' project for higher geometry was one that would pursue Vieta's crucial move, by developing a new geometrical analysis on the basis of instruments other than that of Vieta's algebra. To rival the results and generality of analytical geometry, another step towards abstraction must be taken: it was necessary to develop a new instrument, that is a new geometrical language. For Chasles, this very possibility had been opened a few decades prior by Monge and Carnot, who are identified as the key actors of a renewal of geometry at the onset of the 19th century. Indeed, as Chasles concludes his historical overview with a third stage, the two French geometers are now given the front role in the following terms⁵⁵:

Les ouvrages qui, au commencement du siècle présent, ont eu une heureuse influence sur la marche et les progrès de la Géométrie sont, à des titres différents, ceux de Monge et de Carnot. [...]

Ces ouvrages, en agrandissant les idées, en inspirant aux jeunes mathématiciens le goût des recherches de bonne Géométrie, leur offraient des méthodes et des ressources nouvelles.

Chasles continues by crediting Monge with the invention of "Géométrie descriptive", a science which aims to "represent on a plane, which is a two-dimensional surface, bodies which have three dimensions; in other words, to gather in one plane figure all the necessary elements to make known the shape and position in space of a three-dimensional figure". Here again, Chasles emphasizes a contribution to geometry in the form of a new technique for representing complex figures in simpler terms. Already in the *Aperçu Historique*, Chasles' appraisal of the works of Monge insisted on the changes they had brought to the language of geometry⁵⁶:

Cette influence utile de la Géométrie descriptive s'étendit naturellement aussi sur notre style et notre langage en mathématiques, qu'elle rendit plus aisés et plus lucides, en les affranchissant de cette complication de figures dont l'usage distrahit de l'attention qu'on doit au fond des idées, et entrave l'imagination

⁵⁵"At the onset of the present century, the works which had a most fortunate influence on the march and progress of Geometry are, in many respects, those of Monge and Carnot. [...] These works offered young mathematicians new methods and resources, while awakening in them a taste for research in good Geometry", [Chasles, 1852], pp.lxxi-lxxii.

⁵⁶ [Chasles, 1837], p.190.

et la parole. La Géométrie descriptive, en un mot, fut propre à fortifier et à développer notre puissance de conception; à donner plus de netteté et de sûreté à notre jugement; de précision et de clarté à notre langage; et, sous ce premier rapport, elle fut infiniment utile aux sciences mathématiques en général.

Mirroring his description of Vieta’s move from numbers to symbols, Chasles presents Monge’s geometry as the introduction of new literary techniques for geometrical reasoning, thus pushing for a shift in focus from figures to pure reasoning. In both cases, this clarification and improvement of mathematical language is correlated with a step from concreteness towards abstraction. Moreover, Chasles crucially praises Monge for “binding [a variety of questions] to a small number of abstract and elementary operations⁵⁷”.

However, Chasles’ addition in 1846 of Carnot to what is identified as a turning point in the history of geometry is a strong departure from the narrative dominating the 1837 *Aperçu Historique*, whose fifth “époque” opened with a zealous praise of “Monge’s creative genius⁵⁸”. In this first book, Chasles presented Carnot as merely one amongst many who had followed the path opened solely by Monge, alongside Dupin and Brianchon⁵⁹. In his 1846 opening lecture, however, Chasles attributes equally to Monge and Carnot the crucial leap towards the renewal of geometrical methods⁶⁰, and describes Carnot’s contribution as having “the special purpose of expanding upon the Ancients’ Geometry⁶¹”. Reiterating his judgement on the lack of generality of the propositions one can find in the Ancients’ geometry, Chasles then discusses Carnot’s “principe de corrélation”, which was hardly mentioned in the *Aperçu Historique*. He now finds it to be an important step towards generality, as Carnot showed that a proof valid for a single configuration of the figure can yield proofs for all other configurations through sign changes⁶². This contribution is then described in striking terms⁶³:

⁵⁷“On n’avait pas songé à rattacher toutes ces questions à un petit nombre d’opérations abstraites et élémentaires, et surtout à présenter celles-ci dans un traité spécial et sous un titre particulier, qui leur donnât un caractère de doctrine indépendant des pratiques d’où il avait suffi de les faire sortir. C’est là ce que Monge a conçu”, [Chasles, 1852], p.lxxii.

⁵⁸ [Chasles, 1837], p.189.

⁵⁹ [Chasles, 1837], p.210-213.

⁶⁰Chasles would remain faithful to this new narrative. In his 1870 *Rapport sur les progrès de la géométrie*, one can read: “Geometry, this primordial part, this basis of mathematics, nowadays presents two different directions with regards to methods, which stem mainly from the works of Monge and Carnot”. See [Chasles, 1870], p.2.

⁶¹“Les ouvrages de Carnot ont pour objet spécial l’extension de la Géométrie des Anciens”, [Chasles, 1852], pp.lxxvi-lxxvii.

⁶²The reevaluation of Carnot’s “principle of correlation” plays a crucial role in the geometrical theory of homography at the heart of Chasles’ higher geometry. We will come back to the more technical details below. For a thorough study of Carnot’s principle of correlation and the value of generality in early 19th-century geometry, see [Chemla, 1998]

⁶³“This way of writing Geometry characterizes modern Geometry, and it is Carnot’s works which

Cette manière d'écrire la Géométrie fait le caractère de la Géométrie moderne, et ce sont les ouvrages de Carnot qui ont le plus contribué à la répandre.

The shifts in Chasles' historical narratives are strongly correlated to shifts in epistemological concerns. The mode of reasoning, the Ancients' "geometrical analysis", the techniques for the expression of abstract geometrical knowledge and proofs, are all themes which gained a much larger role in Chasles' *épistémologie historique*. Topics such as notations were hardly discussed in 1837⁶⁴, while by 1846 they had become a rather important historiographical focus. Consequently, the roles of Vieta and Carnot in the development of geometrical methods are reevaluated positively, thus betraying Chasles' growing attention to the language of geometry⁶⁵.

This shift is of great importance, as it occurs as Chasles is turning to teaching his own material and methods to advanced students at the Faculté de Paris. As recent scholarship has convincingly shown, the turn to professorship has, in a lot of cases, caused profound changes in the way mathematicians understood, practiced, and wrote their science⁶⁶, and is as much a knowledge-producing process, as it is a knowledge-circulating one – if not more. This is particularly true for Chasles, whose *Géométrie Supérieure* purports not so much to add to a previous body of knowledge, but to change its structure and its exposition. To describe the reform of the language of geometry that Chasles deemed necessary for the modernization of geometrical analysis, such is the task we shall tackle in the rest of this section.

Chasles' 1852 *Traité de Géométrie Supérieure* is divided into four *Sections*, only the first of which will be directly discussed in the present chapter. Indeed, the first *Section* expounds, as Chasles puts it, "a set of propositions whose natural sequence gives rise to three theories which follow each other and form the development of a single notion and a single fundamental theorem⁶⁷". This notion is that of the cross-ratio; and these three theories are respectively the theory of the cross-ratio, the theory of homographic divisions

contributed the most to spread it", [Chasles, 1852], p.lxxvii

⁶⁴A rare exception is a discussion of the "Geometry of Indians", [Chasles, 1837], p.418. See [Smadja, 2016] for a detailed study of this passage.

⁶⁵This theme is not entirely new: Lacroix, in his pedagogical essays, had also emphasized the importance of linguistic transformations in the development of a science. See [Lacroix, 1838], pp.177-178, "to educate students in the ways of [historical] authors, even the most celebrated of them, must cease because since they wrote, the science has made progress which has completely changed the connections of the propositions and often their language".

⁶⁶The ways in which teaching impacts mathematicians' scientific practice and theoretical outlooks has been the object of renewed interest by historians for several decades now, and has been a source of important results. For a general overview of the benefits to be reaped from this perspective, see the programmatic but still highly relevant [Belhoste, 1998]. For recent studies in this vein on sources and actors closer to us, see [Ehrhardt, 2009], [Gispert & Schubring, 2011], [Belhoste, 2014].

⁶⁷"La première [section] contient un ensemble de propositions dont l'enchaînement naturel donne lieu à trois théories qui se font suite et sont le développement d'une même notion et d'un même théorème fondamental", [Chasles, 1852], p.xviii.

and sheaves, and the theory of involution. All of these notions were already discussed in the 1837 *Aperçu Historique*, albeit in very different terms. Homographic divisions, for instance, only appeared once⁶⁸, while cross-ratios and involutions were not written with the notational conventions that Chasles would introduce in his teaching⁶⁹. We will not compare further the ways in which these notions appear in both books, in the interest of clarity and (relative) brevity. Our focus will rather be on Chasles’ systematization thereof, within what he considered to be a wholly methodical and unified framework for the expression and proving of the propositions of higher geometry. Sections 2 to 4, which respectively apply these theories to rectilinear figures, correlative figures, and circles, will not be discussed in detail. Furthermore, in expounding the fundamental theories around which the entire *Treatise* is built, we will only give the results and proofs related to the properties of points (or series thereof). However, the entire book is constructed so that each proposition has its dual, that is to say an analogous statement where points and lines are reversed. This is to lighten the exposition, as our goal is not to summarize the whole of Chasles’ *Higher Geometry* as a mathematical theory, but to present it as the construction of a literary technology to produce a geometrical knowledge that embodies the epistemological virtues described previously⁷⁰.

2.1.3 The abstract geometry of signed segments

A key component of Chasles’ literary technology for the expression of abstract geometrical knowledge is what he calls the “principle of signs”, which he not only discusses at length in the *Préface* of the *Traité*, but to which he also devotes the entire first chapter thereof⁷¹. For Chasles, this principle enables the gathering of several concrete results into one abstract proposition, but it also allows propositions to be stated in the most general way possible, and as such, to endow them with their full meaning⁷²:

L’usage explicite du principe des signes est souvent indispensable pour donner aux propositions leur signification complète et toute la portée qui est leur est propre, et à la science toutes ses ressources naturelles.

⁶⁸ [Chasles, 1837], p.844.

⁶⁹ [Chasles, 1837], compare p.302-307 for instance with the two subsections below.

⁷⁰For an exposition of the mathematical content of this book in terms more familiar to mathematically-trained readers, see [Nabonnand, 2006], pp.159-180.

⁷¹The introduction of negative quantities in geometry had been a hotly-debated topic amongst French mathematicians in the second half of the 18th-century, from Fontenelle to Carnot; see [Schubring, 2005], pp.99-113; 353-368; 410-419. Chasles was probably aware of these discussions to a certain extent; however, in his own introduction of negative segments, he does not mention any of the epistemic worries that these past authors may have had in that regard.

⁷² [Chasles, 1852], p.viii.

At its core, this principle is mostly a notational convention. A segment delimited by two points a, b can be denoted either ab or ba . In the first case, we convene to call a its origin; in the second case, we convene to call b its origin. Arbitrarily convening of a direction on the line on which this segment lies, which we regard as being positive, we can regard ab as being positive or negative depending on whether this (oriented) segment is in the same direction as the positive direction convened upon. Chasles then writes:

$$ab = -ba$$

From this convention follows what Chasles calls a “fundamental theorem”: for any three points a, b, c on a line, the sum of the segments ab, bc, ca is always zero, which translates into the following equation⁷³:

$$ab + bc + ca = 0$$

Chasles’ proof is as follows: a and b being given, the order of the three points a, b, c (with respect to the convened upon direction) can only be one out of three possibilities, as c can either be between a and b , left of both, or right of both. These configurations can be written as a, b, c ; c, a, b , or a, c, b . Any two configurations differ only by the permutation of two letters, which leaves the equation above unchanged. Indeed, if for instance a and b are switched, then ab becomes ba , bc becomes ac , and ca becomes cb . The equation is then $ba + cb + ac = 0$; multiplying this equation by -1 , i.e. to change the direction of every segment, yields the original equation. Therefore, explains Chasles, it suffices to show that the formula holds in one case only, which is easy enough to do, as for instance in the order a, b, c , we have $ac = -ca = ab + bc$. Of course, it wouldn’t have been hard to show that the formula holds in each case, but the whole point of the principle of the signs as a way of writing the equations of geometrical figures is that it removes the need for particular considerations and allows general proofs and statements of propositions. A few paragraphs later⁷⁴, Chasles gives a proof by induction of the same formula for n points.

Chasles then explains how this way of writing signed segments can be used to write coordinate changes. When a point a is determined by its distance with respect to a fixed

⁷³ [Chasles, 1852], p.2. It must be noted that Chasles writes both the sentence on the “sum of segments” and the equation. This equation is the reason why Chasles’ name is associated, at least in French secondary education curricula, to the analogous equation for vectors: $\overrightarrow{AB} + \overrightarrow{BC} = \overrightarrow{AC}$, where A, B, C are three points in an affine space. However, it is clear that this vectorial equation has little to do with Chasles’ principle of signs. The latter deals with colinear points, and is a notational convention. Furthermore, the concept of vector does not appear in Chasles’ *Higher Geometry* in the least. Recently, some scholars have argued (against Crowe’s 1975 *History of Vector Analysis*) that Chasles, through his reading of the work of his childhood friend Gaetano Giorgini, may have played a role in the emergence of this concept via articles on kinematics and mechanics, see [Crowe, 1967], [Koetsier, 1986], [Caparrini, 2003].

⁷⁴ [Chasles, 1852], pp.3-4.

origin O , to change the origin from O to another point O' on the line Oa , it suffices to use the fact that $Oa = O'a - O'O$. Furthermore, the principle of signs can be used to study the properties of middle points of segments. For any two points a, a' whose middle point is α , and for any point m on their common line, we have the following equations:

$$m\alpha = \frac{ma + ma'}{2}, \quad ma \cdot ma' = \overline{m\alpha}^2 - \overline{\alpha a}^2$$

Conversely, these equations defines the middle point α : given two points a, a' , it is the only point on the line aa' that satisfies the equations above for any point m .

These formulas are the first occurrences of a kind of proposition which will prove to be extremely important in the *Treatise*. What we have here is an equation that defines an object (here, the point α) by a relation involving an unknown quantity (here, point m) that remains constantly true whatever the value or position of the unknown is. In other words, two points are being *represented* by a point and a rectangle. The letters chosen here, while not commented upon, will retain the same roles in the rest of the *Treatise*, namely m will always be a variable point on a line, while a and a' always denote two points of a pair. Similar considerations are applied to angles, in accordance with the principle of duality. Note that Chasles uses overbars to denote the rectangle $\overline{m\alpha}^2$; this, however, does not mean that we are dealing with a new kind of segment, or with a positive, numerical length. Rather, the introduction of a bar to denote rectangles or higher powers of a single segment serves to mark the difference with algebraic notations. Despite the similarity between both notations (and the potential translations that can be provided between these), Chasles is still manipulating segments, and not algebraic quantities. It may be tempting to think that Chasles is trying to import notions from algebra, and to compare m with the unknown x of algebraic equations. In fact, for Chasles, it is rather the opposite: his m and the algebraists’ x are two different instruments for what he had called “geometrical analysis”.

To demonstrate the expressive power granted by this principle is a major concern of Chasles’. In Chapter II, §21, Chasles gives a striking example, starting with the following statement⁷⁵ :

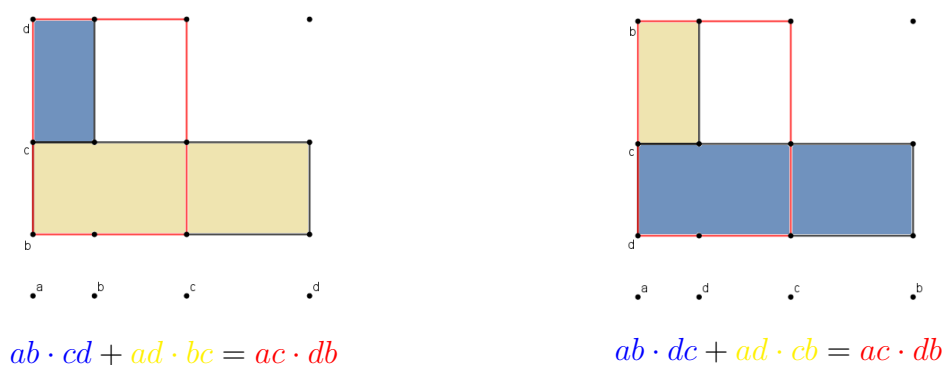
Étant donnés quatre points a, b, c, d en ligne droite, on a toujours entre les six segments que ces points déterminent deux à deux, la relation

$$ab \cdot cd + ac \cdot db + ad \cdot bc = 0$$

Chasles comments further, and explains that by passing from the “abstract form” of the

⁷⁵“For any four given points a, b, c, d on a straight line, the following relation between the six segments determined by pairs of points always holds: $ab \cdot cd + ac \cdot db + ad \cdot bc = 0$ ”, [Chasles, 1852], p.17.

theorem to its “concrete expression”, that is to say by fixing the relative position of the four points on the line and by looking at the equation stated above as expressing a relation between the numerical values of lengths of segments, the theorem states nothing more than a trivial property of three rectangles. Moreover, each permutation of the four points yields a different property of these rectangles (see fig. below). The principle of signs, thus, enables Chasles to state and prove all of these properties at once⁷⁶: one concise equation instead of a variety of ratios between numerical lengths, one abstract theorem instead of a variety of particular propositions. The principle does not merely shorten the wording of these propositions: it makes their unity appear, it shows the abstract truth behind all of the particular configurations, and enables the move from concreteness to abstraction. With the use of the terms “concrete” and “abstract”, Chasles echoes his previous theses



on the history of Algebra, and the distinction between Fibonacci’s and Vieta’s uses of letters. The letters used by Chasles in his equations do not represent numerical lengths or angles, but segments or angles directly. This shift is akin to that of arithmetical algebra to symbolical algebra. As such, the principle of signs is the first step toward a geometrical language that can capture the same level of generality as algebra. Rather than showcasing new results, Chasles is constructing a new language, the principle of signs playing the role of a fundamental grammatical rule therein. This is a radical departure from the *Aperçu Historique*, where signs were systematically stipulated and discussed⁷⁷. The strategy of importing the generality of (geometrical) analysis by recreating abstract variables within geometrical language, and the associated move from concreteness to abstraction, is a novelty which stemmed from Chasles’ renewed historiography of mathematics. Faithful to Comte’s positivist project, this course shows Chasles turning historical insights into a general and methodical doctrine.

⁷⁶The proof of this formula, while rather simple, derives from notions we will discuss below.

⁷⁷For instance, see [Chasles, 1837], pp.175-176.

2.1.4 Imaginary numbers made intrinsic

In fact, the generality and abstraction granted by the principle of signs goes further than unifying diverse configurations of objects. In Chapter V of the *Traité de Géométrie Supérieure*, Chasles uses this principle to introduce imaginary numbers in geometry. For any two points a, a' , of middle point α , Chasles lets M be a fixed point on the straight line joining a and a' , and ν be the product of the distances between the two points a, a' and M . This configuration translates into the following equation:

$$\overline{aa}^2 = \overline{Ma}^2 - \nu$$

Conversely, explains Chasles, “the determination of the two points depends on the construction of the expression⁷⁸ $\pm\sqrt{\overline{Ma}^2 - \nu}$ ”. As we already saw, the two points a, a' can be determined and represented by the point α and the rectangle ν . However, when ν is sufficiently large, the expression representing the points becomes imaginary since the argument in the square root is negative. Two real quantities thus determine two conjugate, imaginary points. As such, in Chasles’ geometry appear only pairs of conjugate imaginary points (or lines). Rather than postulating imaginary points from which ideal lines can be defined as Poncelet did, Chasles presents an intrinsic way of representing and manipulating pairs of points, from which the introduction of imaginary points naturally follows. When discussing Poncelet’s principle of continuity, in the *Préface* to this very *Treatise*, Chasles commented⁷⁹:

Mais il est une autre considération plus puissante [than that of question of rigor] qui m’a déterminé à ne pas profiter, dans ce Volume destiné à poser les bases de méthodes générales, des facilités qu’auraient pu offrir souvent le principe de continuité. Une étude attentive des différents procédés de démonstration qui peuvent s’appliquer à une même question m’a convaincu qu’à côté d’une démonstration facile, fondée sur quelques propriétés accidentelles ou contingentes d’une figure, devaient s’en trouver toujours d’autres, fondées sur des propriétés absolues et subsistantes dans tous les cas que peut présenter la figure en raison de la diversité de position de ses parties.

Indeed, by simply postulating imaginary objects and applying the principle of continuity to them, Chasles claims, Poncelet obtains proofs whose “facilité” obfuscates, or rather glosses over, the very property of the object that makes the proposition remain true whether or not some of its parts have become imaginary. The same goes for changes in

⁷⁸“La détermination des deux points dépend de la construction de l’expression $\pm\sqrt{\overline{Ma}^2 - \nu}$ ”, [Chasles, 1852], p.56.

⁷⁹ [Chasles, 1852], p.xv.

the sign of expression: when unifying the propositions deriving from the various relative positions of four points on a line, as discussed in a previous paragraph, Chasles did not resort to a principle of continuity. Rather, he founded his statement and proof upon the abstract expression of a property that subsisted whatever the relative positions of the points.

When discussing the properties of pairs of points, Chasles has two representational tools at his disposal⁸⁰. The first one is to use two real quantities, namely α and ν , which define the imaginary points with respect to one arbitrary point m which serves as origin for the definition of ν . Indeed, given α and ν , the distances between points a, a' and any origin m are given by the equations:

$$\begin{aligned} ma &= m\alpha + \sqrt{m\alpha^2 - \nu} \\ ma' &= m\alpha - \sqrt{m\alpha^2 - \nu} \end{aligned}$$

Note that, when defining pairs of imaginary points, Chasles used a capital letter M ; however, when proving properties of these pairs, he uses an equation with a variable point, denoted by a lower case m . Effectively, this is another way of writing the equation of an object, here a pair of points.

Another representational tool, perhaps more familiar to us but also to Chasles' contemporaries, is to consider the pair of imaginary points as the roots of an equation of the form $ax^2 + bx + c = 0$. When pairs of imaginary points arise as double points of an homographic division (defined and discussed below), they are regarded as the roots of an equation $\overline{Am}^2 + (\lambda + \mu)\overline{Am} + \nu = 0$, which is both an equation of the second degree and an equation that does not yet belong to Algebra, as it involves geometrical quantities and not algebraic variables. As we will see in the rest of this chapter, having several equations, or indeed, representational devices, for one object is something Chasles seeks fervently. It is rather common in the *Treatise* to have alternative proofs, as if to assess the respective strengths of several equational presentations. However, close to no comments are made on the role of these alternative descriptions of an object⁸¹.

One objection to this way of introducing imaginary points in geometry, rejected by Chasles later in the *Treatise*, must be mentioned. It seems that, in the course of a proof or a construction, we might end up manipulating meaningless expressions. For instance, assume a pair of points a, a' has been constructed, which sometimes are imaginary, and an

⁸⁰See for instance [Chasles, 1852], pp.58-59.

⁸¹"Du reste, on verra que cette égalité [de deux rapports anharmoniques] même ne s'exprime pas uniquement par une équation à deux termes, comme on pourrait le penser d'après la définition du rapport anharmonique, mais aussi par des équations à trois et à quatre termes, de formes variées, équations dont chacune a des applications spéciales fort étendues", [Chasles, 1852], p.xxiv.

equation of the form $\frac{2}{ef} = \frac{1}{ea} + \frac{1}{ea'}$ is obtained, for two fixed points e, f on the same line as a, a' ⁸². How can this equation be said to be generally meaningful? While this equation has “an explicit meaning” when a and a' are real, Chasles seems to be forbidden from considering isolated imaginary points. What, then, is the meaning of an expression such as $\frac{1}{ea}$, taken in isolation from the rest of this equation? Chasles’ answer is simply that, per their construction, imaginary numbers will only appear in symmetric equations. Therefore, it suffices to rewrite the equation as $\frac{2}{ef} = \frac{ea+ea'}{ea \cdot ea'} = 2\frac{ea}{ea \cdot ea'}$, wherein all elements are real and perfectly meaningful in their own rights. Chasles’ conclusion is that segments like $a'e$ ought to be considered merely as *symbols*, through which we only express relations of the elements that are always real (for instance, the point and the rectangle aforementioned). This would be considered to be a major weakness of Chasles’ conception of imaginary points by later commentators, including Gaston Darboux, Chasles’ successor as professor of *Higher Geometry*. In his own lessons on geometry, Darboux claimed that there were two methods to introduce imaginary points in geometry: the use of Descartes’ coordinates, or Chasles’ *Higher Geometry*, completed by von Staudt’s geometry of position. Chasles’ method, however, was plagued by one major problem⁸³:

Dans cette méthode, deux points imaginaires conjugués ne sont pas séparés. Cela n’a pas d’inconvénients dans beaucoup d’applications où ces éléments entrent de manière symétrique. Cependant, le rapport anharmonique de quatre points imaginaires sur une droite ne peut être ainsi défini.

That Chasles considered and yet rejected this objection shows how his epistemological concerns differ from that of later generations of mathematicians. He is not creating a geometrical theory of complex spaces: he is crafting a tool for the expression of abstract truths in the real plane, which may depend on imaginary parts. Chasles’ goal is not to integrate imaginary points to the collection of basic geometrical entities, but to write proofs in a general manner, that does not hinge upon contingent properties such as the reality or imaginary status of a point in a configuration.

To that end, he devotes considerable time comparing the expressive power of his instrument to that of Algebra⁸⁴. For Chasles, his own conception of imaginary points “conforms absolutely⁸⁵” to what is done in analytical geometry, with one major difference:

⁸²This equation is that of an harmonic ratio, see [Chasles, 1852], p.64.

⁸³“In this method, two conjugate imaginary points cannot be separated. This causes no inconvenient in many applications where these elements appear in symmetric manner. However, the cross-ratio of four imaginary points on a line cannot be defined in this way”, [Darboux, 1917], pp.2-3.

⁸⁴For instance, [Chasles, 1852], pp.57-58.

⁸⁵“Cette manière de considérer les imaginaires est tout à fait conforme à ce qu’on fait en Géométrie analytique. Mais ici les équations sont formées avec les données mêmes de la question, ce qui est le plus haut point de simplicité que l’on puisse désirer. En Géométrie analytique, au contraire, elles ont lieu entre des coordonnées introduites auxiliairement”; [Chasles, 1852], p.xxix. The image of Algebra with which

when Analysts introduce systems of coordinates to write their equations, they introduce artificial auxiliaries, with no direct link to the objects being constructed. In so doing, they obfuscate the reasons for the validity of a construction or a proof. On the other hand, the introduction of imaginary points that is used by Chasles in his *Higher Geometry* is “direct”, follows a “natural method”, and thus reaps the benefit of clarity, simplicity and intuitiveness. Consequently, the principle of signs must be understood as the first but primordial stepping stone for Chasles’ broader project to craft literary technologies to import the epistemic uses and benefits of equations, without the loss due to the introduction of extrinsic algebraic quantities⁸⁶.

2.2 A new language for (projective) geometry

2.2.1 Cross-ratios and (an)harmonic systems of points

These notational and definitional innovations are not the final end of Chasles’ geometry: their main purpose is to express in an abstract manner what Chasles, in 1846, took to be the central notion of the whole of geometry: “cross-ratios⁸⁷” (*rapports anharmoniques*). For any four points a, b, c, d on a line, their cross-ratio is defined in Chapter II as the quantity⁸⁸:

$$\frac{ac}{ad} : \frac{bc}{bd}$$

where the principle of signs is used to take into account the relative position of these four points. In fact, as Chasles explains, there are two ways to determine the sign of a cross-ratio. It is possible to use the principle of signs and determine the sign of each segment then compute the sign of the entire expression; or one can determine the sign of each ratio by looking at whether the segments composing it are in the same direction or not. These sign determinations coincide as far as the result is concerned, adds Chasles, but the first one is necessary when segments other than those present in the cross-ratio enter the geometrical proposition at hand, while the second method is swifter in cases

Chasles writes such sweeping statements is certainly not in touch with the most advanced of contemporary developments. Similarly, while presenting his theory of characteristics to the Paris Académie des Sciences in 1864, Chasles would make comparisons with Algebra which show that his familiarity with recent works was limited. However, one can safely assume that he had a passing knowledge of the textbooks written by Irish mathematician George Salmon, which he sometimes refers to and quotes; see 4.2.4.

⁸⁶A similar argument can be found in the *Aperçu Historique*: “en Géométrie pure, il faut remonter à une raison plus directe, prise de la nature seule de la chose, et non empruntée d’un système de coordonnées auxiliaire et artificiel”, [Chasles, 1837], p.119.

⁸⁷Already in [Chasles, 1837], Chasles stated that “[La fonction anharmonique] sera utile surtout dans la théorie des coniques, où elle montrera, entre une infinité de propositions isolées, une liaison et des rapports qui les rattachent toutes à un petit nombre de principes généraux”, p.35. In the *Aperçu Historique*, however, cross-ratios are given without the principle of signs.

⁸⁸ [Chasles, 1852], p.7.

where no other segments are to be considered.

Furthermore, four points determine six possible cross-ratios, depending on their order; three of these are the reciprocal of the other ones. Among these three, Chasles shows, there are always two and exactly two that are positive⁸⁹. Even though these multiple cross-ratios will play close to no role in the rest of the *Treatise*, taking signs into account when discussing cross-ratios is not an unimportant innovation. For instance, in the case of the harmonic ratio⁹⁰ ($\lambda = -1$), Chasles laments the fact that it is still customary to write this configuration through the equation $\frac{ae}{af} = \frac{a'e}{a'f}$, with the added hypothesis that the points are ordered in a manner such that an odd number of these segments are to be read as negative quantities. This practice, for Chasles, is a result of the habit of working “concretely”, with “the figure before our eyes⁹¹”. To capture the complete meaning of the harmonic ratio, continues Chasles, we must write the equation $\frac{ae}{af} = -\frac{a'e}{a'f}$, thereby allowing for the study of harmonicity of four points, irrespective of their relative positions.

While the first paragraphs of chapter II state elementary propositions pertaining to cross-ratios and are followed by proofs thereof, §11 of the second chapter reads⁹²:

Connaissant le rapport anharmonique λ de quatre points dont trois sont donnés de position, construire le quatrième.

Mimicking the language of Ancient Geometry, Chasles continues outlining another way to pursue the Greeks’ geometrical analysis in a modern and general manner, without resorting to Algebra. Indeed, instead of a proposition to be proven, Chasles gives a “problem” to solve. The construction that Chasles gives to solve this problem is the following (see fig. below): let a, b, c, d be the four points whose cross-ratio is λ , and suppose, for instance, that b is the unknown point. Then, from a , draw a line in any arbitrary direction and on this line form two segments $a\alpha, a\alpha'$ in ratio λ . Then draw the lines $\alpha c, \alpha' d$, and let β denote their intersection. The parallel line to $a\alpha$ drawn from β will determine the point b , as two pairs of similar triangles have been constructed: αac , βbc ; and $\alpha' ad$, βbd . This ensures that $\frac{ac}{bc} = \frac{a\alpha}{b\beta}$ and $\frac{ad}{bd} = \frac{a\alpha'}{b\beta}$. Dividing these equations, and using the ratio between $a\alpha$ and $a\alpha'$, Chasles can verify the fact that the cross-ratio between a, b, c, d is as required (see fig. below for this construction⁹³).

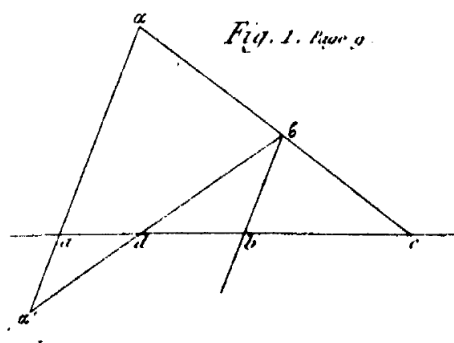
⁸⁹Let λ denote the first one, then the other cross-ratios are $\frac{1}{1-\lambda}$, and $\frac{\lambda-1}{\lambda}$. A cross-ratio cannot be equal to 1.

⁹⁰This term is usually attributed to the Pythagoreans, who formulated a theory of (musical) harmony in terms of series of numbers in such a ratio.

⁹¹ [Chasles, 1852], p.40.

⁹²“Knowing the cross-ratio λ of four points, three of which are given in position, construct the fourth”, [Chasles, 1852], p.10.

⁹³While there were almost no figures in the 1852 edition; many would be added in the second edition, published in 1880. This figure, for instance, only appears in the second edition.



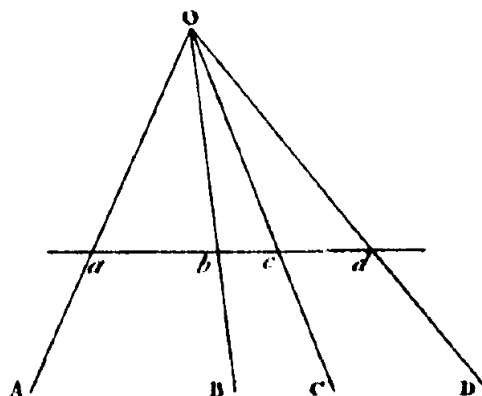
[Chasles, 1880], Planche I

Chasles then proceeds to show that this construction is general, that is to say that the construction is always possible whatever the relative position of the four given points. To determine c , it suffices to draw the parallel line to aa' from b instead in the second step of the construction. To determine a , by swapping the letters in the expression of the cross-ratio, Chasles points out that it suffices to carry out the same construction starting with point b instead of a . If λ is negative, then it suffices to put α and α' on different sides of point a . Therefore, this construction is always possible. This way of presenting problems and their (general) constructions will reappear constantly throughout the *Treatise*. In particular, Chasles does not need to “generalize” the construction, for instance adding a subclause to the procedure when confronting a case such as a negative cross-ratio. Instead, he has set up a way to describe a construction which relies on permanent properties of four points, whether they are real points, at infinity, or imaginary (in pairs). The step of the construction in which its generality is demonstrated is a novel addition to the problem-solving of the Greeks, and one of the ways in which Chasles’ *Higher Geometry* both borrows from and expands on what is identified as an old and respectable tradition.

Every notion presented in the *Treatise* has its own dual counterpart. In the case of the cross-ratio, this means that we can define the cross-ratio of four lines A, B, C, D passing through a single point o (or, as Chasles puts it, four lines from a single sheaf), as the following quantity (see fig. below):

$$\frac{\sin(A, C)}{\sin(A, D)} : \frac{\sin(B, C)}{\sin(B, D)}$$

Fig. 2. p.11.



where $\sin(A, C)$ denotes the sine of the angle at point o between the lines A and C .

The fundamental property of the cross-ratio, which Chasles already mentioned in his Préface, is that it is *projective*⁹⁴. This means that, for any four lines from a single sheaf, their cross-ratio is equal to that of the four points formed by the intersection of any transversal line and these four lines. Furthermore, this implies that the cross-ratio of four points formed by intersection of four given lines and a transversal line does not depend on the position of the transversal line. Once again, Chasles carefully shows that the equation also preserves signs, and maintains the generality of the geometrical discourse⁹⁵. Note that this property provides Chasles with a short proof of the equation $ab.cd + ac.db + ad.bc = 0$ discussed above⁹⁶. He goes on to discuss various properties of cross-ratios, in particular when one point is at infinity, which we need not discuss here.

In Chapter III, Chasles turns to what he calls “systems of four points”. When two such systems of points a, b, c, d ; a', b', c', d' (possibly on different lines), have the same cross-ratio, this can be expressed through several equations. First come what Chasles calls “two-term equations”, such as

$$\frac{ac}{ad} : \frac{bc}{bd} = \frac{a'c'}{a'd'} : \frac{b'c'}{b'd'}$$

which only express the equality between the cross-ratios. Then Chasles gives “three-term equations”, which he obtains by taking the right-hand term above, and replacing the (first) cross-ratio of the second system of points by one minus the second cross-ratio of

⁹⁴The term originates from Poncelet’s *Traité des propriétés projectives*, [Poncelet, 1822], p.12. Chasles attributes the discovery of this property to Pappus, and perhaps even to Euclid, [Chasles, 1852], pp.xxi.

⁹⁵ [Chasles, 1852], pp.12-13.

⁹⁶ [Chasles, 1852], p.17.

the second system. This gives, among other things, the following equation:

$$\frac{ac}{ad} : \frac{bc}{bd} + \frac{a'b'}{a'd'} : \frac{c'b'}{c'd'} = 1$$

Now, other kinds of three term equations can be given, by introducing an arbitrary point m' on the line of the second system. It must be pointed out that Chasles also discusses properties which cannot be expressed by a certain kind of equation. For instance, when expounding the notion of the harmonic ratio of two systems of two points, he points out that a simple “équation de condition” can characterize this configuration. Indeed, if two systems of points are represented by the equations

$$\begin{aligned} x^2 + ax + b &= 0 \\ x^2 + a'x + b' &= 0 \end{aligned}$$

the equation $b + b' = \frac{aa'}{2}$ (or, alternatively, $\nu + \nu' = 2m\alpha \cdot m\alpha'$) fulfils this task. However, no similar equation can be found to express the fact that two systems of two points have a certain cross-ratio, as the points from one system do not play a symmetric role in the formation of a cross-ratio as they do in the formation of an harmonic ratio.

2.2.2 Homographic divisions of a line

In Chapter VI, Chasles introduces a new key notion, namely *homographic divisions of a line*⁹⁷. The definition of this notion seems rather ambiguous at first⁹⁸:

Définitions. – Quand deux droites sont divisées en des points qui se correspondent un à un et tellement que le rapport anharmonique de quatre points quelconques de l’une soit égal au rapport anharmonique des quatre points correspondants de l’autre, nous dirons que ces deux droites sont divisées *homographiquement* ou bien que leurs points de division forment deux *divisions homographiques*.

One way to understand this definition is to think of homographic divisions as mappings

⁹⁷This notion was already introduced toward the end of Chasles’ memoir on the principle of homography in 1837, see [Chasles, 1837], p.832. However, this notion did not occupy such a central place as it would in Chasles’s lectures. This is partially due to the fact that, in 1837, Chasles mostly viewed the principle of homography as a geometrical transformation, used for instance in the study of the property of second-order surfaces.

⁹⁸“Definitions. – When two straight lines are divided in points which correspond one by one so that the cross-ratio of any four points of one of them are equal to that of the four corresponding points of the other, we say that these two lines are *homographically* divided, or that their dividing points form two *homographic divisions*”, [Chasles, 1852], p.67.

from one line to another⁹⁹, such that the cross-ratio of any four given points of the first line is equal to that of the four images of these points under the mapping. However useful to modern-day readers this interpretation may be at first, it is not only anachronistic, but more crucially it introduces foreign means of representation. With symbols for mappings, come symbols for inverse mappings or images, notations for composing mappings etc., which are all operations with which Chasles’ textual device sometimes struggles, and sometimes performs surprisingly efficiently. Chasles’ use of letters and apostrophes to denote corresponding points on both lines (for instance, a corresponding to a') has certain restrictions. It makes it cumbersome to iterate or inverse the correspondence. But Chasles’ device also has a certain plasticity, which better fits the specific geometrical context in which it is supposed to be used. Indeed, for Chasles, homographic divisions are not abstract mappings, or even fundamental objects to be consider for their own sake, but rather a general term to discuss in a uniform and systematical manner the configurations which arise in many a geometrical question. In Chasles’ own words¹⁰⁰:

Nous verrons qu’il y a beaucoup de manières de former des *divisions homographiques*.

As we shall see, more than a mathematical object or a property of some object, homographic divisions act as a textual device to structure the geometrical discourse. It facilitates the expressions of general propositions, and of general constructions; in particular, it will be used to discuss not only series of points, but also of curves. As we see in this definition, homographic divisions are intimately connected to the rather loose notion of correspondence between series of points or sheaves of lines. While no explicit definition is given here, later uses of this term tend to suggest that a correspondence between two lines is usually obtained by a geometrical construction which can be reversed. Alternatively, a correspondence is given by an algebraic equation between the coordinates of two variable points. In the case of the attraction of ellipsoids, for instance, Chasles defined a correspondence between the points of two confocal ellipsoidal layers by a simple algebraic equation, which he later interpreted as saying that corresponding points of a series of confocal ellipsoidal layers formed normal lines to families of level-surfaces¹⁰¹.

When actually using the notion, Chasles does not refer to an homographic division merely by words, but always uses an unfinished series of letters to denote its points. Typically, when describing the construction of a division homographic to a first, given

⁹⁹In modern terms, an homographic function on a projective line is a function $f : \mathbb{P}^1 \rightarrow \mathbb{P}^1$ of the form $f(x) = \frac{ax+b}{cx+d}$. In practice, in Chasles’ geometry, homographic divisions are one-to-one correspondences between series of points or rays.

¹⁰⁰“We shall see that there are many ways to form *homographic divisions*”, [Chasles, 1852], p.67.

¹⁰¹See [Michel, 2020], pp.24-27.

division, he writes¹⁰²:

Une droite L étant divisée en des points a, b, c, d, \dots , si l'on veut diviser *homographiquement* une seconde droite L' , on pourra prendre arbitrairement sur celle-ci trois points a', b', c' pour correspondre, un-à-un, aux trois points a, b, c ; puis on déterminera les points d', e', \dots , qui correspondront aux autres points d, e, \dots de la première droite, par la condition que le rapport anharmonique des points a', b', c' et un quatrième d' soit égal à celui des quatre points a, b, c, d .

Notice that the general construction of a fourth point d' forming a given cross-ratio with three given points a', b', c' , laid out previously, plays a crucial role here: without it, one would not be able to form homographic divisions in general.

After stating a few elementary properties of homographic divisions, such as a transitivity property¹⁰³, Chasles puts this notion to use in a way that it is useful to describe precisely. He first explains that when a sheaf of lines intersect two lines, it forms on them two homographic divisions. This is once again another way for Chasles to restate the fundamental theorem underlying the whole *Treatise*, namely the projectivity of the cross-ratio. However, this permits the introduction of a new terminology. Chasles uses the term “homolog” to denote points on each line which correspond to each other in the equation for the equality of the cross-ratios; then, he adds, it is clear that the intersection point of both lines “represents two coinciding homologous points”. This sets up a converse statement¹⁰⁴:

Quand deux droites sont divisées homographiquement, si leur point de concours, considéré comme appartenant à la première division, est lui-même son homologue dans la seconde division, les droites qui joindront un à un respectivement tous les points de division homologues concourront en un même point.

¹⁰²“A straight line L being divided into points a, b, c, d, \dots , if one wants to divide *homographically* a second straight line L' , it will suffice to choose arbitrarily on the latter three points a', b', c' to correspond, one by one, to the points a, b, c ; then the points d', e', \dots which correspond to the other points d, e, \dots of the other straight line will be determined by the condition that the cross-ratio of the points a', b', c' and a fourth one d' be equal to that of the four points a, b, c, d ”, [Chasles, 1852], pp.67-68.

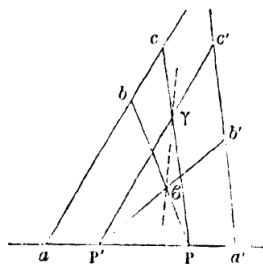
¹⁰³When two divisions are homographic with a third one, they are homographic with one another as well, [Chasles, 1852], p.70.

¹⁰⁴“When two lines are homographically divided, if their intersection point considered as part of the first division is its own homolog in the second division, then all of the lines joining all pairs of homologous points will meet at some point”, [Chasles, 1852], p.70. The reason for this is simple: suppose $a = a'$ is the intersection point of these two lines, and take b, c, d on the first line (resp. b', c', d' on the second line). Let S denote the intersection point of the straight lines bb' and cc' , and d'' the intersection point of Sd and $a'b'$ (that is to say, the second line). Then the cross-ratio of a, b', c', d'' is equal to that of a, b, c, d per invariance by projection, and therefore is equal to that of a', b', c', d' per hypothesis. This equality between two cross-ratios, whose first three points are the same, means that $d' = d''$, for reasons discussed previously. Therefore, S is on the line dd' , which suffices to prove the theorem.

It must be stressed that here Chasles operates within the confines of a particular linguistic structure, which makes the expression of some properties rather cumbersome to our modern eyes. This definition, for instance, would seemingly be much easier to express with the help of functional notations¹⁰⁵. However, as we will see, this allows a certain flexibility in the actual use of these fundamental properties in the context of a proof or a construction. Note also that he does not consistently use the notion of correspondence, as exemplified by this property where homologous points are discussed instead. Chasles’ vocabulary is in flux, both across time and across writing contexts. Similarly, he alternatively mentions “sheaves” (*faisceaux*) of lines or “rays” (*rayons*) from a point (or even turning about a point), in a way that does not seem to correspond to any theoretical or practical logic.

Next Chasles shows the dual version of this statement, that is to say that if, in two sheaves of rays turning about two fixed points, corresponding rays always intersect at points on one same straight line, then these rays form two homographic sheaves. As in the previous proposition, the line joining the centres of both sheaves has the unique property of being its own homolog. Conversely, if two homographic sheaves are such that the line joining their centres is its own homolog, the intersection points of homologous rays will all lie on a line (which they entirely describe). This provides Chasles with a “general mode of description of a straight line by points”, which goes as follows¹⁰⁶ (see fig. below):

Quand deux droites sont divisées homographiquement aux points a, b, c, \dots , et a', b', c', \dots , qui se correspondent un à un respectivement, si l’on prend sur une droite aa' , qui joint deux points correspondants, deux points fixes P, P' , les droites Pb, Pc, \dots rencontreront respectivement les droites $P'b', P'c', \dots$, en des points β, γ, \dots qui seront en ligne droite.



This is nothing short of an alternative way of writing the equation of a line. This mode of description, on top of being completely general, is useful insofar as P and P' can

¹⁰⁵Suppose $f : L_1 \rightarrow L_2$ is an homographical function between two projectives lines; let a denote the intersection between L_1 and L_2 , then Chasles’ proposition translates into the equation $f(a) = a$.

¹⁰⁶“When two lines are homographically divided in the points a, b, c, \dots , and a', b', c', \dots in a 1 – to – 1 correspondence, taking any two fixed points P, P' on the line aa' , the lines Pb, Pc, \dots will intersect the lines $P'b', P'c', \dots$ at points β, γ, \dots on a straight line.”, [Chasles, 1852], p.71.

be arbitrarily chosen, but also because homographic divisions on lines can be constructed in a variety of ways. Therefore, unlike Algebra, this mode of description has no need for the introduction of auxiliary quantities, and can be used naturally in the context of a proof or a construction in many cases¹⁰⁷.

In the following chapter, Chasles examines the different ways in which an homographic division can be expressed. Here again, he expounds the several kinds of equations which can be used to that effect, introducing variable (or, rather, indeterminate) points. Given two lines, the equation for an homographic division thereof can be obtained by fixing three points a, b, c on the first one, and three points a', b', c' on the second one. For any point m on the first line, its homolog m' is determined by the two-term equation:

$$\frac{am}{bm} : \frac{ac}{bc} = \frac{a'm'}{b'm'} : \frac{a'c'}{b'c'}$$

which can be rewritten as $\frac{am}{bm} = \lambda \frac{a'm'}{b'm'}$, where λ is a constant quantity¹⁰⁸ depending only on a, b, c, a', b', c' . Conversely, two variable points m, m' on two lines $ab, a'b'$ which satisfy an equation like this one (for any fixed λ) form homographic divisions on these lines. Chasles then notes that λ has a “very simple geometrical expression”, which he obtains by supposing that m' is at infinity. If I is the homolog (in the first division) of the point at infinity, then $\lambda = \frac{aI}{bI}$. This, in turn, allows Chasles to choose a, b, a', b' so that $\lambda = -1$ and reduce the equation above to $\frac{am}{aI} = \frac{a'm'}{J'm'}$, where J' is the homolog of the point at infinity (viewed as a point of the second division). Other transformations of the equation are considered, however none is deemed better than the others¹⁰⁹. This work on the many forms this equation can take only serves as a reservoir, from which the most suitable will be picked when actively solving a problem.

In the same manner as for the equations expressing equalities between the cross-ratios of systems of four points, Chasles obtains three-term equations by substituting to one cross-ratio r the quantity $1 - r$, or “second cross-ratio” in his terms. Subsuming the fixed quantities depending only on a, b, c, a', b', c' into two constants, he obtains an equation he claims will turn out “very useful¹¹⁰”, then the following four-term equation¹¹¹:

$$am \cdot b'm' + \lambda am + \mu b'm' + \nu = 0$$

which we will run into at several occasions in the second half of this chapter, especially

¹⁰⁷Chapter XVII gives many more ways to describe the points of a line.

¹⁰⁸Not to be confused with former uses of the same character, where λ denoted a given cross-ratio.

¹⁰⁹ [Chasles, 1852], pp.81-82.

¹¹⁰ [Chasles, 1852], p.87.

¹¹¹ [Chasles, 1852], p.93. In fact, Chasles even shows how to create equations of an arbitrary number of terms (pp.100-101), even though he does not seem to have ever used an equation with five terms or more. These equations are all equivalent, as they represent the same configuration.

because it is consistently Chasles’ choice when discussing homographic divisions formed on the same line, which will be the usual setup in which proofs by correspondence are carried out.

Note how the textual device used to talk about homographic divisions allow for an easy manipulation of homologous points, as these are marked with the same lower-case letter, with or without a prime. However, denoting divisions in this manner does not allow for an easy manipulation of points which are transformed several times. For instance, in Chapter X, Chasles is able to define homographic divisions in involution with the following terms¹¹²:

Pour que deux divisions homographiques formées sur une même droite soient en involution, il suffit qu’un seul point quelconque de cette droite, considéré comme appartenant successivement aux deux divisions, ait le même point homologue dans les deux cas.

This may seem like a rather heavy-handed way of expressing this property. Furthermore, this very expression highlights the difficulty that Chasles’ language faces when trying to grasp points that are projected from one division to the other several times. In fact, the two first chapters (IX, X) devoted by Chasles to the theory of involution give a plethora of alternative descriptions of this object. Some of these will be deemed simple, yet rejected on the grounds that they do not constitute the natural definition of this object. For instance, two aligned pairs of points a, b ; a', b' being given, a unique point O is defined by the equation $Oa \cdot Oa' = Ob \cdot Ob'$. This gives rise to an involution in which O corresponds to the point at infinity (O is then called the “central point” of the involution). This is a general property which fully characterizes a system of six points in involution; however, it relies on a point that is “foreign” to the system of points and hence does not adequately define involutions¹¹³.

In the *Treatise*, to study the properties of involutions, Chasles often elects to view them as a special case of the general equation for homographic divisions. Indeed, he explains, homographic divisions in involution possess the same generality as any two homographic divisions: the only particularity of involutions stems from the way the two homographic divisions are conceived “on top of each other¹¹⁴”. In other words, given two homographic divisions on two lines, one line can be placed on top of the other so that

¹¹²“For two homographic divisions formed on a single line to be in involution, it suffices that any single point on this line, considered as belonging successively to each division, have the same homolog in both cases”, [Chasles, 1852], p.168.

¹¹³“La propriété du point central caractérise d’une manière très-simple le système de six points en involution. Toutefois cette propriété ne me paraît pas être la plus propre à définir l’involution, parce qu’elle repose sur la considération d’un point étranger au système des six points dont il faut exprimer les relations mutuelles”, [Chasles, 1852], p.141.

¹¹⁴ [Chasles, 1852], p.169.

these two divisions are in involution. Therefore, the general equation for homographic divisions ($am \cdot b'm' + \lambda am + \mu b'm' + \nu = 0$) can be used to express the relation of involution. Taking the four-term equation given above, demanding that the divisions are in involution amounts to the demand that, for instance, the point at infinity has the same homologous points when viewed as a point of each division. Thus, if it corresponds to J' in the second division, and I in the first division corresponds to the point at infinity, then $I = J'$ must be imposed. When the point m is at infinity the general equation yields $aI = -\mu$; and when m' is at infinity then $b'J' = b'I = \lambda$. Thus, Chasles obtains a condition for an homographic division to be in involution:

$$\lambda - \mu = ab'$$

Chasles then adds that a given equation for two homographic divisions represents an involution if and only if the equation is symmetrical with regards to the variable points m, m' , as this means that these two variable points “are interchangeable, which characterizes involution¹¹⁵”. Therefore, a better way to handle involutions is found in the writing of equations itself, which overcomes the initial limitation of the literary technology that is formed around the vocabulary of divisions.

2.2.3 An example: Apollonius’ *De Sectione Determinata*

We conclude this study of Chasles’ *Higher Geometry* with one of the first examples he deals with, at the beginning of Section II. As we already mentioned, Sections II to IV are mainly applications of the theories outlined above. The beginning of Section II serves to show how classical geometrical problems can be captured with this new language and solved in a general way. Consequently, the very first problem presented by Chasles is Apollonius’ *De Sectione Determinata*¹¹⁶. This problem, Chasles notes, is part of the corpus of Ancient Geometrical Analysis. Although in this text he reverts to using Analysis to refer to algebraic methods, this choice of example shows Chasles’ intention to display the efficiency of his newly-created instrument when tackling problems very much in the tradition of geometrical analysis. Apollonius’ statement of the problem, as translated by Chasles, goes as follows¹¹⁷:

Etant donnés quatre points en ligne droite, on demande de déterminer sur

¹¹⁵ [Chasles, 1852], p.170.

¹¹⁶In fact, this book was lost and is only known through Pappus. Famous attempts to restore the content of this book, which Chasles was keenly aware of, included those of Snell and Simpson. An Arabic translation of this text, probably dating from the ninth century A.C., was preserved, translated, and published recently, see [Apollonius, 2010].

¹¹⁷Chasles’ discussion of this problem can be found in [Chasles, 1852], pp.199-211.

cette droite un cinquième point tel, que le produit de ses distances à deux des quatre points donnés soit au produit de ses distances aux deux autres dans une raison donnée.

Chasles lets a, a', b, b' denote these four given points and λ the given ratio (or *raison*), and reformulates this problem as that of finding a point m so that

$$\frac{am \cdot a'm}{bm \cdot b'm} = \lambda$$

Immediately, with this rewriting of the problem, Chasles is able to give two direct solutions of this problem. Indeed, if one point m satisfies it, then the point m' which, coupled with m , forms a pair of points in involution with the pairs $a, a'; b, b'$, must also satisfy it, as involutions satisfy the equation $\frac{am \cdot a'm'}{bm \cdot b'm} = \frac{am' \cdot a'm}{bm' \cdot b'm'}$. From there many of the constructions given in the first Section enable Chasles to construct the solutions. Similarly, the equation above can be rewritten as

$$\frac{am}{bm} = \lambda \frac{a'm}{b'm} \quad (2.1)$$

which Chasles reads as expressing the fact that the solutions are the double points of two homographic divisions. According to results obtained previously, there are two such points, and they can be constructed in a variety of ways.

This problem may seem overly simple. Indeed, as Chasles acknowledges, it is immediately solved by the analytical method: just set an origin, write the equation for the distances between these points and the origin, and you immediately have a second-degree equation whose roots will give you the solution of the problem in a general manner. However, in pure Geometry, no such general answer could be found. Simpson, for instance, only solved this problem “with the help of various propositions, which make the general question depend on its particular cases¹¹⁸”. Chasles’ theories do not result in such shortcomings: on the contrary, they enable constructions which are directly rooted in general theories and require no auxiliary quantity as the analytical solution does. Chasles’ first solution, *via* the theory of involution, is even valid when both given pairs of points are imaginary. Furthermore, these geometrical methods - in particular, the first one - allow for a precise discussion of the number of solutions in cases of impossibility. Indeed, if α, β denote the middle points of aa', bb' , then the middle point of mm' satisfies the relation $\frac{\alpha\mu}{\beta\mu} = \lambda$. Therefore, when the segments aa', bb' do not overlap, for instance, λ must be negative (as μ must be between α and β), and $|\lambda|$ must be comprised between two bounds that are easy to determine with the help of some elementary results in the theory of involution. When λ crosses these bounds, both solutions are real, but coincide, and when λ is out of these bounds, the problem has no solution. What’s more, Chasles’ discussion

¹¹⁸ [Chasles, 1852], p.200.

completely solves the problem for a value of λ that can be either negative or positive. Suppose you do not care for signs, then the number of solutions is augmented, although not always in the same way, depending on the given data. This, claims Chasles, was another blindspot of Ancient Geometry¹¹⁹:

Les anciens regardaient la constante toujours comme positive et ne donnaient pas de signes aux segments; cependant ils ne trouvaient, généralement, qu'une solution. D'après ce que nous venons de dire, ils auraient dû en trouver deux dans le cas de quatre points, et quatre dans le cas de deux points.

Il y a là un fait mathématique qui mérite d'être remarqué et que l'on en recherche la cause : c'est que les anciens, qui probablement s'étaient aperçus de la multiplicité des solutions et qui cependant ne pouvaient pas les comprendre sous un principe unique, parce que la notion des signes pour exprimer la direction des segments leur manquait, introduisaient dans les données de la question une condition qui suppléait à l'usage des signes.

Chasles' *Higher Geometry* remediates fully this problem: when solving the problems of Ancient Analytical Geometry, no further condition is to be introduced in the statement of a problem or a proposition. Furthermore, Chasles' choice of problem was strategic: this problem is one of those which, as Darboux would put it in his own lessons of geometry, "was the subject of entire Treatises, and is solved in but a few lines in Descartes' *Géométrie*¹²⁰". In Chasles' geometry, the same is true: a single "stroke of the quill"¹²¹ is enough to subordinate the varieties of configurations of this problem to one proposition.

Conclusions

Chasles' *Higher Geometry* can be viewed first and foremost as the construction of a new language for geometrical discourse, which captures the level of abstraction and generality of algebra whilst retaining the advantage of intrinsicality. Through this language, Chasles sought to unify the geometrical results of his predecessors in a systematic and methodical doctrine. To that end, he constructed a way to write non-algebraic equations for geometrical figures and correspondences between their elements. However, the expressive resources crafted through his lectures would evolve along with the problems which Chasles successively incorporated into his higher geometry. Indeed, as Chasles turned to more complex figures, new ways of verbally manipulating correspondences between series of points would be needed. In the next chapter, we turn to Chasles' adaptation of the

¹¹⁹ [Chasles, 1852], p.208.

¹²⁰ [Darboux, 1917], p.4.

¹²¹ [Chasles, 1837], p.143.

expressive resources presented here, in the context of their application to the geometry of algebraic curves.

Geometrical equations: the generation of curves via correspondences (1853-1860)

Introduction

The 1852 *Traité de Géométrie Supérieure* shows Chasles carrying out a reform of the language of geometry: therein were introduced basic notations, such as those for angles and segments, and textual devices for the manipulation of the fundamental objects of modern geometry, such as homographic divisions or involutions. However, nothing was said of figures more complex than circles. And yet, we know from the opening lecture of the second year of his teaching that applying the methods and language of *Higher Geometry* to curves, and in particular to conic sections, was one of the main goals in sight for Chasles¹. If an instrument has been created, whose role and expressive strength was comparable to that of Algebra, there remained to use it to effectively capture the generality and abstraction of Cartesian equations.

Between 1853 and 1857, Chasles gave a series of communications at the Académie des Sciences, directly borrowing from the content of the lectures he gave at the Sorbonne at that time², bearing precisely on these objects absent from the *Traité*: geometrical curves and surfaces. In these texts, he showed how homographic divisions, involutions, and the principle of signs could be used to construct curves of the third order passing through nine given points, as well as to construct roots of equations of degree three and four. These problems were inherited from the early-modern problem-solving tradition, of which Chasles was keenly aware. In particular, Chasles set out to walk in the footsteps of the authors he identified as forming a British geometrical tradition, namely: Newton,

¹This lecture is preserved as a handwritten draft in Chasles' folders, at the Archives de l'Académie des Sciences.

² [Chasles, 1853a], [Chasles, 1853b], [Chasles, 1855a], [Chasles, 1855b], [Chasles, 1857a].

MacLaurin, and Braikenridge. Like them, he set out to construct curves and roots of equations; but within the new language for geometry which he had constructed in his lectures. In so doing, he sought to make these constructions perfectly general, that is to say uniform and valid even if the conditions of the problem (such as the given points through which a cubic ought to pass) involved imaginary points, or points at infinity.

The goal of this line of research was not only to give a construction of certain curves or surfaces, but also to do so in a way that can be used to derive general properties of curves of a certain degree. In so doing, Chasles was led to introduce a new kind of geometrical objects, namely homographic sheaves of curves, which are written in a manner analog to the writing of homographic series of points. This analogy led in 1855 to the identification by Chasles of what he deemed to be a new principle of geometry: the principle of anharmonic correspondence.

This research programme grew out of Chasles' teaching; and in fact, at least one of his students took it up. The naval officer Ernest de Jonquières (1820-1901), who had attended some of Chasles' lectures, also proposed new notations and methods for the generation of curves of degree m , relying on this notion of correspondence. Through the intersection of corresponding sheaves, Chasles and De Jonquières would construct what they viewed as the “geometrical equations” of curves and surfaces.

3.1 The *Higher Geometry* of curves

3.1.1 An old problem in need of modern answers

In his series of papers on geometrical curves and surfaces, Chasles identified two sorts of geometrical problems which were yet to be solved generally by modern geometrical methods. These problems were that of the construction of curves passing through a number of given points, and that of the construction of algebraic equations³. In what follows, we shall focus exclusively on the former, because only it will play a role in Chasles' enumerative theories, which we study in chapter 4. However, it must be stressed that Chasles' construction of the curve of order three (to which we turn in section 3.1.2) and his construction of the cubic equation mirror one another, and that his answers to both questions are explicitly related.

In a fashion typical of his usual historiographical strategy, Chasles deemed the problem of the construction of curves to have been fully solved by the methods which grew

³The construction of equations is the problem of the construction of line segments with lengths equal to the roots of an algebraic equation, whose coefficients are given in way of line segments. This was a mathematical problem of great importance for early-modern geometers, especially after Descartes' 1637 *Géométrie*, in which interest had drastically weaned in the second half of the 18th century, see [Bos, 1984].

out of Descartes' *Géométrie*⁴, but that there remained to find a way for geometry to reproduce the feat of analysis in these questions. To that end, Chasles identified a tradition whose results and methods would prove crucial: this tradition is that of the British pure geometers of the 18th century, and in particular of Newton, MacLaurin, and Braikenridge⁵.

Chasles' interest for these geometers lay in the successive results which they had obtained regarding the "organic description of curves"; a term Chasles did not feel the need to define or discuss. To fix the ideas, let us begin with the definition given by D'Alembert in 1754 for the *Encyclopédie*⁶:

DESCRIPTION, *terme de Géométrie*, est l'action de tracer une ligne, une surface, &c. Décrire un cercle, une ellipse, une parabole, &c. c'est construire ou tracer ces figures.

On *décrit* les courbes en Géométrie de deux manières, ou par un mouvement continu, ou par plusieurs points. On les décrit par un mouvement continu lorsqu'un point qu'on fait mouvoir suivant une certaine loi, trace de suite & immédiatement tous les points de la courbe. C'est ainsi qu'on trace un cercle par le moyen de la pointe d'un compas ; c'est presque la seule courbe qu'on trace commodément par un mouvement continu : ce n'est pas que nous n'ayons des méthodes pour en tracer beaucoup d'autres par un mouvement continu ; par exemple, les sections coniques : M. Maclaurin nous a même donné un savant ouvrage intitulé *Geometria Organica*, dans lequel il donne des moyens fort ingénieux de tracer ainsi plusieurs courbes. [...] Mais toutes ces méthodes sont plus curieuses qu'utiles & commodes. La *description* par plusieurs points est plus simple, & revient au même dans la pratique. On trouve par des opérations géométriques différens points de la courbe assez près les uns des autres ; on y joint ces points par de petites lignes droites à vûe d'oeil, & l'assemblage de ces petites lignes forme sensiblement & suffisamment pour la pratique la courbe que l'on veut tracer.

Of course, descriptions of curves can already be found in Ancient Geometry, and one of the main impetus for the emergence of this general problem among early-modern mathematicians was the 1588 translation by Commandino of Pappus' *Collections*⁷. In fact, to

⁴ [Chasles, 1853a], pp.943-946; [Chasles, 1855a], pp.677-679. It is to be noted that, in Chasles' historiography of this problem, a collective identified as 'the Arabs' is credited with playing a crucial role in combining geometry and algebra. This assessment was based on the works of Sédillot and Woepcke, which Chasles actively supported and promoted at the time. See [Smadja, 2015], p.276.

⁵See [Chasles, 1853a], pp.944-945, [Chasles, 1870], p.223. Additionnal evidence can be found in Chasles' scientific archives left at the Académie des Sciences, where several leaflets betray a growing interest in the figure of Braikenridge starting from 1853 (Archives de l'Académie des Sciences, 35J/13).

⁶ [D'Alembert, 1754], p.878b.

⁷ [Bos, 2001].

reobtain these key insights of the Greeks' geometrical analysis was, as we saw previously, one of the tasks of the *Traité de Géométrie Supérieure*: the description of a line was given by a construction of its points using homographies and cross-ratios. It is crucial for Chasles that such solutions to problems be general, in the sense that the construction should work whatever the accidental position of the given data; such a solution was given to simpler construction problems, such as Apollonius' section. To describe curves, however, remained a difficult problem.

All these authors were already mentioned and praised in the 1837 *Aperçu Historique*. In this book, Chasles claimed that Newton had made great progress in that direction by introducing "methods of transformation, wherein points respond to points, lines to lines, and certain convergent lines become parallel⁸". But this tradition did not come from nowhere: some "germs" thereof can be already found in De Witt's general theory (and enumeration) of plane curves, explains Chasles⁹. However, De Witt's "fertile idea" only grew half a century later, with the works of the three aforementioned British mathematicians. The trio is then identified by Chasles as the source of a geometrical theory of great value and interest, which had been undervalued until the arrival of modern geometry, claims Chasles. There is a reason, however, why these works have not been sufficiently cultivated: they were difficult because they still lacked the aforementioned generality¹⁰.

In Chasles' historical reconstruction, Newton was the first to put Descartes' analytical geometry, this "universal instrument", to productive uses with respect to the theory of curves¹¹, and he was soon to be followed in this endeavor by MacLaurin. Indeed, Chasles asserts, Newton's 1706 *Enumeratio linearum tertii ordinis*, published at the end of his *Opticks*, contains a first successful attempt at the enumeration and classification of the lines of the third order. Newton's classification proceeds by sorting these curves into 5 classes, branching into 72 different kinds of curves. This classificatory practice completely disappears in Chasles' own work on the theory of curves, where such finely grained distinctions are to be avoided. Indeed, they would only betray a lack of generality in the theory itself, which in turn would only show that the true origin of these truths has not been found yet. However, the fifth chapter of the *Enumeratio*, titled *Genesis curvarum per umbras*, does not reproduce such a slew of particular curves, in Chasles' reading. Indeed, in this chapter, Newton explained how a luminous point could generate every conic curve as the shadow projected by a circle. In the sixth chapter, entitled *De curvarum descriptione organica* Newton elaborates on this idea to generate curves as the intersections of "legs" in a certain motion.

⁸ [Chasles, 1837], p.135.

⁹ [Chasles, 1837], pp.100-101.

¹⁰ [Chasles, 1837], p.136.

¹¹ [Chasles, 1837], p.141.

Newton's constructions are not perfectly general in Chasles' sense, although the latter refrains from making these shortcomings explicit, probably for strategic purposes in the context of the promotion of pure geometry. Indeed, these constructions are constantly embarrassed with particular cases: exceptions are made when some lines pass through special points, or when some lines are parallel, which is precisely what modern geometry should do away with. Moreover, Newton did not provide proofs for these results. Analytical proofs thereof were subsequently given, but without reaching for their true nature and origin, asserts Chasles¹². This shortcoming is made obvious by the fact that certain modes of generation for curves of the third order, deeply connected to that which Newton had described, had remained oblivious to the eyes of the Analysts who had yet sufficiently proved the validity of Newton's constructions.

In Chasles' reading, despite its shortcomings, Newton's *Enumeratio* proved to be most fruitful and influential. In its wake, and as an extension of its main results, MacLaurin wrote two crucial texts¹³. In 1720 he published his *Geometria Organica*¹⁴, wherein mobile angles are used to describe curves. Chasles further comments that the proofs contained in this book, which mainly rest on the use of coordinates, lack simplicity. This shortcoming, however, would be fixed shortly thereafter in a second text, whose writing started in 1721, but which was only published posthumously with MacLaurin's 1748 *Treatise of Algebra*. This text, to which we will refer as the *De Linearum*¹⁵, displays a "precision" and "elegance" which Chasles deemed "admirable¹⁶". In the footsteps of MacLaurin's, continues Chasles, walked Scottish mathematician William Braikenridge¹⁷, who published in 1733 his *Exercitatio Geometrica de Descriptione Linearum Curvarum* on this subject. Chasles has little comment to offer on his work, however his name will often be mentioned alongside those of Newton and MacLaurin¹⁸ in discussions of the (organic) description of curves¹⁹. After MacLaurin and Braikenridge, although several geometers have attempted to pursue this line of research, none have made any real progress, claims Chasles. How-

¹² [Chasles, 1837], p.146.

¹³Chasles' discussion of MacLaurin's work on the description of curves can be found in [Chasles, 1837], p.146-151.

¹⁴The full title is *Geometria organica, sive descriptio linearum curvarum universalis*, [MacLaurin, 1720]. Chasles claims it was published in 1719, which seems to be a mistake.

¹⁵The full title is *De linearum geometricarum proprietatibus generalibus tractatus*, and can be found in [MacLaurin, 1748], p.

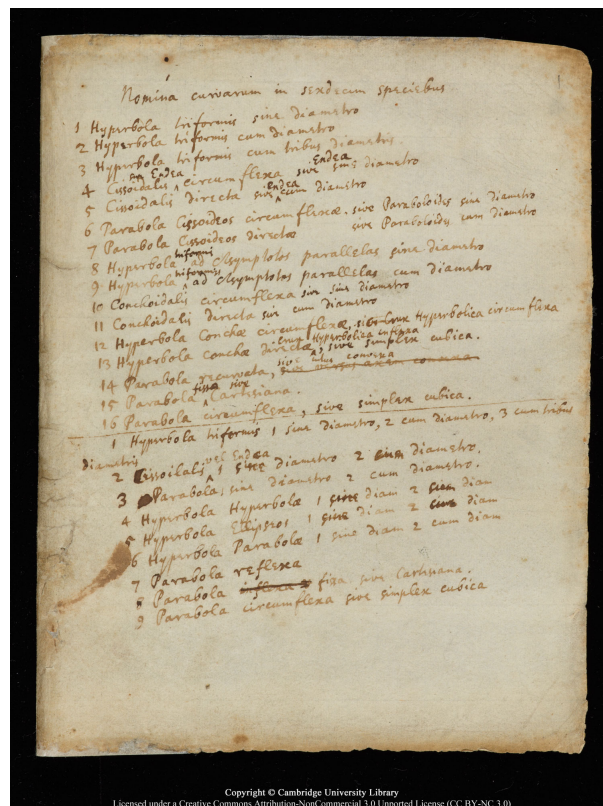
¹⁶See [Bruneau, 2011] for a discussion of the reception of MacLaurin's *Organic Geometry* in French 19th geometry, in particular in the works of Poncelet and Chasles. In particular, Bruneau notes (pp.28-29) that Chasles' interest in MacLaurin seems to have arisen between 1830 and 1837, probably whilst writing the historical part of the *Aperçu Historique*. Note that De Jonquières also studied and translated the *De Linearum* in 1856, which we discuss in the next section, see [de Jonquières, 1856a], pp.197-261.

¹⁷ [Chasles, 1837], p.151.

¹⁸ [Chasles, 1870], p.223 for instance.

¹⁹Note that Chasles ignores the relationship (and the priority dispute) between MacLaurin and Braikenridge, see [Bruneau, 2005], p.168 *et passim*.

ever, the progress of Geometry, and in particular the theories on which rests the *Higher Geometry*, allow for such a foray to be reached.



Cambridge University Library, MS Add.3961

Chasles' praise of Newton and MacLaurin owes mainly to the fact that he attributed to them a strong faith in pure geometry despite being also among the creators and first promoters of infinitesimal analysis. In that sense, Chasles viewed them as the last geometers to have cultivated both theories in their own rights. In the 1870 *Rapport sur les progrès de la Géométrie*, Chasles would even reproduce Poincaré's claim that "since Newton and MacLaurin, synthesis had been neglected, as if abandoned²⁰". Furthermore, as we have shown elsewhere²¹, MacLaurin's work was considered by Chasles to embody particularly well the epistemological advantages that pure geometry possesses over analysis. In giving alternative proofs of MacLaurin's generalized theorem for the attraction of ellipsoids, Chasles had constantly tried to emphasize how the generality of MacLaurin's purely geometrical proofs had been underestimated, and how the swift success of Analysis with regards to this specific mathematical problem had come to the expense of many other properties of the attraction of bodies. Similarly, with regards to the theory of curves, Chasles was keen on pointing out that few truly general results have been achieved through the 18th century, despite the rise of analytical methods.

²⁰ [Chasles, 1870], p.107.

²¹ [Michel, 2020].

Modern geometry, therefore, was to build on the British theory of curves, and to remediate the shortcomings thereof. In particular, in Note XV of the *Aperçu Historique*, Chasles discussed the special importance of Desargues' theorem for the theory of conics²². In order to show the fundamental character of Desargues' theorem (or, rather, of the converse of a conclusion of Desargues' theorem), Chasles showed how the following general description of conic sections could be derived from it²³:

Si deux angles de grandeur quelconque, mais constante, tournent autour de leurs sommets, de manière que le point d'intersection de deux de leurs côtés parcourt une conique passant par leurs sommets, leurs deux côtés se croiseront sur une seconde conique qui passera aussi par les deux sommets.

This theorem, Chasles continued, is already a generalization of Newton's description of conic sections. Indeed, Newton (as read by Chasles) had formed two sheaves of lines turning about two distinct points, so that the sheaves intersected on a given line. Newton then moved the position of one of these sheaves: thus, the cross-ratio of four lines of the first sheaf remained equal to that of the four corresponding lines in the other sheaf, so that the intersection point of the sheaves described a conic. But even this generalized mode of description itself can be further generalized, for instance by substituting cross-ratios to angles, by taking into account the possibility of parallel lines, or of lines at infinity. Chasles then commented²⁴:

Cette circonstance nous paraît bien propre à montrer deux choses: d'abord qu'il est toujours utile de remonter à l'origine des vérités géométriques, pour découvrir, de ce point de vue élevé, les différentes formes dont elles sont sus-

²²Note XVI discusses the dual version of this question. It must be noticed that the principle of duality does not operate as a result or a theorem from which one could derive dual theorems related to the theorems of Note XV. Rather, what Chasles does is reproduce the proofs and statements of said theorems, while constantly interchanging words such as points and lines. A further note also briefly discusses the generation of curves of the third order, see [Chasles, 1837], pp.348-350.

²³"If two angles, of arbitrary but constant magnitude, turn about their summits so that the intersection point of two of their sides describe a conic passing through both summits, then their two sides will cross on a second conic which also passes through both summits", [Chasles, 1837], p.337.

²⁴"This circumstance seems to use well suited to show two things: first, that it is always useful to trace back the origin of geometrical truths, to discover, from this high vantage point, the different forms they are susceptible to take, as these forms can extend the applications of these truths; for the theorem of Newton, which some very distinguished geometers did not neglect to prove, being one of the most beautiful of the theory of conics, has not had many important consequences, for its form was no suited for corollaries. The general theorem, on the contrary, from which we obtained Newton's theorem, is suited to a wealth of various deductions. We see here a proof of this truth, that the most general and fruitful propositions are at the same time the simplest and easiest to prove; for none of the proofs given of Newton's theorem can be compared, with respect to brevity, to the one we gave of the general theorem aforementioned", [Chasles, 1837], p.337. For a detailed study of this note, and in particular of Chasles' thesis that logically equivalent formulations of the same theorem may have different ranges of application, and different kinds of generality; see [Chemla, 2016], pp.77-85.

ceptibles et qui peuvent en étendre les applications; car le théorème de Newton, que quelques géomètres très-distingués n'ont pas dédaigné de démontrer, comme l'un des plus beaux de la théorie des coniques, n'a pourtant point eu de grandes conséquences, parce que sa forme ne se prêtait qu'à peu de corollaires. Le théorème général, au contraire, d'où nous le déduisons, se prête à une foule de déductions diverses.

On voit ensuite ici une preuve de cette vérité, que les propositions les plus générales et les plus fécondes sont en même temps les plus simples et les plus faciles à démontrer; car aucune des démonstrations qu'on a données du théorème de Newton n'est comparable, en brièveté, à celle que nous avons donnée du théorème général en question.

For Chasles, Newton's organic description of conics, like Pascal's mystical hexagram or Desargues' involution of six points, were all corollaries of one fundamental theorem, which lies “at the center²⁵” of the theory of these curves. To seek a purely geometrical and perfectly general description of a curve, in the *Aperçu Historique*, one ought first to search for such a fundamental property of said curve, which can then be transformed into a description.

Chasles' 1853 approach to the construction of cubics passing through nine given points builds on the same general idea; and Chasles would also begin by expounding one such central property for cubics. In fact, Chasles explicitly tied his new research with his past discussion of the theory of conics, which he considered to have been fully solved, including by pure geometry. Things were rather different with cubics, that is to say curves of the third order. While MacLaurin had given several modes of description of such curves, as intersections of lines turning about fixed points or of angles of fixed magnitude, these descriptions were all limited to particular cases; for instance requiring that the curve have double points²⁶. Chasles thus identified his task very clearly: to do for cubics what had been done for conics.

However, as we saw in chapter 2, through his lectures, new epistemological themes had emerged in the course of Chasles' teaching, among which was the importance of linguistic resources and notational devices for the expression of generality and abstraction. For this reason, when Chasles turned to the construction of curves after the publication in 1852 of the *Traité de Géométrie Supérieure*, this central theorem had become a “true equation²⁷”:

On conçoit, en effet, toute l'importance de cette question, au point de vue théorique; car un moyen simple de construire un dixième point quelconque

²⁵ [Chasles, 1837], p.338.

²⁶ [Chasles, 1853a], pp.944-945.

²⁷ [Chasles, 1853a], p.945.

de la courbe du troisième ordre déterminée par neuf points, exprimera une propriété générale, véritable équation de la courbe, qui devra se prêter, avec plus ou moins de facilité, au développement de toutes les autres propriétés. C'est ainsi que les propositions relatives à six points d'une conique, telles que le théorème de Pappus *ad quatuor lineas*, le théorème de l'*involution* de Desargues, celui de l'hexagone de Pascal, et le théorème fondé sur l'égalité des *rapports anharmoniques* des deux faisceaux de quatre droites, menées de deux points de la courbe à quatre autres, constituent, sous des formes différentes, autant d'équations de la courbe, et sont les éléments les plus utiles et les plus féconds dans cette vaste théorie.

Notice how Desargues' and Pascal's theorems are now described by Chasles as “propositions relative to six points of a conic”, which all “constitute, under different forms, equations of the curve”. Of course, these theorems were already in 1837 identified by Chasles as expressing a property of six points of a conic, but this was not their key characteristic. Instead, Chasles would describe these theorems as “stating an equality of *cross-ratios*²⁸”.

Similarly, the idea of finding a geometrical equivalent to equations of curves was only present in 1837 in a discussion of Euclid's *Porisms*, a lost text central to the tradition of Ancient Greek geometrical analysis. However, we saw in section 2.1 how both geometrical analysis and new ways of forming equations for elementary figures (pairs of points, lines) had gained a tremendous importance within Chasles' teaching of geometry in the wake of a renewed understanding of the history of analysis and algebra. In the same way, when thinking about the description of curves, Chasles renews his comparison with analytical geometry. The nine given points are now likened to nine given (or *data*), and the Cartesian equation of the cubic is just one way of expression of a property of a tenth (variable) point on a cubic determined by nine given (fixed) points²⁹. While Chasles had previously lauded Descartes' ability to “subordinate the whole of the collection of geometrical curves to one stroke of the quill³⁰”, he would now think of these equations through the lens of a renewed understanding of algebra as just one possible instrument for the development of geometrical analysis. As a result, he would search for an alternative instrument: one made of correspondences, homographies, and involutions.

This first foray into the theory of curves of the third order was undertaken via two successive communications given at the Académie des Sciences. In his initial communication, given on May 30th, Chasles explained he had found two such constructions, but

²⁸ [Chasles, 1837], p.335.

²⁹ See Chasles' discussion of the analytical expression for the cubic passing through nine given points given earlier by Salmon; [Chasles, 1853a], p.946.

³⁰ [Chasles, 1837], p.143.

only presented the first one. The second construction would be given a few months later, on Aug. 16th, in another communication to the Académie. The first method rests on the construction of a 10th point, which is entirely determined by the 9 given points; while the second one rests on the construction of a straight line, which is also determined by the 9 given points³¹. Both constructions, Chasles insists, are not only “very simple”, but they also “encompass all particular cases³²”: some of the given points can coincide (thus producing a cubic with a double point), or be at infinity, and there can even exist pairs of conjugate imaginary points. In fact, Chasles only mentions this generality, and does nothing to showcase it within the construction itself. The notational convention adopted in the *Traité de Géométrie Supérieure* are supposed to produce propositions that are true regardless of particular configurations, and to be inserted in proofs that are general in the sense that no special adaptation is required for particular configurations. As such, the principles of signs and the introduction of imaginary points as described previously have very much become part of the grammar of Chasles’ geometry: they need no introduction or explicit reference to convey generality to the proofs and constructions which resort to homographies, involutions, and correspondences.

3.1.2 Constructing curves of the third order

We now turn to Chasles’ first construction of the cubic passing through nine given points. The general idea behind this construction is to form a homographic correspondence between the elements of a sheaf of conics passing through four common points, and a sheaf of rays turning about a fixed point. The two intersection points of the corresponding elements is then generates a cubic. Indeed, as it was known at least since Lamé, the equation of a sheaf of conics passing through four points is

$$C_1 + \lambda C_2 = 0$$

where $C_1 = 0$ and $C_2 = 0$ are the equations of any two conics of the sheaf, and λ a (real) parameter. Similarly, all the straight lines turning about a fixed point have an equation of the form

$$\Delta_1 + \lambda' \Delta_2 = 0$$

where $\Delta_1 = 0$ and $\Delta_2 = 0$ are the equations of any two rays of the sheaf, and λ' another parameter. Since the elements of both sheaves are in a homographic correspondence, Chasles will be able to show that, with specific choices for $C_1, C_2, \Delta_1, \Delta_2$, one can ensure that $\lambda = \mu \lambda'$, for some fixed quantity μ . In this case, the equation of the intersection of

³¹ [Chasles, 1853b].

³² [Chasles, 1853a], p.947.

two corresponding elements can be computed by eliminating λ from both equations; and it is

$$\mu C_1 \Delta_2 - \lambda C_2 \Delta_1 = 0$$

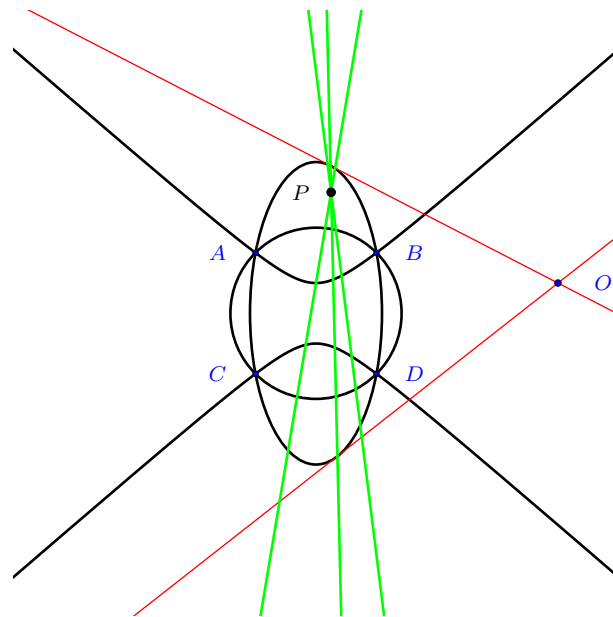
which is an equation of degree 3. This shows that the intersection points do generate a cubic. There remain only to construct both sheaves, as well as the correspondence between them, in a way that ensures that the cubic thus generated pass through the stipulated points – which, in what follows, we shall denote A, B, \dots, I .

To construct a correspondence between a sheaf of conics and a sheaf of rays, Chasles first defines the cross-ratio of four conics in such a sheaf. To that end, he uses the following pair of propositions, which at the time he had stated and proven in his lectures, but in no published texts³³ (see fig. below):

When several conics go through four same points (real or imaginary),

1. The polars of a fifth point, with regard to these curves, all go through one same point.
2. The polars [of a fifth point] with regard to any four curves [of this sheaf] always have the same cross-ratio, regardless of [the position of] the fifth point.

We can also say that the polars [with regard to these curves] of two points, taken arbitrarily, form two homographic sheaves.



The polar lines (in green) of O with respect to the conics (in black) passing through four fixed points all intersect at one common point P .

³³ [Chasles, 1853a], p.948. The proof we sketch below is borrowed from [Chasles, 1865b], p.203.

Indeed, sheaves of conics defined by four points intersect on a fixed straight line segments in involution, per Desargues' theorem. Thus, on this straight line, there are two points (possibly imaginary) which divide harmonically each of the segments intercepted by the conics of the sheaf – these points are the centers of the involution of segments. Consequently, these two points must be conjugate with respect to each conic of the sheaf; that is to say, the polars of one must go through the other (and the converse is automatically true). Now, denoting P the fixed point of which the polar lines in the proposition are constructed, and A, B two conics of the sheaf, Chasles considers the intersection point P' of the two polars of P with respect to A and B . P and P' are conjugate with respect to A , per construction. Similarly, P and P' are conjugate with respect to B . Therefore, P and P' are the two points that are conjugate with respect to every conic in the sheaf, that is to say the center of the involution of segments described above. Therefore, P' is on the polar of P with respect to any other conic C in the sheaf.

The import of this proposition is that it enables Chasles to construct a correspondence between the conics passing through four given points, and the rays turning about one fixed point – the sheaf of polars described in the propositions above. In other words, this introduces the possibility of discussing homographic sheaves of conics and rays. Furthermore, this construction does not depend on the choice of the pole of these rays; this will prove important as Chasles will let the center of this sheaf indeterminate when setting up the construction of the cubic. Equipped with this tool, Chasles can state what he calls a “general theorem”, wherefrom the general solution to the problem of constructing a cubic passing through nine given points is said to derive “sur-le-champ”³⁴:

THÉORÈME GÉNÉRAL. Quand une série de coniques passent par les quatre mêmes points (réels ou imaginaires), si l'on prend les polaires d'un cinquième point arbitraire, par rapport à ces courbes, puis, que, par un autre point fixe P quelconque, on mène des droites (dont trois de direction arbitraire) formant un second faisceau homographique au faisceau formé par les polaires, ces droites, qui correspondront, une à une respectivement, aux coniques, rencontreront, respectivement, ces courbes en des points dont le lieu géométrique sera une courbe du troisième ordre passant par les quatre points communs aux coniques et par le point P .

In other words, suppose that to a sheaf of conics passing through A, B, C, D , there corre-

³⁴“GENERAL THEOREM. When a series of conics go through the same four points (real or imaginary), if one takes the polars of a fifth arbitrary point with regard to these curves, and then, if through another fixed point P , one draws straight lines (three of which are in arbitrary directions) which form a second homographic sheaf to the sheaf formed by the polars, then these straight lines, which correspond respectively one-to-one to the conics, will meet, respectively, these curves at points whose geometrical locus will be a curve of the third order, passing through the four points common to the conics and the point P ”, [Chasles, 1853a], p.949.

sponds (homographically, that is to say one-to-one) a sheaf of polar lines as described by the first proposition. Then, from a point P , construct a sheaf of rays which is homographic to that first sheaf of polar lines. This can always be ensured by choosing arbitrarily three rays r_1, r_2, r_3 passing through P , and by associating them to any three rays s_1, s_2, s_3 of the first sheaf. Then, for each ray s_4 of the first sheaf, it suffices to construct the ray r_4 passing through P such that the cross-ratio $[r_1, r_2, r_3, r_4]$ be equal to $[s_1, s_2, s_3, s_4]$, and then by associating r_4 and s_4 . By transitivity, we then have a one-to-one correspondence between each conic passing through a, b, c, d and each straight line passing through P . The locus generated by the two intersection points of corresponding curves and straight lines, the theorem asserts, is a cubic passing through these five points. Note that, while the four base points of the sheaf of conics are already chosen by Chasles amongst the nine points through which the cubic will have to pass, P remains at this stage indeterminate.

This theorem, Chasles explains, constitutes a general property of cubic curves “of the same kind as [property of] cross-ratios in the geometry of conics, which can form the basis for a theory of curves of the third degree” – that is to say that this theorem plays a role similar to that fundamental property at the center of the theory of conics about the equality of two cross-ratios defined by pencils of straight lines. The key difference here, of course, is that one of the pencils has become a sheaf of conics.

After listing several ways in which this theorem could be proven, Chasles elects to show that any given line L crosses this locus in exactly 3 points. To that end, he first writes that the line L intersects each conic of the sheaf at points m, m' , and the (corresponding) ray drawn from point P at point n . Note that these letters m, m', n denote three variable points, that is to say points which are determined uniquely for each conic of the series and each line of the pencil, but which vary when the conic changes. These letters enable Chasles to talk simultaneously of the corresponding conics and lines, as well as of their intersection points with L . Chasles also lets μ denote the (variable) centre of the segment mm' . The points μ, n form two divisions of the line L , which Chasles claims are homographic. Since the rays drawn from P form, per definition, a pencil that is homographic to the pencil composed of the polars of any fixed point with regard to the series of conics, the intersections of these pencils with L form two homographic divisions of the line³⁵. Therefore, Chasles explains, by choosing this fixed point as the point at infinity on L , the property still stands, and the divisions thus created are precisely n and μ .

Furthermore, Chasles notes that the segments mm' are in involution. As we saw in the previous section, this means that in the division m of the line L , m and m' play

³⁵Remember that the cross-ratio of four lines intersecting at a common point is equal to the cross-ratio of the four intersections of these lines with any other line.

symmetric roles. Chasles justifies this assertion by the fact that the conics pass through four common points. Indeed, this implies that for any point m , there is one and only one conic passing through the four given points and m , and this conic passes through m' as well. Therefore, the conics determined by m and by m' are the same, and the segments mm' are indeed in involution. Thus, Chasles can use one of the equations characterizing segments in involution, which he gave in the *Traité de Géométrie Supérieure* (see section 2.2.2), namely:

$$Om \cdot Om' = \nu$$

where O is a point on L , uniquely defined and called the “central point” of the involution³⁶, and where ν is a constant number.

Chasles had also given several equations which characterized homographic divisions of a line, some of which involved an arbitrary fixed point a on the line. Writing the four-term equation, which we discussed above, for the homographic division formed by n and μ , and choosing the central point O of the involution mm' as the fixed point A , Chasles obtains:

$$O\mu \cdot On + \alpha \cdot O\mu + \beta \cdot On + \gamma = 0$$

where α, β, γ are three constant numbers.

From this equation, it follows that to each point n corresponds exactly one point μ , or, equivalently, two points m, m' . For n to be a point on the curve that is being sought, that is to say the intersection of a conic and its corresponding straight line, n has to coincide with either m or m' . To form the equation of these intersections, Chasles eliminates $O\mu$ and Om' from the equations above.

Since $Om' = 2O\mu - Om$ (as μ is the center of mm'), the first equation can be rewritten as $\overline{Om}^2 - 2 \cdot Om \cdot O\mu + \nu = 0$, so that Om' does not appear anymore. However, Chasles does not explicitly describe how the elimination of $O\mu$ ought to be carried out. One possible solution is to rewrite this last equation as $O\mu \cdot Om = \frac{1}{2}(\overline{Om}^2 + \nu)$, and to multiply both sides of the four-term equation for homographic divisions by Om , which yields:

$$(O\mu \cdot Om) \cdot On + \alpha \cdot (O\mu \cdot Om) + \beta \cdot On \cdot Om + \gamma \cdot Om = 0$$

After a simple substitution of the products $O\mu \cdot Om$, this equation yields:

$$\frac{\overline{Om}^2 \cdot On}{2} + \frac{\alpha \overline{Om}^2}{2} + \beta \cdot On \cdot Om + \frac{\nu}{2} \cdot On + \gamma \cdot Om + \frac{\alpha \nu}{2} = 0$$

In the general equation thusly obtained, only Om and On are present. Thus, it suffices

³⁶ O is the only point of the division which corresponds to itself.

now to equate $On = Om$ to find the points n which are on the locus being sought. The following equation is then formed:

$$\overline{Om}^3 + (\alpha + 2\beta)\overline{Om}^2 + (2\gamma + \nu)Om + \alpha\nu = 0$$

This is an equation of the third degree. Hence, there are exactly three points n which possess the aforementioned property. This proves that the locus of the intersections of corresponding conics and rays is indeed a cubic curve.

There only remains to prove that the curve thusly described passes through the four common points of the conics as well as P , which Chasles claims “can be acknowledged without any difficulty”. Indeed, when the ray drawn from P intersects one of the four common points, that point is necessarily one of the points of the corresponding conic, hence it is part of the locus formed by the intersections of the rays and the conics. Similarly, when the conic intersects P , the corresponding ray will of course pass through P as well, therefore P is on the cubic.

At this point, the sheaf of conics is entirely determined by four of the nine given points, but P remains indeterminate, as well as the correspondence between rays and conics. It is this correspondence that Chasles further specifies in order to force the resulting cubic to pass through more of the given points. To that end, Chasles forms a series of conics which he denotes $C, C', C'' \dots$, and which pass through the four points a, b, c, d , and so that C, C', C'' respectively pass through E, F, G . Choosing a point P arbitrarily, Chasles associates the conics C, C', C'' to the rays Pe, Pf, Pg . This defines one and only one homographic correspondence between the conics passing through a, b, c, d and the rays passing through P . This correspondence ensures that e, f, g will be on the intersection of the rays Pe, Pf, Pg and their corresponding conics, that is to say that e, f, g are points of the curve of the third degree constructed via the theorem above. Moreover, a, b, c, d are also part of this locus, per the end of the theorem, and so is P . Thus, there remains only to choose P so that h and i are points of the curve.

Chasles knows *a priori* that this can be done, for the position of P can be always determined so that two conditions are satisfied³⁷. In other words, a point is uniquely determined by two conditions (for instance, by two straight lines on which it must lie), so that P can be determined by the conditions that the curve pass through h and i . For the cubic to pass through h , it suffices to choose P such that C''' , the conic in the sheaf passing through h , corresponds to Ph . Per construction, C''' corresponds to a ray r such that the cross-ratio $[Pe, Pf, Pg, r]$ is equal to the cross-ratio $[C, C', C'', C''']$, which itself can be defined as the cross-ratio of the tangents to these conics at, for instance, point a .

³⁷ [Chasles, 1853a], p.949.

In other words, P can be chosen as a point such satisfying an equation of the form

$$[Pe, Pf, Pg, Ph] = \lambda = [C, C', C'', C''']$$

Such an equation (for given values of e, f, g, h, λ) defines a conic (it is easy to see that it is a quadratic equation in P), which can be constructed. Similarly, one can construct a conic determined by $e, f, g, i, [C, C', C'', C^{iv}]$, where C^{iv} is the conic passing through a, b, c, d, i , and such that choosing P on this conic ensures that the cubic generated passes through i . It suffices to choose P among the four intersection points of these two conics to obtain a correspondence which generates a cubic passing through all nine given points. Note that the point P is (not uniquely) determined by all nine given points and belongs to the cubic as well; in that sense, the construction does indeed rest on a property of ten points of a cubic.

The second construction given by Chasles³⁸ involves a property of the curves of the fourth degree, whereby Chasles claims to make a new use of the notion of cross-ratio, and to give a “natural extension” of the general theorem used in the first construction. More precisely, Chasles states at the onset of his communication that he will form a curve of the fourth degree as the intersection of two sheaves of conics, which is more general a construction than that of curve of the third degree given above, which was formed as the intersection of a sheaf of conics and a sheaf of lines. To carry out this construction, as we have seen, Chasles had to first establish a anharmonic correspondence between the sheaf of conics and the sheaf of lines: this was done using the fact that the polar lines of a fixed point with regard to the conics had the same cross-ratio wherever the fixed point was. Transforming once more his geometrical language, Chasles proposes in this second communication to simply use the expression “cross-ratio of four conics” to denote the cross-ratio of these four polar lines of some fixed point. With this extension of the terminology, it becomes possible to speak of two sheaves of conics which correspond to each other so that the respective anharmonic ratios of four curves of each sheaf are the same. Chasles can then state another result which he also calls “Théorème Général”, namely the fact that, for any two sheaves of conics passing through four given points, respectively a, b, c, d and a', b', c', d' , the intersections of corresponding conics will form a curve of order four, which passes through these 8 given points.

Chasles then explains that a proof similar to the one given above can be obtained as well, by fixing a line L and showing that it crosses the locus of the intersections at exactly four points. However, he elects to use another method, which he claims “has the benefit of being applicable to the general case of two sheaves of curves of any order³⁹”. Writing $S =$

³⁸ [Chasles, 1853b].

³⁹ [Chasles, 1853b], p.273.

$0, S_1 = 0; U = 0, U_1 = 0$ the equations for two conics of each sheaf, respectively, Chasles then states that a third conic of each sheaf will be given by the equations (respectively)

$$S + \lambda S_1 = 0 ; U + \lambda' U_1 = 0$$

To prove the result stated above, Chasles must show that a relation holds between λ and λ' , which is so that to one value of α corresponds exactly one value of α' . To that purpose, Chasles notes that one value of λ determines exactly one conic of the first sheaf, and therefore exactly one tangent to this conic at one of the four points which determine this first sheaf. Conversely, one tangent line at this point determines exactly one conic of the first sheaf, and therefore one value of λ . Similarly, to one value of λ' corresponds exactly one tangent line to a conic of the second sheaf, at one of four points. Therefore, the coefficients λ, λ' determine two tangents, which form two homographic sheaves of lines, per construction. Chasles continues by using once more the fact that the homography of two sheaves is characterized by the fact that to one of the lines of the first sheaf corresponds exactly one line of the second sheaf. Hence, he continues, between these two variables λ, λ' must hold an “analytical relation” which is such that to one value of λ , corresponds exactly one value of λ' , that is to say a relation of the form:

$$\alpha \cdot \lambda \lambda' + \beta \cdot \lambda + \gamma \cdot \lambda' + \delta = 0$$

In a footnote, Chasles comments on this reasoning⁴⁰:

Ce mode de démonstration, qui comporte la rigueur désirable, et qui dispense de tout calcul, pourra être employé dans beaucoup de questions il forme, à cet égard, une des applications les plus utiles de la théorie du rapport anharmonique.

Indeed, no fewer than two years later, this would form the basis of a new principle of geometry, namely the principle of anharmonic correspondence, which we discuss in section 3.1.3.

Chasles then remarks that, if S corresponds to U , and S_1 to U_1 , then $\alpha = \delta = 0$, and the equations for the systems of conics can be rewritten so that λ' does not appear anymore. Eliminating λ in this system, he obtains the following equation for the locus of the intersection points of corresponding conics:

$$a \cdot S U_1 = U S_1$$

where a is a constant number. This concludes Chasles’ proof of the “Théorème général”

⁴⁰ [Chasles, 1853b], p.274.

stated above. A construction of a curve of the third degree passing through nine given points is then given by Chasles. First, he considers the conics⁴¹ $A, A', A''..$ which pass through four given points. Fixing a line L , each conic A intersects L at two unnamed points. A conic B can be constructed passing through these two points, plus three given points a', b', c' . The conics A and B intersect at two points which are not the unnamed points mentioned previously. By virtue of the theorem stated above⁴², these two points plus the unnamed points on L form a curve of the fourth degree, which passes through a, b, c, d, a', b', c' . However, the line L is a branch of this curve. Therefore, the two points which were not on L form a curve of the third degree, which passes through the seven given points. In a similar fashion to the first construction, Chasles ends his communication by showing how L can be chosen so that the two remaining given points are on the constructed curve⁴³.

Unbeknownst to Chasles, his second method of proof resembles one which Otto Hesse had devised at around the same time⁴⁴. For instance, with regards to the simpler theorem which describes conic sections as the locus of the intersection of two homographic pencils of lines, Hesse wrote the following system of equations:

$$\begin{aligned} C_1 + \lambda C_2 &= 0 \\ C'_1 + \lambda C'_2 &= 0 \end{aligned}$$

in which, it must be noted, the parameter λ is immediately taken to be the same for both sheaves. Hesse then would find the intersections of corresponding lines by eliminating λ , and the locus of these intersections is expressed by the following equation:

$$C_1 C'_2 - C'_1 C_2 = 0$$

which is an polynomial of degree 2, hence this locus is a conic section. In a similar fashion, writing the equation of an homographic pencil of lines and a sheaf of conics, a polynomial of degree 3 is obtained. This literary technology has several benefits, among which the capacity to ‘select’ one member of a collection and its corresponding member(s) in other series of figures, simply by choosing a value for the parameter λ . However, it fails on other accounts, as for instance it does not seem to allow for the sort of fine-tuned results

⁴¹Here, he does not refer to these conics as a sheaf, or as a series, but as “plusieurs coniques $A, A', A''..$ ”, which not only shows that the thematisation of the notion of systems of conics is still ongoing, but also that he only talks about sheaves when a certain anharmonic correspondence is considered.

⁴²For the hypotheses of the theorem to hold, Chasles only has to prove that the conics B pass through four common points, and that the conics A and B form two homographic sheaves, which he does, see [Chasles, 1853b], p.276.

⁴³ [Chasles, 1853b], p.277.

⁴⁴See for instance [Hesse, 1853].

that Chasles aims for, when giving general modes of construction of cubic curves. Hesse did not particularly care for the problems of construction which Chasles was taking up; as such, he could assume the correspondence to be generic, and not writing the equations for both sheaves with different parameters λ and λ' . In Chasles' approach, however, the correspondence must be constructed with specific goals in mind: to force the resulting curve to pass through certain points.

Through the vocabulary of correspondences and homographies, Chasles was crafting a textual device to perform complex, abstract operations, such as the indexing of elements of infinite collections in a way that would keep track of certain 'matches' between them. There are many other textual devices at our disposal today to do such a thing, whether one prefers to use indexes, function symbols, or Hesse's equations and their λ 's. It is certainly difficult to acutely measure the relative powers of these devices. Remarkably, Chasles and his followers would constantly revise theirs, and come up with new ways of writing theorems, proofs, and constructions. One key step, which we now turn to, would be the identification of correspondences as the main component of this new geometry they were creating. In so doing, they would enunciate the first propositions about correspondences *per se*, instead of merely constructing them.

3.1.3 The principle of anharmonic correspondence

Two years after giving these constructions of the cubics, and as he was using similar techniques to construct geometrical solutions of equations of degree three and four, Chasles returned to the Académie des Sciences to give another communication. This time, however, he would not attempt to solve a particular problem: he had something far more important in mind. On December 24th 1855, Chasles announced he had found a new principle of geometry, namely the principle of correspondence⁴⁵.

We pointed out in section 1.2.3 that the word "principle" is somewhat ambiguous in Chasles' texts. Sometimes, it is used to denote these fundamental truths around which a certain theory is to be centered, like the involution of six points on a conic for the theory of this curve. With the principle of correspondence, however, Chasles is using this term in the same way as he did when discussing the principle of signs, the principle of duality, or the principle of homography. These principles all serve to structure geometrical discourse as a whole.

Indeed, these principles operate outside the deductive order. The principle of duality, for instance, is very rarely stated, used, or even mentioned within Chasles' proofs –

⁴⁵In 1864, in the context of the theory of characteristics, Chasles would use the same term to refer to a more general principle; see sections 4.2.2, 4.3.2. When confusions can be made, we shall use "principle of anharmonic correspondence" to refer to the 1855 principle, and simply "principle of correspondence" for the 1864 version. On the relation between both principles, see 5.1.2.

despite being largely discussed in the context of his historical or epistemological writings. However, it structures the very organization of Chasles' treatises or articles: in the 1852 *Traité*, for instance, each chapter is divided into two parts, the first of which discusses at lengths properties of certain systems of points, such as homographic divisions of a line. The second part, on the other hand, would systematically expound the correlative properties of systems of lines, such as homographic sheaves of lines, with similar wordings being used to display the correlation of the properties. In most cases, only the proofs of the first section are spelled out, as precisely the principle of duality ensures that these first proofs need only be translated word by word to be valid in the realm of lines. Hence, a principle such as the principle of duality is never used as a deductive step, as a rule of inference that would allow Chasles to derive properties of lines from properties of points. Similarly, the principle of signs operated within the mathematical texts for the two constructions of the cubic determined by nine points studied previously, but in a way that never requires Chasles to make its use explicit.

For that reason, these principles are the only results which Chasles presents, and whose scope is not limited to a particular kind of figure: the principle of duality, Chasles had asserted in the *Aperçu Historique*, is in fact a property of extension, or perhaps even of Nature itself⁴⁶. In section 1.1, we showed how the search for a minimal set of universally applicable principles was a core tenet of the engineering *ethos* shaped and transmitted at the École Polytechnique, and one to which Chasles largely participated. The principle of correspondence was, therefore, an important discovery for Chasles, and one which he immediately said would present a quasi-infinite number of possible applications. The justification for the universality of this principle, Chasles explained, was that the principle pertained to “two variable objects”, without requiring any further determination⁴⁷:

C'est à raison du grand nombre et de la variété des applications dont ces propositions [in which the principle is contained] sont susceptibles, que j'ai pensé pouvoir les présenter sous ce titre de *principe*, qu'on a si rarement lieu d'employer en Géométrie. Car on rencontre dans l'Analyse et dans la Mécanique de ces propositions générales qui ramènent à une même expression des questions fort diverses ou d'un genre déterminé, et auxquelles on donne pour cette raison le nom de *principe* ou de *loi*; mais ces propositions abstraites et d'une grande portée manquent à la Géométrie, dont les spéculations ont presque toujours le caractère concret, si l'on excepte, toutefois, quelques méthodes de transformation et surtout cette grande *loi de dualité* mise en évidence par la belle méthode des *polaires réciproques* de M. Poncelet,

⁴⁶ [Chasles, 1837], pp.408-416.

⁴⁷ [Chasles, 1855b], pp.1097-1098. Underlining in the original text.

et qui a été si utile aux progrès de la Géométrie moderne.

Many principles, in Chasles' geometrical practice, are not directly used within proofs. Rather, what appears in proofs are propositions within which principles are “contained”: in the case of duality, for instance, Chasles presents in Note XXIX of the *Aperçu Historique* a theorem “wherefrom the principle of duality results⁴⁸”. The theorem in question is hardly equivalent to the general principle of duality: for one, it expresses a property on the geometry of space, while the principle of duality also applies to planar figures. In fact, what Chasles gave was one general theorem which could serve to apply the universal principle of duality in certain circumstances.

Similar, in the case of the principle of correspondence, Chasles immediately stated that it was to be “contained in two abstract propositions”. The first one is the following⁴⁹:

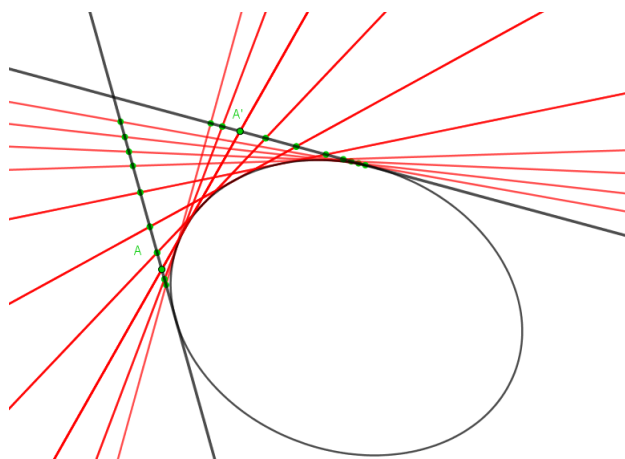
PREMIÈRE PROPOSITION. – *Quand on a à considérer dans une question où n'entrent pas de transcendentes (fonctions, ou courbes), deux séries de points sur deux droites, ou sur une seule, et que l'on démontre que les relations ou dépendances qui ont lieu entre les points qui se correspondent dans ces deux séries, en vertu des données de la question, sont telles, qu'à un point de la première série ne correspond qu'un point dans la seconde, et réciproquement qu'à un point de la seconde série ne correspond qu'un point de la première, alors on peut conclure que les deux séries de points sont homographiques, et par conséquent, que le rapport anharmonique de quatre points de la première est égal à celui des quatre points correspondants de la seconde.*

Ce que nous exprimerons en disant simplement que les points de deux séries se correspondent deux à deux *anharmoniquement*.

Chasles immediately gave a simple example of such a correspondence: a conic section and two of its tangents being given, if a mobile tangent glides over the conic section, it will intersect the two given tangents in two points a, a' , in a $(1, 1)$ correspondence. Hence, this construction gives rise to a homographic division of the two given tangent lines (see fig. below). The proposition serves to go from the construction of a correspondence to an important property thereof.

⁴⁸ [Chasles, 1837], pp.375-376.

⁴⁹“In a question where no transcendental functions or curves are involved, when one has to consider two series of points on two lines, or on one line, and when one shows that the relations or dependencies between the points which correspond to each other in these two series, by virtue of the given data of the question, are so that to a point of the first series corresponds only one point of the second series, and conversely to a point of the second series corresponds only one point of the first series, then one can conclude that these two series are homographic, and, therefore, the cross-ratio of four points of the first series is equal to that of the four corresponding points of the second series. This will be expressed simply by saying that the points of both series correspond to each other two-by-two anharmonically”, [Chasles, 1855b], p.1098.

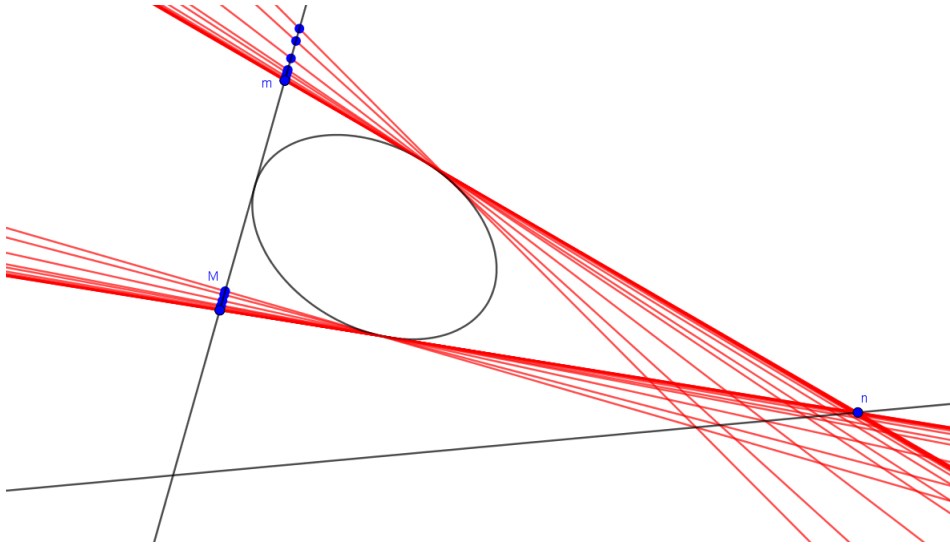


The second proposition which contains the principle of correspondence pertains to $(1, 2)$ -correspondences, that is to say correspondences between series of points and series of segments⁵⁰:

DEUXIÈME PROPOSITION. – *Quand on a à considérer dans une question où n'entrent pas de transcendentes (fonctions, ou courbes), deux séries de points sur deux droites, ou sur une seule, et que l'on démontre que, d'après les relations entre ces points résultantes des conditions de la question, à un point de la première série ne correspond qu'un point de la seconde, mais qu'à un point de la seconde série correspondent simultanément et indistinctement deux points dans la première, on en conclura alors que tous ces couples de points sont en involution, et qu'ils correspondent anharmoniquement aux points uniques de la seconde série.*

Chasles then gave an example analogous to the previous one. Suppose that a straight line, a conic section, and a tangent line to the conic are given. From each point n of the straight line, one can draw two tangent lines to the conic, which will intersect the given tangent line in two points m, M (see fig. below). To each point n correspond two points m, M ; but to one point m corresponds only one point n . Therefore, Chasles claimed, the pairs of points m, M are in involution and correspond anharmonically to the points n .

⁵⁰“In a question where no transcendent functions of curves are involved, when one has to consider two series of points on two lines, or on one line, and when one shows that the relations or dependencies between the points which correspond to each other in these two series, by virtue of the given data of the question, are so that to a point of the first series corresponds only one point of the second series, but that to a point of the second series correspond simultaneously two points of the first series, then one can conclude that all these pairs of points are in involution, and that they correspond anharmonically to the unique points in the second series”, [Chasles, 1855b], pp.1099-1100.



While Chasles gave no proofs of these propositions, such proofs can be found in the *Mélanges de Géométrie Pure* written in 1856 by his friend and student Ernest de Jonquières, whose work will be discussed in greater detail in section 3.2. In fact, in this book, an entire chapter is devoted to the principle of anharmonic correspondence⁵¹, and to various applications thereof (including the theory of curves of order three and four). A footnote at the beginning of this chapter tells us that Chasles was teaching the principle of anharmonic correspondence before publishing this note, as De Jonquières wrote his own chapter before December 1855. This pattern of research being first delivered through lectures, and only then written and communicated to the Académie des Sciences seems rather frequent in Chasles’s scientific activity.

De Jonquières proved Chasles’ first proposition by considering two series of points m and m' taken on two different lines, and by fixing one arbitrary origin A and B on each straight line⁵². From these points, two variables Am and Bm' can be formed. The correspondence between the points m and m' , De Jonquières explains, translates into a “purely algebraic equation” in the two variables, that is to say that there is a polynomial in Am and Bm' that is constantly null. This is because the construction of the correspondence is purely geometrical, and no transcendental curve has been mobilized. This algebraic equation, however, must be so that to any one value of the first variable corresponds exactly one value of the second variable; and conversely. Therefore, this equation is necessarily

$$Am \cdot Bm' + \lambda \cdot Am + \mu Bm' + \nu = 0$$

for some λ, μ, ν depending on the specific correspondence being considered. Note that this equation is precisely the four-term equation for homographic divisions of two lines

⁵¹ [de Jonquières, 1856a], pp.152-196.

⁵² [de Jonquières, 1856a], p.154.

which Chasles had already given in the *Traité de Géométrie Supérieure*⁵³.

As for the second proposition containing the principle of (anharmonic) correspondence, De Jonquières lets M denote the series of points, and m, m' the corresponding points of the second series⁵⁴. The series of points m and m' are in fact homographic according to the first proposition, since to one point m there corresponds a single point m' , and conversely. This homography is in fact an involution, since m and m' play symmetrical roles: indeed, because m and m' simultaneously correspond to a single point M , each point of the line mm' , when viewed alternatively as an element of both divisions, has the same homolog. Therefore, the segments mm' are in involution, and the proposition is proven.

These proofs are remarkably simple: in fact, they are probably shorter than any direct proof of the fact that a specific correspondence is anharmonic (or a series of segments in involution) could possibly be. But what Chasles' 1855 paper brings to the fore is not a new set of results and theorems that were previously unattainable. Rather, with the principle of correspondence comes a new way of ordering and relating the elementary concepts of higher geometry, as well as a new way of writing these proofs. Proofs relying on the principle of correspondence minimize the uses of specific properties of a given system of figures involved in the condition of a problem, by simply requiring that one surveys the nature of the correspondence (that is to say, the number of corresponding points in both directions).

This principle, despite or perhaps due to its relative simplicity, profoundly changed Chasles' geometry of curves and surfaces. In 1857, he gave another communication, on “Two general theorems on the geometrical curves and surfaces of any order⁵⁵”, wherein anharmonic correspondences between sheaves of curves of orders n and n' (determined respectively by n^2, n'^2 points) are used to form curves of order $(n+n')$, in a similar manner to what had been done in 1853 for conics. An anharmonic correspondence between curves here means that there is a one-to-one correspondence between them, such that the cross-ratio of any four curves of one sheaf is equal to that of the four corresponding curves.

However, there is one major difference between these two texts, in which the principle of correspondence plays a subtle yet important role. Indeed, defining homographic sheaves of curves or surfaces can now be done in a novel manner. In 1853, Chasles had to construct auxiliary sheaves of lines which corresponded to the sheaves of conics, such that their cross-ratios would yield homographic equations that allowed for a transfer of properties. In 1857, for instance in the case of surfaces of order m , this can be done just by writing

⁵³ [Chasles, 1852], p.88. See also section 2.2.2.

⁵⁴ [de Jonquières, 1856a], p.155.

⁵⁵ [Chasles, 1857a].

the following “general equation” of these surfaces

$$A_m + \lambda B_m = 0$$

and the cross-ratio of four surfaces will be defined simply by the cross-ratio of the four points on a line whose abscisses are the four corresponding parameter λ with respect to an arbitrary origin – that is to say, as the ratio

$$\frac{\lambda - \lambda''}{\lambda - \lambda'''} : \frac{\lambda' - \lambda''}{\lambda' - \lambda'''}$$

The possibility of treating sheaves of curves or surfaces in this manner, Chasles claims without further comment, is a natural application of the principle of anharmonic correspondence. More generally, the principle of correspondence allows manipulations on corresponding elements of sheaves of curves, lines, or points without having to form the equations for said correspondences. For this reason, correspondences would form the basis of a new language for the writing of the general and geometrical equations of curves and surfaces, as we shall see in section 3.2.

3.1.4 Intermezzo: A Dangerous Theorem?

In his 1997 autobiography *Un mathématicien aux prises avec le siècle*, French mathematician Laurent Schwartz (1915-2002) tells the story of his first mathematical publication⁵⁶. While still a student in Classes Préparatoires at Lycée Louis-Le-Grand, he had stumbled upon an apparently solid proof of an otherwise obviously false theorem, namely that all real lines of the plane go through a common point. His proof, whose flaw he had a hard time detecting, went as follows⁵⁷:

⁵⁶ [Schwartz, 1997], pp.64-65. On the relationship between Schwartz and Hadamard, see [Paumier, 2014], pp.31-32.

⁵⁷“Let D and D' be two imaginary conjugate lines. Their intersection point is therefore real. If M and M' are distinct, complex conjugate points on D and D' respectively, then the line MM' is real. Conversely, every real line crosses D and D' at complex conjugate points. We have there an algebraic biunivocal relation between points D, D' . It is homographic by virtue of a known theorem. However, if we have a homographic correspondence (M, M') between the points of two lines, and if the intersection point O of these lines corresponds to itself, which is the case since it is real, the line MM' goes through a fixed point. Therefore all real lines of a plane go through a fixed point. [...] I asked this question to Coissard, who did not think otherwise: ‘The correspondence is algebraic, it is obvious, therefore..’. Coissard, being cautious, told me: ‘I do not see the mistake, I will think about it and answer you tomorrow’. And the day after, he indeed had found out that the correspondence between M and M' , imaginary conjugate points, was absolutely not algebraic. I then showed the trick to Paul Lévy and Jacques Hadamard who, obviously, immediately noticed the flaw. Jacques Hadamard was thrilled, and indeed thought that the theorem ‘An algebraic, biunivocal correspondence is homographic’ should not be taught to students in spéciales, who had to learn how to show that a correspondence is homographic in a direct manner, with theorems on the stability of these correspondences. On the contrary, these students are not capable of seeing whether a correspondence is algebraic or not. He then campaigned for this theorem to be withdrawn from curricula,

Soient D et D' deux droites imaginaires conjuguées. Leur point d'intersection est alors réel. Si M et M' sont complexes conjugués distincts sur D et D' respectivement, la droite MM' est réelle, et inversement toute droite réelle coupe D et D' en des points complexes conjugués. On a là une relation algébrique et biunivoque entre les points des deux droites D, D' . Elle est donc homographique en vertu d'un théorème connu. Mais, si l'on a une correspondance homographique (M, M') entre les points de deux droites, et si le point d'intersection O des deux droites se correspond à lui-même, ce qui est le cas puisqu'il est réel, la droite MM' passe par un point fixe. Donc toutes les droites réelles d'un plan passent par un point fixe. [...] Je posai la question à Coissard [his professor] qui ne pensait pas différemment: «La correspondance est algébrique, c'est évident, et elle est biunivoque aussi, donc...». Coissard, prudent, me dit: 'Je ne vois pas la faute, j'y réfléchirai et je vous répondrai demain.' Et le lendemain effectivement, il avait trouvé que la correspondance entre M et M' , points imaginaires conjugués, n'est absolument pas une correspondance algébrique. Je montrai ensuite l'astuce à Paul Lévy et à Jacques Hadamard qui, évidemment, repérèrent immédiatement la faute. Jacques Hadamard en était enchanté, et pensait en effet que ce théorème: 'Une correspondance algébrique et biunivoque est homographique' ne devrait pas être enseigné aux élèves des classes de spéciales [second year of classes préparatoires], qui devraient apprendre à montrer qu'une correspondance est homographique de façon directe, par des théorèmes de stabilité sur ces correspondances. En revanche, ils ne sont pas capables de voir si une correspondance est algébrique ou non. Il fit donc campagne pour le rejet de ce théorème de l'enseignement et décida de le publier avec sa démonstration, afin qu'on ne l'enseignât plus en taupe [slang term for classes préparatoires].

In Schwartz' remembrance of these events, Hadamard's warning against this theorem which shouldn't be taught to young, untrained mathematicians echoes earlier warnings made by his professor, Coissard. Coissard mistrusted "algebra" (by which, according to Schwartz, he meant analysis), where intuition leads only to pitfalls, and where one must always tread with the utmost care. Schwartz, who enjoyed geometry more than anything else in his youth but went on to become a renowned analyst, tells us through this anecdote the shaping of his mathematical taste: he who had no skills whatsoever as far as visualisation and graphical intuition were concerned, enjoyed geometry primarily as a purely verbal, formal game, hence his later conversion to analysis.

While Schwartz gives no reference for this supposedly well-known theorem, the simultaneous decision to publish it with its proof, so that it would not be taught in classes préparatoires anymore".

ilarity with Chasles' principle of correspondence is striking. For Schwartz, the theorem states that if there is a binary, bijective and algebraic relation \mathcal{R} between the points M and M' of two complex projective lines, then M' is the image of M under an homographic mapping. Here, \mathcal{R} is called algebraic if there is a projective frame for each of these lines and a polynomial $P(X, X', Y, Y')$ homogenous in (X, Y) and in (X', Y') such that $(X, Y)\mathcal{R}(X', Y')$ iif $P(X, X', Y, Y') = 0$. It seems unproblematic to identify both statements. Despite a few complications in Chasles' terminology, the mathematical content of both theorems is, in a sense, equivalent. This equivalence, however, says very little about the drastically different uses that both mathematicians were able or inclined to make of this theorem, as well as about their respective evaluation of the epistemological merits and dangers of this result.

Indeed, what Hadamard and Coissard found dangerous or tricky, Chasles had used countless times without fail, and put at the center of his teaching. In many ways, the new perceptions elicited by a seemingly unchanging theorem betray a growing anxiety within mathematical practice⁵⁸: the generality of analysis, for Schwartz's professors, is constantly under threat of being refuted by counter-examples, whose presence is not always obvious. Schwartz's discovery also highlights the power of the concept of base-fields in geometrical studies: in modern terms, the paradox he had discovered was that the complex conjugation $(X, Y) \rightarrow (\bar{X}, \bar{Y})$ is not algebraic *over the field of complex numbers*, and as such cannot give rise to an algebraic relation.

But why is it that Chasles and his readers never made such a mistake, or even felt the need to warn against the possibility thereof? Schwartz's retelling pivots around the fact that what seemed obviously algebraic actually wasn't. This manipulation of correspondences as abstract relations (or mappings) opens the possibility of such a mistake, exemplified by the construction of something that is within the realm of relations, but without that of the relations that satisfy the proper hypotheses.

The faulty construction which Schwartz' "proof" builds upon was not unthinkable in Chasles' times, nor was such a mistake impossible to arrive to: indeed, comparable criticism was published for instance by C. F. Geiser, a Swiss geometer who edited Steiner's collected works⁵⁹:

Ma si può muovere un dubbio sul procedimento della dimostrazione ; poichè esso parte dalla tacita supposizione, che ogni relazione univoca fra λ e λ' si possa rappresentare analiticamente ; ipotesi questa che in altre parti della

⁵⁸On anxiety as a historical category, especially in nineteenth-century mathematics, see [Gray, 2004].

⁵⁹"But a doubt can be raised about the procedure of the demonstration; since it begins with the tacit assumption, that every univocal relation between λ and λ' can be represented analytically; hypothesis which in other parts of Mathematics is long recognized as inadmissible. In fact, this doubt is not unfounded even in the present case, which will be detected by some simple examples that we will discuss in this Note", [Geiser, 1870], p.26.

Matematica è da molto tempo riconosciuta come inammissibile. Che in fatto il dubbio non sia infondato anche nel presente caso, lo si rileverà da alcuni semplici esempi che tratteremo in questa Nota.

Geiser went on to discuss similar cases where correspondences which cannot be represented analytically, that is to say non-algebraic biunivocal correspondences, lead to counter-examples of Chasles' principle of correspondence. Geiser was very much in the minority in holding this principle faulty: Zeuthen, himself a student of Chasles', and someone who would make use of this principle at several occasions, quickly dismissed this criticism. He pointed to Chasles' explicit mention of the necessity of the correspondence not involving transcendent functions or curves⁶⁰, a condition which he described as being "inseparable from the principle itself". At this time, the principle was indeed used by several geometers across Europe, either explicitly crediting Chasles for its creation⁶¹, or just reproducing the main equation without giving any reference⁶². For a majority of these mathematicians, the question of whether or not a certain correspondence was indeed algebraic was not something to verify in each case, lest a counter-example may arise. In fact, the algebraic nature of correspondences is never something that is actually proven or even mentioned by Chasles or his students: it is a core part of the principle, because, for Chasles and his students, correspondences were always obtained within a framework structured by a tacit, finite set of constructions – those of polar lines, of projections, of intersections etc.

Chasles' principle of correspondence moving from the status of abstract principle for a simple and intuitive practice of geometry, to that of a dangerous theorem which should only be placed in the hands of expert mathematicians, clearly shows how a mathematical theorem is never just a pure piece of knowledge, to be circulated amongst the members of an ideal community of pure minds. Rather, it is to be thought of as a technology to be used within a certain mathematical laboratory, from which derive its purpose, its meaning, but also some of the rules for its operation. When transported from one laboratory to another, tacit information regarding its use may be lost: therefrom came the increased dangerousity of Chasles' principle of correspondence, when in the hands of mid-20th-century students in Parisian Classes Préparatoires.

3.2 The geometrical equations of curves

Despite his enthusiastic description of this new principle of correspondence in 1855, Chasles would scarcely make explicit uses of and references to it in publications over

⁶⁰ [Zeuthen, 1873], pp.188-189.

⁶¹ [Cremona, 1862], pp.7-10.

⁶² [Hesse, 1866], p.370 *et passim*.

the following decade. In 1864, in the context of the publication of the theory of characteristics (to which we turn in section 4.2), Chasles would reforge this principle into something much more general and powerful – albeit deprived of its fundamental connection to the theory of homographies and cross-ratios. Nonetheless, this first principle of correspondence played a role in the next two books Chasles published, namely his interpretation of Euclid’s *Porisms* in 1860 and the 1865 *Traité des Coniques* (which had been written prior to the 1864 reformulation of the principle of correspondence). De Jonquières, on the other hand, would use this principle extensively, to tackle different kinds of problems.

All these texts, however, converge in that they all collectively put forth the idea that this new higher geometry was able to produce “geometrical equations⁶³” of various figures, curves, and surfaces. In the rest of this chapter, we discuss briefly several attempts by Chasles and De Jonquières at mobilising this constantly evolving notion through the (sometimes implicit) use of the principle of anharmonic correspondence.

3.2.1 Geometry on the seven seas

As we are about to delve deeper into his geometrical research, and his promotion of Chasles’ program for the development of pure geometry, it is necessary to say a few words about Ernest de Fauque de Jonquières⁶⁴. Born in 1820 into a military family (his father was a naval officer before him, and his son would become one as well) in the South-East of France, he entered the *École Navale* at the age of fifteen. He spent the following forty-four years within the French Navy. According to his own count, by the time he was promoted to the rank of Vice-Amiral in 1879 – the highest rank he would ever reach – he had spent thirty-six years on sea, in all parts of the world. This intensive maritime activity, which sent him to all corners of the world, was crucial to his own self-styling as a mathematician, as he would regularly present his work as that of pupil of the great masters, of someone who merely dabbles in geometry.

De Jonquières wrote an autobiographical account as part of a candidacy to become member of the Académie des Sciences. De Jonquières did not apply for the mathematical section of the Académie des Sciences; as he probably would not have had any chance of entering this section at a time where mathematics was increasingly becoming a professional activity, and the preserve of an elite trained either at the *École Polytechnique* or the *École Normale Supérieure*. Instead, De Jonquières (successfully) applied to become a *membre*

⁶³These equations have nothing to do with the geometrical equations discussed by the algebraic geometers studied in [Lê, 2015].

⁶⁴Biographical information on De Jonquières can be found in his own *Notice sur la carrière maritime, administrative et scientifique*, [de Jonquières, 1883], as well as the articles of Gino Loria, [Loria, 1902], [Loria, 1947].

libre of the Académie des Sciences⁶⁵. To that end, he presented himself as someone who was not only versed in mathematics, but also in military science and naval engineering; and as someone who had explored the world extensively, had made natural observations across the world, and had managed important administrative duties. The series of anonymous portraits of the other *membres libres* of the Académie which De Jonquières gave at the end of his own application showcase the various facets of the ideal savant which he viewed as rightful members of this section: these members are pictured as intrepid explorers, administrators, military men etc., who happened to also make important scientific contributions on the side. De Jonquières' self-styling as an amateur mathematician was part of this application strategy⁶⁶.



Ernest de Fauque de Jonquières (1820-1901)

More prosaically, these large periods of time spent at sea regulated his access to scientific journals and books, as well as the time he was able to devote to his geometrical research. The daily activity of a sailor, even this late into the nineteenth century, already involved a use of a certain astronomical knowledge⁶⁷. During his first missions, De Jonquières read with great interest the astronomical theories of Delambre, and conducted several observations himself while at sea. However, it is only in 1845 that De Jonquières was able to read advanced mathematical texts for longer periods of time. During a long and mostly peaceful surveillance mission off the coasts of Sierra-Leone, he read the mechanical works of Poinsot and Poisson.

In 1848, as the third French Revolution unfolded, De Jonquières was back in France. On leave at first, he would stay there for two years, as he had been tasked by the 'Conseil

⁶⁵On the "Académiciens libres", see [Crosland, 1992], pp.405-409.

⁶⁶As such, Loria's biographies and assessments of De Jonquières' work and career must be taken with some distance. This strategy is made explicit in a letter by De Jonquières to Cremona, responding to the latter's surprise that geometrical results did not figure more prominently in this notice, see [Israel, 2017], pp.999-1000.

⁶⁷For the role of Astronomy in the daily activities of sailors toward the end of the 18th century, see [Schotte, 2019], pp.173-183.

d'amirauté' with the upgrading of a military vessel. It is probably at this point that De Jonquières first met the geometrical theories of Chasles and Poncelet, some versions of which he brought with him as he returned to the sea by 1850, eventually taking part in the Crimean War. Afterwards, his service kept him at sea for most of the following years, albeit with more peaceful missions. In 1856, aboard the *Arcole*, he went to Terre-Neuve: it is during this time that he wrote his first mathematical works, including the aforementioned *Mélanges de Géométrie Pure*, which expand on Chasles' kinematics, his (and Poncelet's) interest in MacLaurin's organic description of curves, and, more importantly for us, on the principle of anharmonic correspondence. Between 1856 and 1861, De Jonquières would publish several papers in various European journals, such as Crelle's and Liouville's journals or the *Nouvelles Annales de Mathématiques*. De Jonquières was, for obvious reasons, incapable of attending the majority of Chasles' lectures. In fact, during his controversy with Chasles in 1866, he would claim to have attended a mere four or five such lectures. However, he was in possession of handwritten versions of some of these lectures.

Throughout his career, the rhythm of his scientific publications would be the inverse of that of his military and administrative duties. Toward the end of the year 1861, De Jonquières was sent to participate to the *Expédition du Mexique*. More precisely, he was to watch over the shores of Vera Cruz, and the Tampico river. This campaign would be described by De Jonquières as a "monotonous cruise": far from most of the military action, he had the time to write and send an entry for the Grand Prix de Mathématiques of the Académie des Sciences. This entry would be rewarded with two thirds of the prize-money. A few stays in France, as well as an administrative mission in Saïgon, left him with enough time to develop a theory of curves which would, for a time, clash with that of Chasles (see section 5.1). By contrast, after De Jonquières' promotion to the rank of captain, in 1866, constant military duties would prevent him from publishing any more mathematics until 1878.

In what follows, we focus on the research he conducted between 1855 and 1859, that is to say, as he was expanding on Chasles' higher geometry.

3.2.2 The generation of curves

In 1856, De Jonquières wrote several texts on the generation of curves. He sent one memoir to the Académie des Sciences, for which Chasles and Poncelet wrote a positive report and which was eventually published in 1858 in the *Mémoires présentés par divers savants à l'Académie des sciences*⁶⁸. De Jonquières also wrote four papers for Liouville's Journal on

⁶⁸ [de Jonquières, 1858]. For Chasles' report, see [Chasles, 1857b].

the basis of this very memoir⁶⁹. This interest in the generation of curves, and in particular of those of order four, is clearly in line with what Chasles was doing at the time. The *Mélanges* already were a book entirely devoted to the promotion and expansion of Chasles' newest methods, as well as to a translation⁷⁰ of text, namely MacLaurin's *Geometria Organica*, which Chasles had described as crucial for the historical development of this theory.

To construct and describe curves of order four, determined by fourteen points, De Jonquières relies on sheaves of conics, just as Chasles did for cubics. Chasles had justified this use of conics on methodological grounds⁷¹:

On pense bien, sans qu'il soit besoin de le dire, que les sections coniques jouent un rôle nécessaire dans nos nouvelles constructions, comme dans celles de la Géométrie analytique. Employer d'autres courbes d'un ordre supérieur, serait une faute de méthode, d'autant plus grave, qu'on peut dire que la destination philosophique et essentielle des sections coniques, en Géométrie, est précisément la résolution des questions qui admettent trois ou quatre solutions, de même que la propriété essentielle du cercle est de servir à résoudre celles qui en admettent deux seules.

However, De Jonquières would introduce new notations for the consideration of sheaves of conics⁷². A sheaf of conics being determined by four points a, b, c, d , De Jonquières would represent its conics as $(abcde), (abcdf), (abcdg), (abcdh), etc.$, and the sheaf itself as $(abcd)[e, f, g, h, etc.]$. In the papers for *Liouville's Journal*, this notation is slightly different, as numbers instead of letters are used for the parts in brackets. In both cases, $(abcd)$ is called the “basis” (*base*) of the sheaf, while $[e, f, g, h, etc.]$ is its “variable part” (*partie variable*). Similarly, De Jonquières would represent sheaves of curves of order three or four by expanding the numbers of letters in parentheses and in brackets. Sheaves of curves of order three having a double point, for instance a , are represented by $(\dot{a}bcdef)[g, h, i, ..]$; and more dots are added to represent points of higher multiplicity. Furthermore, unknown points can appear in the basis of a sheaf; they are represented by letters from the other end of the alphabet, such as x and y . This is useful to represent Chasles' construction

⁶⁹ [de Jonquières, 1856b], [de Jonquières, 1857a], [de Jonquières, 1857b], [de Jonquières, 1858].

⁷⁰This “translation”, however, contains several notes and additions to MacLaurin's text. Furthermore, De Jonquières replaces several of MacLaurin's proofs with some that rely on Chasles' *Traité de Géométrie Supérieure*. The reason invoked for these changes is a gain of brevity. De Jonquières, like Chasles, reads texts of the past at the light of modern treatises, and what he calls translations are often displacement of past results into more general frameworks.

⁷¹ [Chasles, 1855a], p.678.

⁷²This notation would be praised by Cayley, himself a mathematician who introduced a lot of new notations involving letters and numerals in productive manners. See [Cayley, 1862], pp.223-224. In what follows, we focus on De Jonquières' 1858 presentation of this theory, [de Jonquières, 1858].

of the cubic passing through nine given points: remember, indeed, that Chasles had constructed a sheaf of conics and a sheaf of rays passing through a point P which had been left undetermined until the end of the construction. This point acts as an indeterminate which is to be specified at the end of the construction to satisfy the remaining conditions. In fact, as we shall see, De Jonquières' notations serve to describe in more general terms Chasles' construction, and to extend it to the generation of curves of any order.

Chasles had shown how to define anharmonic correspondences between sheaves of conics and sheaves of lines. Similarly, such correspondences can be defined with sheaves of curves of any order, and sheaves of lines: it suffices to take a regular point of the basis of the first sheaf, and to form the lines tangent to all curves of the sheaf at this very point. From there, De Jonquières defines anharmonic correspondences directly between sheaves of curves: given two sheaves and one regular point of the basis of the first sheaf, the two sheaves are said to be *anharmonic* if any (regular) point of the basis of the second sheaf yields a sheaf of tangent lines that is *anharmonic* to the sheaf of tangent lines formed from the first given point, and the first sheaf of curves. Two such sheaves of curves are also called *homographic* by De Jonquières. However, he reserves this term for sheaves composed of the same kind of figures; that is to say, anharmonic sheaves of lines and sheaves of curves cannot be said to be homographic.

At this point, De Jonquières introduces Chasles' algebraic equations for sheaves, which he calls the “analytical relation between two anharmonic sheaves”. In fact, De Jonquières reproduces most of Chasles' reasoning on these equations, transposing it to sheaves of curves of any order. If

$$S = 0 \text{ and } S_1 = 0$$

are the equations of two curves of degree n , belonging to one sheaf, then the equation of any other curve of this sheaf is

$$S + \lambda S_1 = 0$$

Similarly, De Jonquières forms an equation $V + \lambda' V_1 = 0$ of degree n' for the second sheaf. If the two sheaves are anharmonic, then to one curve of the first sheaf corresponds exactly one of the second sheaf, and conversely. This means that λ and λ' verify an equation of the form

$$\alpha \lambda \lambda' + \beta \lambda + \gamma \lambda' + \delta = 0$$

where $\alpha, \beta, \gamma, \delta$ are constant coefficients, one of which De Jonquières calls “arbitrary” (as the whole equation can be divided by any factor without altering its validity). Choosing the curves of equations S and V (respectively, S_1 and V_1) to correspond to one another, De Jonquières eliminates most of these coefficients, and rewrites the equations of both

sheaves as

$$S + \lambda S_1 = 0 \text{ and } V + \alpha \lambda V_1 = 0$$

De Jonquières then eliminates λ from this system to obtain the equation for the locus of the intersection points of the corresponding curves of the two sheaves, namely

$$\alpha S V_1 = V S_1$$

This is an equation of degree $n + n'$. De Jonquières notes that the resulting equation is satisfied whenever $S = 0$ and $V = 0$ (simultaneously). He has shown that the curve generated by the intersections of two sheaves of curves of order n and n' is a curve of order $n + n'$, passing through all the points in the bases of these sheaves.

This result justifies a priori Chasles' many constructions of curves of order three, as formed by intersections of sheaves of lines and sheaves of conics. However, De Jonquières claims that this result “is only fully satisfactory so long as one does not seek to make the curve thus constructed absolutely particular amongst all those of its degree⁷³”. Indeed, De Jonquières explains, suppose you want to generate a given curve of order m as the intersection of two anharmonic sheaves of curves of order n and n' , where $n + n' = m$. The curve is characterized by $\frac{m(m+3)}{2} = \frac{(n+n')(n+n'+3)}{2}$ given points. Amongst these, $\frac{n(n+3)}{2} + \frac{n'(n'+3)}{2} - 2$ can be taken to form the bases of the two sheaves, and three points can be chosen to form the anharmonic correspondence between the two sheaves. In total, this means that $\frac{n(n+3)}{2} + \frac{n'(n'+3)}{2} + 1$ points have been selected, but this number is smaller than that of the number of points characterizing the curve to construct, whenever n and n' are above 1. Therefore, the curve being thus generated does not necessarily pass through all the given points.

De Jonquières' proposed solution to this problem is to introduce “unknown points” in the bases of the sheaves, so that the points determining the curve to be constructed will uniquely determine these points. This is, in fact, precisely what Chasles had done for cubics. To that end, De Jonquières introduces $nn' - 1$ unknown points to be part of the bases of the sheaves. The K remaining points of these bases will be taken amongst the given points, where

$$K = \frac{n(n+3) + n'(n'+3)}{2} - 1$$

The number of given points remaining at this stage, which De Jonquières calls the “common variable part of both sheaves”, is

$$\frac{m(m+3)}{2} - K = \frac{(n+n')(n+n'+3) - n(n+3) - n'(n'+3)}{2} - 1 = 2nn' + 1$$

⁷³ [de Jonquières, 1858], p.169.

To determine $(nn' - 1)$ unknown points, explains De Jonquières, $2(nn' - 1)$ elements are required: this is for instance the number of coordinates x and y required. Meanwhile, the problem of constructing the curve through the two sheaves thus far considered depends on precisely $2(nn' - 1)$ conditions. Denoting B and B' the bases of these two sheaves, and $a, b, c, d, e, f..$ the $2nn' + 1$ remaining points, these conditions are the $(2nn' + 1) - 3 = 2(nn' - 1)$ following equalities between cross-ratios:

$$B(abcd) = B'(abcd)$$

$$B(abce) = B'(abce)$$

etc.

This concludes De Jonquières' proof: the number of equations is equal to the number of conditions, which means that a given curve of order $n + n'$ can always be generated via two sheaves of curves of order n and n' . Note that there is no unicity in the mode of description: as De Jonquières quickly points out, the variable points can be spread in various ways across the bases of both sheaves, which leads to various equally valid generations.

This mode of reasoning may seem algebraic to us: it resembles the construction of a system of linear equations. And yet, no systems of coordinates or Cartesian equations are introduced. In fact, De Jonquières explicitly describes this theory as a geometrical analog to algebraic equations⁷⁴:

L'objet du présent mémoire est d'exposer un mode général et uniforme de description des courbes géométriques déterminées par un nombre suffisant de points. Cette méthode, véritable *mise en équation* géométrique du problème fait le sujet de la première section du mémoire.

Building on Chasles' latest principle, and his idea that sufficiently fundamental geometrical properties can play the same role as general algebraic equations, De Jonquières had devised a new project: the writing of "geometrical equations". This term would prove key in both geometers' subsequent geometrical works. In the following chapter, we shall see how it played a key role in the shaping of Chasles' theory of characteristics (see 4.2.4 in particular). Similarly, Chasles' *Traité des Sections Coniques* published in 1865 (but whose content was already part of Chasles' teaching at the Faculté des Sciences since its inception⁷⁵) opens with a single "fundamental theorem", wherefrom derive two "fundamental properties" regarding the points and tangents of a conic, which Chasles would

⁷⁴ [de Jonquières, 1858], p.159. Emphasis in original.

⁷⁵ [de Jonquières, 1865d].

later describe as the punctual and tangential equations of conics⁷⁶. The first of these properties, for instance, is stated as follows⁷⁷:

PROPRIÉTÉ FONDAMENTALE RELATIVE AUX POINTS D'UNE CONIQUE. – Si de quatre points d'une conique on mène des droites à un cinquième point de la courbe : le rapport anharmonique de ces droites a une valeur constante, quel que soit le cinquième point.

Here, this recasting of a geometrical property as a general equation is justified by that fact that the first property says something on four given points of a conic, and a fifth variable point. In fact, Descartes' equation of the conic itself is found to be a consequence of this fundamental property⁷⁸. The property of five points is more fundamental, because it resorts to no artificial systems of coordinates: as such, it ought to come first in the methodical exposition of the theory of conic sections.

3.2.3 Divinating Euclid's Lost Porisms

Another key text in which this concept of geometrical equations would be put to work is Chasles' reconstruction of Euclid's lost *Porisms*; a lost text, attributed to Euclid, and often described as a core component of the Greeks' geometrical analysis. Most of what is known of this text comes from Pappus' description in Book VII of the *Collections*⁷⁹. In Chasles' interpretation thereof, the *Porisms* are all specific instances of a property of anharmonic correspondences⁸⁰. Already in Note III of the *Aperçu Historique*, Chasles had written that “porisms were, in Euclid's mind, in some sense, equations of curves⁸¹”. However, as we saw in section 2.1.1, Chasles' interest in Ancient Greek geometrical analysis, and therefore in the *Porisms*, grew throughout the teaching and development of higher geometry. This interest culminated in 1860, when he published a book purporting to restore the full meaning of all of the Porisms. We shall not get into the intricate details of these propositions, their classifications and mysterious wordings in Pappus' *Collections*. What matters to our purpose here is to describe the kind of mathematical propositions porisms had become in Chasles' 1860 reading. Indeed, what Chasles found faulty in previous interpretations of the porisms, and in particular in Simson's, was not just that it did not cover all of the propositions reproduced by Pappus. More profoundly, Chasles

⁷⁶ [Chasles, 1870], p.269.

⁷⁷ [Chasles, 1865b], pp.2-3.

⁷⁸ [Chasles, 1865b], pp.23-24.

⁷⁹ See [Pappus, 1986], pp.547-572, for a translation and a commentary of this description. Chasles probably read Pappus from Commandino's 1588 Latin translation, if the inventory of his personal library, established after his death, is to be trusted; see [Boulland & Claudin, 1881], pp.158-159.

⁸⁰ Chasles' interpretation of the *Porisms* has been convincingly criticized in [Hogendijk, 1987].

⁸¹ [Chasles, 1837], p.276.

claimed, Simson had not searched what “thought had guided the Greek geometer in his original conception⁸²”, that is to say the general nature of these propositions – in other words, he had not seen them as the germs of an alternative to the equations of algebra.

To restore the meaning of the lost porisms, Chasles would rely both on Pappus’ commentary and on the new kind of propositions higher geometry had led him to consider. In particular, the projective property of cross-ratios, as well as the various equations for homographical divisions of a line are invoked as necessary to fully understand Euclid’s propositions. This mathematical knowledge was obviously absent from Simson’s reconstitutions; however, for Chasles, most damaging for Simson was the fact that (pure) geometry in his time had not yet produced propositions of the same kind as Euclid’s porisms⁸³. Pappus’ text on the porisms, of which Chasles gave a complete translation in his book, established a distinction between three kinds of propositions⁸⁴:

Le Théorème est une proposition où l’on demande de démontrer ce qui est proposé. Le Problème est une proposition où l’on demande de construire ce qui est proposé. Le Porisme est une proposition où l’on demande de trouver ce qui est proposé.

To make sense of this tripartition, Chasles proposed to examine it in the case of a specific subcategory of propositions, namely “loci” (*lieux*). Loci are, according to Pappus, part of the porisms. Chasles suggests a tripartition of local propositions analogous to that of Pappus, which involves loci and makes clearer what he understands to be the status of porisms with respect to problems and theorems to be⁸⁵.

“Local theorems”, Chasles claims, are propositions which express a common property of all points of a certain, completely determined, line or curve. An example of such a proposition is the following: on a circle of diameter $[AB]$, consider two points C, D so that $\frac{CA}{CB} = \frac{DA}{DB}$. Then, for all point m on the circle, we have $\frac{mC}{mD} = \frac{AC}{AD}$ (see fig. below). “Loci” are propositions which express the fact that points subordinated to a common, known law (soumis à une loi commune) are on a line (straight line, circular line, or any other sort of line) of which the nature can be given, but which remains to be determined (in position and magnitude). An example of such a proposition is given by Chasles by reworking the previous example: two points and a ratio being given, the locus of a point whose distances to these two points are in this given ratio, is the circumference of a circle, given in position and magnitude. Finally, “local problems” are propositions in which it is

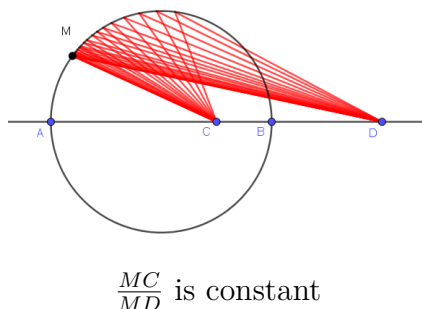
⁸² [Chasles, 1860], pp.8-10.

⁸³ [Chasles, 1860], pp.12-14.

⁸⁴ “The theorem is a proposition where one must prove what is proposed. The problem is a proposition where one must construct what is given. The porisms is a proposition where one must find what is proposed”, [Chasles, 1860], p.15.

⁸⁵ [Chasles, 1860], p.33.

asked to find the nature, magnitude and position of a locus (that is to say a curve) which is the common locus of an infinity of points subordinated to a common law. Once again, an example of such a proposition is obtained by reworking the previous examples. Two points and a ratio being given, the problem of finding the locus of a point whose distances to these two points are in this given ratio is a local problem.



The relation between loci and local theorems is, to Chasles, a metonymy for the relation between porisms and theorems – and similarly, loci and local problems are in the same relation as porisms and problems. Porisms are incomplete theorems; they are “propositions in which a stated truth is to be *proven* and the quality or way of being, such as the magnitude or position, of some things mentioned in the statement of this truth, is to be *found*⁸⁶”. Chasles then explains that one can turn a theorem into a Porism by simplifying its wording, and by doing away with its unnecessary overdeterminations. This, he claims, is precisely what modern geometry does with its statements. Hence, Chasles draws a continuous line from Euclid’s *Data* to Modern Geometry, and in particular his presentation thereof.

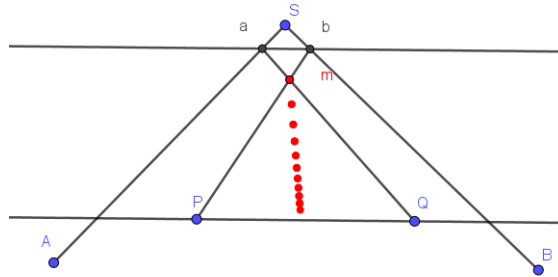
In fact, some of Euclid’s Porisms as reconstructed by Chasles are exact copies of propositions one could already find in the *Traité de Géométrie Supérieure*. The translation and classification of the propositions given by Pappus as being Euclid’s porisms matches exactly the various equations for homographic divisions of a line (see fig. below) – albeit with repetitions, since for Chasles, as we saw previously, Ancient Geometry was not sufficiently abstract and therefore multiplied properties which in fact were only one.

⁸⁶ [Chasles, 1860], p.35.

- I^{er} Livre.*
- Genres.
- I. $\frac{Am}{A'm'} = \lambda$ (1).
 - II. Tel point décrit une droite donnée de position.
 - III. $\frac{Am}{A'm'} = \lambda$; $\frac{Sm}{S'm'} = \lambda$.
 - IV. $\frac{Am}{mm'} = \lambda$.
 - V. Telle droite est donnée de position.
 - VI. Telle droite passe par un point donné.
 - VII. $\frac{Am}{A'm'} = \lambda$.
 - VIII. $\frac{Am}{Mm'} = \lambda$.
 - IX. $\frac{Am \cdot J'm'}{a \cdot A'm'} = \lambda$. Divisions homographiques sur deux droites.

This proximity between Chasles' higher geometry and his interpretation of the Porisms is striking in his rendition of the ten “Four-line porisms⁸⁷”. The second of these porisms, for instance, reads as follows (see fig. below):

PORISME II. – On donne deux droites SA, SB et deux points P, Q ; une parallèle quelconque à la droite qui joint ces deux points, rencontre les deux droites données en a et b ; on mène les droites Pa, Qb qui se coupent en m : ce point m est situé sur une droite donnée de position.



The position of the line, mentioned at the end of the proposition, is only *given* in the sense that it is entirely determined by the other elements of the proposition. This proposition is a porism because there is a truth to be proven, namely that m is always on one line. However, the position of this line with respect to the other elements of the proposition is missing for this to be a theorem: part of the solution to a porism is to find, or construct, this missing position. In fact, this construction is exactly Chasles' equation of a line given in the *Traité de Géométrie Supérieure* (see section 2.2.2), with the added complication that here, relative positions of points and lines have to be considered carefully due to Ancient Geometry not being abstract in Chasles' sense. Indeed, a and b form homographic divisions of the lines SA and SB (because they are in one to one correspondence), and the

⁸⁷ [Chasles, 1860], pp.99-108.

intersection of the lines Pa and Qb (which, in 1852, would have been denoted $Pa, Pb, Pc..$ and $P'a', P'b', P'c'..$ to emphasize the correspondence) generates a straight line.

In 1870, reflecting on this interpretation in his *Rapport sur les progrès de la Géométrie*, Chasles would make even clearer the link between Euclid's *Porisms* and the research he was conducting at the time, and in particular the principle of correspondence⁸⁸:

Nous avons dit que les propositions rétablies par Simson comme Porismes ne se rapportaient qu'à six des vingt-neuf énoncés de Pappus. D'où venait donc la difficulté que présentaient les autres énoncés ? Cette difficulté dont on ignore la cause, et qui a été le principal obstacle au rétablissement de l'ouvrage d'Euclide, est maintenant connue : c'est que la plupart des énoncés rebelles résument des propositions qu'Euclide avait introduites dans son Traité des Porismes, et qui, ne s'étant point trouvées peut-être dans d'autres ouvrages grecs, ne nous étaient point parvenues et étaient par conséquent ignorées de Simson, comme de tous les géomètres contemporains.

Effectivement, ces propositions renferment presque toutes des relations de segments s'exprimant par des équations à trois et à quatre termes, telles que

$$\alpha xz + \beta x + \gamma z + \delta = 0$$

x et z étant les abscisses de deux séries de points comptés sur une ou sur deux droites.

This last equation is none other than that of the anharmonic correspondences between the elements represented by x and z .

Porisms, in Chasles' reading, were the first historical examples of a kind of geometrical propositions which the principle of correspondence had come to encapsulate. They say something on the nature of a locus formed by variable objects, but without requiring that the entire construction be prescribed already – unlike theorems. And yet, they are not problems because they positively assert something of the nature of the locus being generated. For Chasles, this is exactly what Descartes' equations would achieve. These equations say something of the nature of a locus, whilst doing away with all the specific stipulations which elementary geometry required.

The proximity between the principle of correspondence and the higher geometry of (sheaves of) curves, which Chasles only hinted at in his 1860 book, would not be lost on his close disciples. In a review of this book, which borrowed extensively from Chasles' epistemological vocabulary, De Jonquières commented on the “undisputable analogy”

⁸⁸ [Chasles, 1870], p.239.

between Euclid's Porisms and modern geometrical propositions⁸⁹. Expanding on this theme, he commented on a critical review of a memoir recently written by another, albeit less famous, student of Chasles, namely Charles Méray⁹⁰. Méray, in 1860, had published a *Mémoire sur la théorie géométrique des surfaces du second ordre*⁹¹, in which only a single (algebraic) equation was written down. Olry Terquem, reviewing this memoir for the *Bulletin de bibliographie, d'histoire et de biographie mathématiques*, had taken issue with this way of writing geometry⁹²:

On n'y trouve qu'une seule équation, qui porte le n°1 ; de quoi on aurait pu se dispenser, puisque cette équation est unique ; c'est la méthode logique de M. Chasles, rendue moins *équationnelle*, s'il est permis de s'exprimer ainsi. Est-ce un avantage? Nous trouvons même que la classique et célèbre *Géométrie supérieure* est trop peu équationnelle. Des équations *écrites* valent mieux que des équations *parlées*, mais dont on se sert volontiers pour ressembler, à ce qu'on croit, à Euclide. Pure archéolâtrie. C'est faire rouiller une médaille fondue hier pour lui donner un vernis d'antiquité. La géométrie moderne se compose de *figures*, d'*équations* et de *déductions*, sans négliger les *inductions*, source de découvertes, et de chacune selon les besoins de la cause, comme l'on dit au barreau. Pourquoi les Grecs n'ont-ils pas fait usage d'équations? Même réponse que pour les chiffres : parce qu'ils ne les connaissaient pas. Apollonius ressuscité ne marcherait pas plus sur les traces d'Euclide que Platon ne serait platonicien, qu'Aristote ne serait aristotélicien; hommes de génie, ils apprendraient nos procédés et se placeraient bientôt au premier rang.

De Jonquières mobilized Chasles' restoration of the Porisms to rebuke this criticism. In his review of Chasles' book, De Jonquières explicitly mentioned "a few reflections, recently inserted in the *Nouvelles Annales*, set up an opposition between the *written* equations of

⁸⁹ [de Jonquières, 1861a].

⁹⁰ Hughes Charles Robert Méray (1835-1911) was an alumnus of the École Normale Supérieure, to which he was admitted in 1854. He spent most of his career teaching pure mathematics at the Université de Dijon, see [Huguet & Noguès, 2011].

⁹¹ [Méray, 1860].

⁹² "One can find in there only one equation, which bears the number 1, with which one could have done away, since this equation is unique; this is M. Chasles' logical method, made less *equational*, if one may speak in such a manner. Is it an advantage? We find that even the classical and famous *higher Geometry* is insufficiently equational. *Written* equations are worth more than *spoken* equations, on which however one is glad to rely, it seems, in order to resembled Euclid. Pure archeolatriy. It is like rusting a medal minted yesterday, to give it an antique shine. Modern geometry is composed of *figures*, *equations*, and *deductions*, and let us not forget *inductions*, a source of discoveries, and it is composed of each in amounts that depend on what is to be proven. Why did the Greeks not use equations? Same answer as for numbers: because they did not know them. Apollonius resurrected would not work in Euclid's footsteps, and neither would Plato be a platonist or Aristotle an aristotelian; men of genius, they would learn our methods and quickly claim their spots at the top of the ladder"; [Terquem, 1860], p.70. Chasles never took part to these discussions, at least not publicly.

analysis and the *spoken* equations of geometry”. One of the merits of the book, for De Jonquières, was to provide a strong argument for these spoken equations⁹³:

Je me permettrai de dire que cet ouvrage a encore, à mes yeux, un autre genre de mérite et d'utilité : c'est qu'il fait une diversion, au moins momentanée, aux publications presque exclusivement algébrique-géométriques de notre temps. L'analyse appliquée à la géométrie, surtout depuis qu'elle a simplifié et perfectionné quelques-uns de ses symboles, a pris des allures si vives, et en apparence si sûres; elle a parfois si bien réussi à présenter à sa manière, qu'elle dit être la meilleure, les résultats que souvent la géométrie pure avait d'abord découverts; elle fait, en un mot, des promesses si brillantes et si séduisantes, que bien des personnes seraient tentées de faire passer dans ses mains, disons de lui faire usurper, le sceptre de la géométrie. Cette tendance, qu'à bien des égards je regarde comme une illusion décevante, est peut-être, dans cette branche des mathématiques, un symptôme de cette fièvre d'activité, de ce besoin d'atteindre un but quelconque, qui est un des caractères dominants de notre époque.

Mais il est bon pourtant, dans l'intérêt même de la science, d'y apporter quelque tempérament. Car, en admettant même que la palme de la célérité dans les investigations appartienne aux méthodes analytiques, la science ne saurait encore s'en accommoder d'une façon exclusive. Pour me servir d'une comparaison vulgaire, on acquiert assez promptement la connaissance générale d'une contrée en parcourant les grandes voies de communications ferrées qui la sillonnent; mais pour bien en approfondir les détails, les productions, les ressources, il faut quitter la locomotive, et se résoudre à suivre à pied les anciennes routes et les chemins de traverse. Cela même donne des habitudes de

⁹³“I would like to say that this work has, in my opinion, yet another kind of merit and usefulness: it diverts the gaze, at least momentarily, from the almost exclusively algebrico-geometrical character of the publications of our time. Analysis applied to geometry, especially since it has simplified and perfected some of its symbols, has taken on such a pace that is so lively and seemingly so sure; it has sometimes been so successful in presenting in its own way, that it claims to be the best way, the results that pure geometry has often first discovered; analysis makes, in a word, such brilliant and seductive promises, that many would be tempted to pass the sceptre of geometry through its hands, let us say to usurp it. This tendency, which in many ways I regard as a disappointing illusion, is perhaps, in this branch of mathematics, a symptom of this fever of activity, of this need to reach some goal, which is one of the dominant characters of our time. But it is nevertheless good, in the very interest of science, to bring some temperance to this tendency. For, even admitting that the prize for speed of investigation belongs to analytical methods, science cannot yet accommodate it in an exclusive way. To use a vulgar comparison, one acquires fairly quickly the general knowledge of a country by travelling along the great railroads that criss-cross it; but in order to go into the details of the land, what it produces, what its resources are, then one must step off the locomotive and resolve to follow the old roads and walkways on foot. This even gives one habits of patience, observation and criticism, which one would risk losing if one did not know how to resign oneself to this primitive mode of peregrination”, [de Jonquières, 1861a], pp.8-9.

patience, d'observation et de critique, qu'on risquerait de perdre, si l'on ne savait se résigner à ce mode primitif de pérégrination.

What Chasles' restoration of the porisms had shown was that writing the geometrical equations of curves was not a reactionary return to the past, driven by mere nostalgia. Rather, it was a continuation thereof, in the direction of generality and abstraction. Similarly, the construction of curves that De Jonquières and Chasles had developed on the basis of the works of Newton and MacLaurin was not a return to past and outdated methods: by making abstract and general their constructions, they were shaping a new geometrical language for the expression of general descriptions of curves - that is to say, an alternative literary technology to the equations of algebra. This new instrument, in turn, served as the rightful tool for the virtuous practice of the pure geometer which Chasles had depicted years prior, and transmitted through the course of his lectures⁹⁴.

⁹⁴See section 1.2. In his 1856 *Mélanges*, while discussing the principle of anharmonic correspondence, De Jonquières had already made a similar point: “Mais on voit, en même temps, que cette connaissance de la génération des courbes d'un ordre supérieur ne peut être que l'œuvre lente du temps ; qu'on ne pourra s'y élever que de proche en proche, et en étudiant toutes les propriétés de celles qui les précèdent dans la hiérarchie, avec cette patience et cette sûreté d'investigation que la géométrie sait, mieux peut-être que toute autre science, apporter dans ses recherches, parce que ne pouvant guère ni sauter ni courir, s'il m'est permis de parler ainsi, elle ne laisse inaperçu aucun des détails de la route qu'elle parcourt”, [de Jonquières, 1856a], p.175.

Chasles' theory of characteristics

Introduction

While enumerative geometry as a discipline did not exist in any meaningful sense before the late 1870s, a series of questions and problems emerged and circulated in the middle of the 19th century, which we will call *enumerative problems*. These problems usually took the form of asking how many curves of a given degree touched other given curves. In particular, Steiner famously in 1848 found 7776 conics to be tangent to five other conics in the plane. This result inspired many, and several formulae to count the algebraic curves satisfying other contact conditions were quickly produced, by Steiner himself as well as by others, hailing from Germany, Italy, but also France.

As these problems circulated across Europe, they eventually reached Chasles, in large part through De Jonquières. These problems were immediately taken as a challenge for their novel approach to the higher geometry of curves. Indeed, by then, Chasles was in possession of a technology for the construction of curves determined by points (including when these points are at infinity, coinciding, or imaginary), namely geometrical equations using the principle of signs and the language of (homographic) correspondences (see chapter 3). To construct curves having prescribed contacts would prove a more difficult problem, but one which Chasles would solve by reinventing the principle of correspondence, and the textual practices attached to it. Between 1864 and 1867, Chasles published a general method for the enumeration and construction of all conics in the plane satisfying any given five conditions, which he called the theory of characteristics.

In this chapter, we briefly survey the initial circulation of these enumerative problems from Steiner to Chasles, presenting some of the methods and results obtained along the way. We then turn to the theory of characteristics, focusing mostly on its inner, technical workings. Lastly, we present Chasles' later papers which, whilst departing from enumerative problems proper, form a striking example of the textual practices of generality and

systematicity as he understood them by the end of his career.

4.1 Contact problems from Steiner to De Jonquières

Enumerative problems, broadly conceived as problems consisting in the enumeration of geometrical figures satisfying certain geometrical conditions, can be traced as far back as to (at least) the Ancient Greeks. A famous-example is the so-called Apollonian problem, which consists in the enumeration of the circles in the plane which satisfy three conditions of the form ‘passing through a given point’, ‘touching a given straight line’, or ‘being tangent to another given circle’¹. This problem was also well-known to many geometers of the early-modern tradition of problem-solving which developed in the wake of Commandino’s 1588 translation of Pappus’s *Collections*: Vieta, Descartes, and Van Roomen all proposed solutions for it². However, for these geometers, the crux of this problem was to construct (with ruler and compass) the circles satisfying these conditions. To that end, these geometers had to distinguish between several cases: to construct the circles which go through three aligned points, for instance, was not meaningful for them. As we saw in the previous chapter, Chasles had designed methods to revive the tradition of problem-solving whilst retaining the generality and abstraction provided by analytical geometry; in particular, allowing for the given data determining the problem to be at infinity, coinciding, or imaginary.

Throughout the eighteenth century, interest in constructive methods in geometry weaned. When analytical geometers tackled new enumerative problems, they did so with new tools, but also new concerns. Enumerations relying on algebraic equations of curves included complex solutions, degenerate curves (by means of polynomials which can be written as products) etc. Furthermore, enumerative problems lead to important research on elimination theory. This is the case of a famous paradox, which Cramer shared with Euler between 1744 and 1750. To both Euler and Cramer, it was a well-known fact that nine points uniquely determined a curve of the third order, for nine was exactly the number of coefficients in the general equation of such a curve:

$$Ax^3 + Bx^2y + Cxy^2 + Dy^3 + Ex^2 + Fxy + Gy^2 + Hx + Iy + 1 = 0$$

And yet, it was also known to them that two curves of order p and q had exactly pq intersection points. In the case of two third-order curves, this result yields exactly nine intersection points. Since these nine points are supposed to determine uniquely both of

¹See Book 4 of Pappus’ *Collections*, [Pappus, 2010], see pp.20-26 for the Greek text, pp.91-99 for an English translation, and pp.193-202 for a commentary.

² [Bos, 2001], pp.110-112; [Maronne, 2010], pp.543-548.

these two curves, it seems that a major contradiction between two well-known geometrical results had been reached. The paradox becomes even more apparent for curves of order four and above, as the number of intersection points of two curves become strictly larger than that of the number of points sufficient to determine one curve uniquely. Euler solved this paradox in 1750 by showing that not every set of nine points uniquely determines a curve, as it may happen that a point be “already in the others”, that is to say that the equation in the coefficients provided by this point is merely a combination of the equations provided by the other points³. More largely, the algebraic methods at the disposal of analytic geometers until the mid-19th century would prove rather inefficient at solving any kind of enumerative problem in any general way.

This is not the place to give a complete history of enumerative problems, nor is it clear that such a task would even be meaningful. In fact, none of the actors involved in the examples given above viewed their work as anything resembling “enumerative geometry”: Apollonius and early-modern geometers sought to construct curves satisfying certain problems, while Euler and Cramer were solving systems of algebraic equations. None of these actors had identified the systematic enumeration of figures satisfying conditions as a goal in itself. Instead, as we shall see, this enumerative problem emerged from the collision between the questions raised by a seemingly benign paper of Steiner’s in the 1850s, and the methods for the construction of plane curves constructed in parallel by Chasles.

4.1.1 Steiner’s problem and its circulation

Our starting point here will be a paper written in 1847 and published in 1848 by German geometer Jakob Steiner (1796-1863), entitled *Elementare Lösung einer geometrischen Aufgabe, und über einige damit in Beziehung stehende Eigenschaften der Kegelschnitte*⁴. It is in this paper that Steiner first touched on the question of enumerating the conic sections in a plane which touch five given conics. However, this text did not provide the conceptual grounds for its readers to build on: in fact, the techniques it presents are very rudimentary, and were taken up by no one. Nor did this text present a research programme for others to solve: in fact, Steiner’s text only states *in passing* his (erroneous) solution to the aforementioned question. Nevertheless, this text would serve as a shared reference for many geometers in the years to come, who all tried to produce general results on the enumeration of plane curves satisfying contact conditions (where,

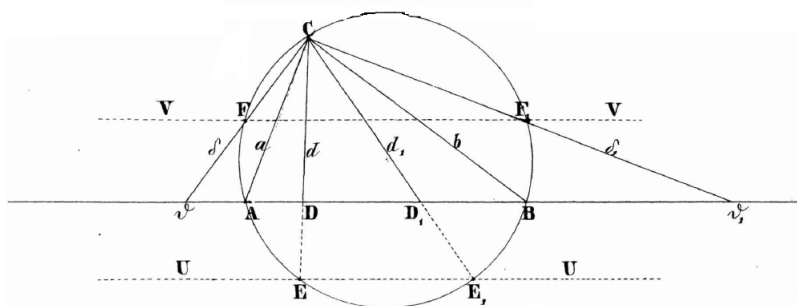
³On the Euler-Cramer paradox, [Euler, 1750b], [Euler, 1750a]. Some commentary is provided in [Struik, 1986], pp.180-183; and [Penchèvre, 2006], pp.113-117. In modern mathematical terminology, we say that $\frac{d(d+3)}{2}$ generic points in \mathbb{C}^2 uniquely determine an algebraic curve of degree d ; see [Brugallé, 2008], p.33.

⁴[Steiner, 1848].

by contact condition, we mean conditions of the form 'to have a contact of order μ with a given curve of order m '). Among these geometers, we find Chasles and De Jonquières, but also the German mathematician Johann Nikolaus Bischoff (1827-1893) or the Italian geometer Luigi Cremona (1830-1903).

In this paper, Steiner had set out to tackle two elementary, related problems. Given a triangle ABC , the first problem was to construct a point D such that $CD^2 = qAD \cdot BD$, where $q > 0$ is a given number. The second problem starts with a straight line AB given in position and in measure, and asks for the locus of the points C so that the triangle ABC yields no solution to the first problem in terms of the relative positions of these conics.

Steiner gives two solutions to these problems. The first one starts with a construction of a point D , which is only possible when the first problem admits a solution. To that end, Steiner draws the circle \mathcal{C} circumscribed to the triangle ABC . He then traces the straight lines U and V , which are parallel and at equal distance to AB , such that the ratio between the distance from C to AB and that from C to U be equal to q . He then considers the intersection points between \mathcal{C} and U (resp. V), which he denotes E, E_1 (resp. F, F_1). The straight lines CF, CF_1, CE, CE_1 are then drawn, which cut AB respectively θ, θ_1, D, D_1 (see fig. below). Per the intercept theorem, $CD \cdot DE = AD \cdot DB$. Furthermore, $\frac{CD}{DE} = q$



[Steiner, 1848], p.193.

per construction. Hence, $CD^2 = qAD \cdot DB$: the point D satisfies the first problem, and so do the points D_1, ν, ν_1 . Four solutions have been constructed, and Steiner goes on to discuss their relative positions, whether they are real or imaginary, whether they coincide etc., depending on the value of q , and the positions of the lines U and V .

Moving on to the second problem, Steiner uses this preliminary work to determine the boundary of the set of the points C for which the first problem is solvable. For instance, the constructions of the straight lines d, d_1 are only possible if the line U effectively intersects the circle \mathcal{C} . Steiner then sets out to describe the points C which satisfy this condition. Through a series of elementary computations, Steiner shows that the boundary of the set

of points C which make the constructions of d, d_1 possible is given by the equation:

$$AC \cdot CB \cdot \left(\cos \frac{\widehat{ACB}}{2}\right)^2 = AC^2 - \left(\frac{AB}{2}\right)^2$$

This means that the boundary for this set of points C is an ellipsis of foci A and B . Similarly, Steiner shows that for C to make the construction of the other two solutions (ν, ν_1) possible, C has to be within a hyperbola of foci A and B . The boundary that Steiner sought to determine is defined by two confocal conic sections (an ellipsis and a hyperbola), which divide the plane into four parts: a point can be within both conics, within the first and without the second, without the first and within the second, or without both. This division of the plane allows Steiner to characterize the number of solutions admitted by the first problem.

After giving a second solution to this problem, which ended with the same division of space, Steiner opens a new section in his paper, and comments⁵:

Die vorstehende Untersuchung führte auf ein System Kreise, welche einen Kegelschnitte doppelt berühren. Aber es kamen dabei einerseits nicht alle Kreise in Betracht, welche den Kegelschnitte doppelt berühren und andererseits stellten sich nicht alle Arten Kegelschnitte ein. Dies giebt Anlass diesen Gegenstand für sich etwas ausführlicher zu erörtern. Es bieten sich dabei noch einige nicht ganz uninteressante Eigenschaften und Sätze dar.

After five paragraphs devoted to various cases of systems of circles having such a double contact with a conic, distinguishing between ellipses, hyperbolas and parabolas, but also between various specific configurations, Steiner eventually writes⁶:

In Rücksicht auf bloss einfache Berührung der Kegelschnitte unter einander ist meines Wissens bis jetzt noch wenig geschehen. In älterer und selbst bis in die neueste Zeit hat man sich fast ausschliesslich nur mit dem sehr beschränkten Falle, mit dem Berührungsproblem bei Kreisen beschäftigt, aber nicht mit den entsprechenden Aufgaben bei den allgemeinen Kegelschnitten. Die letztern sind aber auch in der That ungleich schwieriger. Um dies zu zeigen, wird es genügen, hier nur die folgende Hauptaufgabe hervorzuheben, nämlich : Einen Kegelschnitt K zu finden, welcher irgend fünf gegebene Kegelschnitte

⁵“The investigations above led us to a system of circles which have a double contact with a conic section. But, on the one hand, not all circles with such contacts with a conic have been taken into account, and, on the other, not all sorts of conics appeared. This gives us the opportunity to discuss this topic in greater details for its own sake, which offers many properties and theorems, which are not uninteresting”, [Steiner, 1848], p.174.

⁶ [Steiner, 1848], p.188.

berührt⁷.

However, what follows this paragraph is rather disappointing. Steiner finds $6^5 = 7776$ conic sections touching five given conic sections on an inductive basis. Steiner shows that through 5 points passes 1 conic section, through 4 points pass 6 conic sections which touch another given conic section, and through 3 points pass $6^2 = 36$ conic sections which touch two given conic sections. Steiner is rather cautious in his wording, but no proof or tentative argument is provided⁸.

Steiner would later extend these initial investigations to general contact conditions. In a brief 1855 paper⁹, he gave a list of results such as that of the general existence of $3n(n-1)$ conic sections passing through three points and having an “osculatory contact” with a curve of order n . No proof was given: Steiner only produces a list of numbers of solutions, and comments on special cases for which the numbers are actually only maximal values. For instance, if the three given points are on the given curve, Steiner explained, the formula is not valid, and six conics must be subtracted from it. Three years later, Steiner devoted the fourth and last section of another paper on a variety of problems related to these questions, in what would be his last work on enumerative problems. The vocabulary and the general problem had changed, and Steiner began to ‘thematize’ the notion of condition¹⁰:

Durch fünf gegebene Elemente, oder durch fünf Bedingungen¹¹, ist, im Allgemeinen, ein Kegelschnitt bestimmt, nämlich entweder absolut, oder mehr or

⁷“As far as I know, little has been done so far with regard to conic sections having simple contacts with each other. In past times and even up to the most modern times, one has dealt almost exclusively only with the very limited case of contacts with circles, but not with the corresponding problems with general conic sections. This second case is also much more difficult. To show this, it will suffice to emphasize here only the following main problem, namely: To find a conic section K , which touches any five given conic sections.”.

⁸“Und wird auf diese Weise fortgefahren so gelangt man zuletzt zu $6^5 = 7776$ Kegelschnitten K , welche der obigen Aufgabe entsprechen”, [Steiner, 1848], p.189.

⁹ [Steiner, 1855]. The quote for the geometrical condition is: “eine gegebene Curve n^{ten} Grads in irgend einem Punkte osculirt (dreipunctig berührt)”, p.273.

¹⁰“In general, five given elements, or five conditions, determine one conic, either absolutely or in a more or else ambiguous manner. If the five elements consist only in points or tangents of the conics, then it is known that the solutions are not numerous and can be constructed. However, if one chooses amongst these given elements normals to the conics, then the solutions become harder, and their number increases with that of the normals, so as to reach 102”, [Steiner, 1858], pp.376-378.

¹¹The identification by Steiner of the concept of *Bedingung*, or condition is noteworthy, although Steiner did little with this concept in the papers mentioned here. While outside the scope of this chapter, it would be interesting to see how these texts look from the perspective of a broader study of Steiner's geometrical works. In particular, in the second volume of his posthumous *Vorlesungen über synthetische Geometrie*, edited and published by Geiser in 1867, Steiner introduced sheaves and bundles of conics. However, no concept of a collection of conics defined by unspecified conditions is introduced; [Geiser, 1867], vol.2, pp.224-430. I thank Jemma Lorenat for drawing my attention to this text. Furthermore, it may be worthwhile to compare these texts to those produced by German algebraic geometers of the same generation. For instance, at the same time, Otto Hesse was making important uses of the concept of *Bedingungsgleichung* in the study of related geometrical problems; see [Hesse, 1853].

weniger vieldeutig bestimmt. Bestehen die fünf Elemente nur aus Punkte und Tangenten des Kegelschnitts, so sind die Lösungen bekanntlich nicht zahlreich und geometrisch konstruierbar. Wählt man aber unter die gegebenen Elemente auch Normalen des Kegelschnittes, so werden die Lösungen schwieriger und ihre Zahl vermehrt sich mit der Zahl der Normalen, so dass sie bis zu 102 ansteigt.

Steiner gave a table (see fig. below) with numbers of conic sections passing through P given points, touching T given straight lines, and having N given normal lines, for all values of P , T and N so that $P + T + N = 5$. Once again, no general theory or method for the obtention of these results was given. However, this table would circulate: the following year, De Jonquière reproduced it, and gave proofs for the numbers contained therein¹². Later on, in the first of Chasles' paper on enumerative problems, a very similar table would be given, but with the last condition being replaced by that of touching N given conic sections¹³.

22. Steiner, *vermischte Sätze und Aufgaben.*

	P	T	N	L
1.	5	.	.	1
2.	.	5	.	1
3.	4	1	.	2
4.	1	4	.	2
5.	3	2	.	4
6.	2	3	.	4
7.	4	.	1	3
8.	.	4	1	3
9.	3	1	1	6
10.	1	3	1	6
11.	2	2	1	8
12.	3	.	2	9
13.	.	3	2	9
14.	2	1	2	14
15.	1	2	2	14
16.	2	.	3	23
17.	.	2	3	23
18.	1	1	3	28
19.	1	.	4	51
20.	.	1	4	51
21.	.	.	5	102

[Steiner, 1858], p.377

However, Steiner's problems and results were buried in papers which did not explicitly identify enumerating curves satisfying contact conditions as their main purpose, but rather did so whilst solving other problems. At any rate, Steiner had brought forth no central method or concept to deal with these contact problems, which he had begun to form.

¹² [de Jonquière, 1859b], p.49.

¹³ see [Chasles, 1864c], p.225.

One geometer who attempted to tackle contact problems in a systematic manner was Johann Nikolaus Bischoff (1827-1893), by then a *Gymnasiallehrer* in München about whom very little is known¹⁴. Bischoff was by no means a prolific writer: by 1864, he had published 5 papers, all in *Crelle's Journal*, with only one of them being more than four page long¹⁵. Bischoff's papers are remarkably consistent in the methods they use and the questions they tackle: using Jacobi- and Hesse-inspired analytical geometry, they all purport to find general formulas enumerating the numbers of curves or surfaces of a certain order satisfying certain contact conditions. Some of these formulas are very intricate, and require intensive computations. For instance, in his first paper, Bischoff found that there were

$$npq...(n + 2m - 3)(p + 2m - 3)(q + 2m - 3)...$$

curves of order m touching μ curves of respective order n, p, q, \dots , and passing through $\frac{m(m+3)}{2} - \mu$ points (the number of terms in this formula, while always finite, depends on the order of the curve and the number of given curves). From this complex formula, Bischoff was able to retrieve Steiner's 7776 conic sections touching five other conic sections. To do so, it suffices to take $\mu = 5$, and to replace every letter by 2, and the formula yields $2^5(2 + 2 \times 2 - 3)^5 = 6^5$ tangent conics.

Bischoff had read and identified a specific type of problem within Steiner's writings, to which he thought analytical geometry provided with efficient methods to obtain general answers. At the same time, he maintained an active correspondence with Cremona starting at least in 1861¹⁶, as well as with De Jonquières. The recently published correspondence of Cremona suggests a rapid circulation of ideas between the three men, with Chasles joining the discussion later on through the intermediary of De Jonquières. As a result, Bischoff was rather remarkably referenced by De Jonquières in his 1861 paper on plane curves¹⁷, by Cremona at the opening of his 1862 *Introduzione ad una Teoria Geometrica delle Curve Piane*¹⁸ (among the likes of MacLaurin, Carnot, Poncelet, or Chasles), and by Chasles, in his initial communication on the theory of characteristics¹⁹. Through Bischoff, these French and Italian geometers knew of Steiner's claim on the conics touching five given conics, and his broader interest in general formulae for the number of curves satisfying contact conditions.

¹⁴In 1868, as the Technische Hochschule München was founded, Bischoff was hired as professor of mathematics, in charge of algebraic analysis, trigonometry, and synthetic geometry, see [Hashagen, 2011], p.136.

¹⁵ [Bischoff, 1859], [Bischoff, 1860], [Bischoff, 1861a], [Bischoff, 1861b], [Bischoff, 1863].

¹⁶ [Israel, 2017], p.150.

¹⁷ [de Jonquières, 1861b], p.123.

¹⁸ [Cremona, 1862], p.2.

¹⁹ [Chasles, 1864c], p.222.

Modes of engagement with Steiner's and Bischoff's texts would differ between readers. For Chasles, the reference would serve a strategic purpose: he would show how his own theory was far superior to that of his German colleagues, which was wrong anyway. For De Jonquières and Cremona, however, the reference served a different purpose: as early-career scholars, they sought to present their work as engaging with questions raised by some of the most influential mathematicians of their field. Strikingly, the kind of results sought by Steiner in his later papers, and by Bischoff, would circulate much more than the methods they actually used to find them. Steiner's and Bischoff's solutions to contact problems mostly came in the way of formulae of a specific kind. Given curves of orders m_1, \dots, m_s , the number of curves of order m satisfying contact conditions with these curves would always be expressed as a polynomial in these symbols (with integral coefficients), as exemplified by the formula above. De Jonquières and Cremona looked for results of a same kind, even though they did not preserve anything from the elementary synthetic geometry at the heart of Steiner's papers, or from Bischoff's analytical computations.

What is now called the *Steiner problem*, that is to say the question of finding how many conic sections are tangent to five given conic sections, thus circulated along a thin line, from Steiner to Chasles, with the crucial mediation of Bischoff, Cremona and De Jonquières. In the first phase of this circulation, no real mathematical knowledge was transmitted by way of methods or concepts. However, by the late 1850s, contact problems (and a type of solutions to them) had emerged as a shared object for a growing, international group of mathematicians. This object would come to meet Chasles' higher geometry through a few papers by De Jonquières, to which we now turn.

4.1.2 De Jonquières' theory of plane curves

While we do not have De Jonquières' correspondence, or any archival sources for his daily mathematical practice, his first contact with Bischoff's formulas seems to have occurred in 1860, as De Jonquières' first reference to Bischoff is made in a paper written in the first months of the year 1861. It should come as no surprise that De Jonquières identified these results as being particularly relevant for his own research: as we briefly discussed in the previous chapter, De Jonquières had been working extensively throughout the late 1850s on the generation of geometrical curves through the consideration of sheaves of curves passing through a fixed set of given points. This was of course the result of Chasles' influence, but also of De Jonquières' interest in Steiner's results on the numbers of conics having fixed normals, which he had proven in 1859. Bischoff's framing of Steiner's problem therefore immediately entered the purview of De Jonquières' research interests at this period. However, De Jonquières did not read German, nor was he familiar with the techniques used by Bischoff. Instead, he used a blend of some techniques learnt and

adapted from Chasles, but also of some he drew from his reading of the British geometers Arthur Cayley and George Salmon. In this respect, De Jonquières' work on enumerative problems is a first departure from the heavily Chasles-inspired line of research he had been following earlier - which would not be without consequence for the reception of this work.

De Jonquières' theory is centered around the concept of *series of curves* and that of *indexes*, which he introduced and defined in a paper from 1861 for *Liouville's Journal* in the following terms²⁰:

Définitions. – Je dirai que des courbes géométriques planes du degré n forment une *série*, quand elles ont toutes en commun $\frac{1}{2}n(n+3) - 1$ conditions quelconques, c'est-à-dire quand elles satisfont toutes à autant de conditions, moins une, qu'il en faut pour déterminer une courbe de ce degré ; et, si N désigne le nombre des courbes de cette série qui peuvent, en outre de ces conditions communes auxquelles elles sont assujetties, passer par un point quelconque donné, je dirai que la série est *d'indice* N .

While Steiner had already introduced the general notion of a condition, he had never actually formed conditions that were not contact conditions. Thus, in this paper, De Jonquières makes a crucial departure from previous notions such as Steiner's *Kegelschnittbüschel* or *Kegelschnittschaar*, or even his own 1858 *faisceaux de courbes*, in that the conditions determining the object are unspecified. Any geometrical condition can be arbitrarily chosen to form a *série*. But what exactly does the term *condition* mean in this context? In the context of analytical geometry, it means any polynomial equation in the coefficients of the general equation of curve of a given order. However, such equations do not need to be formed, or actually written down, in order to be a useful concept²¹. In Hesse's analytical geometry, for instance, just discussing the properties of certain “Bedingungsgleichungen” without forming them can be a fruitful endeavour²². De Jonquières does not feel the need to be more explicit about what conditions can be in a purely geometrical context. In fact, he seems to tacitly presuppose the existence of a natural and simple analogy between the notions of analytical geometry and those of pure geometry, as he immediately translates the notion of *série* into a general, algebraic equation²³:

²⁰“Definitions. – I shall say that plane geometrical curves of degree n form a series, when they all have $\frac{1}{2}n(n+3) - 1$ conditions in common, that is to say when they all satisfy to as many conditions, minus one, that are required to determine a curve of this degree; and, if N denotes the number of curves in this series which can, on top of the common conditions which they are required to satisfy, pass through a given point, then I shall say that the index of this series is N ”, [de Jonquières, 1861c], p.113.

²¹A similar phenomenon plays a key role in the shaping of a concept of “geometrical equation”, shared by certain algebraic geometers such as Clebsch and Jordan, see [Lê, 2015]. These ‘geometrical equations’ are totally unrelated to those discussed in this dissertation.

²² [Hesse, 1853], pp.282-283.

²³“Lemma. – All the curves C_n in a series of index N can be analytically represented by equations

Lemme. - Toutes les courbes C_n d'une série d'indice N peuvent être représentées analytiquement par des équations $F(x, y)$ du degré n , dont tous les coefficients sont des fonctions algébriques, entières, et rationnelles d'une indéterminée λ , qui s'élève, dans l'un d'entre eux au moins, au degré N , mais jamais à un degré supérieur, tandis que $\frac{1}{2}n(n+3) - 1$ d'entre eux sont de certaines fonctions déterminées des paramètres des $\frac{1}{2}n(n+3) - 1$ équations qui expriment les conditions auxquelles sont assujetties toutes les courbes de la série.

To this lemma, De Jonquières gives a rather surprising proof: “for each λ ”, De Jonquières claims, “the equation represents a curve of degree n satisfying the conditions of the series”. This is a rather puzzling statement, as the equation has not been formed yet: in fact, De Jonquières takes its existence for granted. He likely obtained this statement by analogy with the case of sheaves of curves, which we discussed in the previous chapter, and in which $N = 1$ and the equation F is linear in λ . This is actually a problematic assumption, as many would point out later, including Cremona, Battaglini, or Cayley²⁴. In fact, the existence of a rational and integral equation F is not generally assured, and De Jonquières’ lemma is false in general. However, when such an equation F does exist, whose coefficients are rational functions of a coefficient λ , the rest of the lemma holds; and so does De Jonquières’ proof. Taking x, y to be the coordinates of an arbitrary point A , De Jonquières views the equation $F(x, y) = 0$ as an equation in λ : it must necessarily be of degree N , as that is the number of curves in the series which pass through A .

Remarkably, despite the pushback against this lemma, it appears that neither De Jonquières nor Chasles ever really doubted its validity. During their controversy in 1866, Chasles would write that “this proposition is obvious; it is the very first one, and there are no more elementary ones in all of the theory of curves²⁵”. Earlier, in a letter to Cremona, dated January 23rd 1864, De Jonquières had expressed similar confidence in this result²⁶:

$F(x, y)$ of degree n , whose coefficients are all algebraic, integral, and rational functions of an indeterminate λ which can reach, in at least a few of them, the degree N , but never a higher degree, while $\frac{1}{2}n(n+3) - 1$ among them are certain functions determined by the parameters of the $\frac{1}{2}n(n+3) - 1$ equations which express the conditions which the curves of the series are required to satisfy”, [de Jonquières, 1861c], p.114.

²⁴In [Israel, 2017], De Jonquières mentions and responds to criticisms against this lemma raised by Cremona and Battaglini, pp.974-975. Cayley published his detailed criticism of De Jonquières’ lemma in [Cayley, 1868a], pp.124-126. After Cayley, most would recognize the lemma as being false.

²⁵“Cette proposition est évidente; elle est la première, et il n’y en a pas de plus élémentaire dans toute la théorie des courbes”, [Chasles, 1866b], p.818. We will come back to this text and this controversy in chapter 4.

²⁶“You seem to have doubts regarding the lemma. I do not see what can justify them. A family of curves of degree n , each of which requires only one more condition in order to be determined by a single equation of degree n , having (in its coefficients) a single indeterminate, each value of which corresponds to a particular curve of the series. This cannot be contested. Furthermore, since by every point of the plane, there must pass N of these curves, it is necessary that the equation provides N distinct values of this indeterminate, once we have substituted to x and y the coordinates of the point in question. Thus the coefficients must, in conclusion, be algebraic, integral, and rational functions of the indeterminate λ ,

Vous paraissez émettre des doutes au sujet du lemme. Je ne vois pas ce qui peut les justifier. Une famille de courbes du degré n , dont chacune n'exige plus qu'une seule condition pour être déterminée peut être représentée par une seule équation du degré n , contenant (dans ses coefficients) une seule indéterminée, dont chaque valeur correspond à une courbe particulière de la série. Cela ne peut se contester. En outre, puisque par tout point du plan, il doit passer N de ces courbes, il est nécessaire que l'équation fournisse N valeurs distinctes de cette indéterminée, après qu'on y aura substitué pour x et y les coordonnées du point dont il s'agit. Donc il faut que les coefficients soient, en définitive, des fonctions algébriques entières et rationnelles de l'indéterminée λ , et des fonctions dont l'une au moins soit du degré N , après qu'on aura effectué toutes les réductions, chassé les dénominateurs et fait disparaître les radicaux s'il en existait. Si ces conditions ne sont pas remplies et ne peuvent l'être, on n'a plus ce que j'ai appelé une série de courbes, et il n'en passe plus un nombre fixe et déterminé par chaque point du plan.

This last sentence is crucial: the step in the proof which others would find faulty or missing, namely that where the function is constructed or shown to exist, is for De Jonquières a fundamental hypothesis on the relation between algebraic equations and geometrical curves, or series thereof. In his following letter to Cremona, dated February 9th 1864, De Jonquières admits, after hearing of Battaglini's criticism, that “in general, series are not rational, regarding parameters”, but that it is difficult to know which series are rational since equations are rarely actually formed²⁷. This admission after De Jonquières' results had come under assault by Chasles. De Jonquières was, at this point, convinced that his formulae were indeed incorrect, and thought that this lemma could be the source of his error. In fact, this would prove to be an unconvincing explanation: while the lemma is indeed false, the difficulties with De Jonquières' 1861 results, as we will see in section 5.1.2, have to do with a more fundamental question, namely that of the enumeration of solutions without any real geometrical significance, and in particular of degenerate curves²⁸.

From this analytical translation of the concept of a series, De Jonquières would derive several crucial theorems, one of which in particular would be the focus of harsh criticism by Chasles²⁹:

and at least one of these functions must be of degree N , once we have carried out all reductions, chased the denominators, and have had the remaining radicals vanished. If these conditions are not met and cannot be so, then we do not have what I call a series of curves, and more than a fixed and determined number of curves go through each point of the plane”, [Israel, 2017], p.974. Underlining in the original.

²⁷ [Israel, 2017], pp.975-976.

²⁸For now, it suffices to say that De Jonquières' formula, like before them Bischoff's and Steiner's, enumerate certain degenerate curves which do not satisfy the conditions in any meaningful sense.

²⁹“*Theorem II.* – Among the curves C_n in a series of index N , there are $2(n-1)N$ which touch a given

Théorème II. – Parmi les courbes C_n d’une série d’indice N , il y en a $2(n-1)N$ qui touchent une droite donnée L .

En effet, la droite L coupe les courbes de la série en des points dont les abscisses sont, d’après le *Lemme*, les racines d’une équation du degré n en x , dont certains coefficients, sinon tous, contiendront une indéterminée λ au degré N , mais non pas à un degré supérieur.

A toute valeur de λ qui rend égales deux racines de cette équation, il correspond une courbe C_n qui touche la droite L . Or la condition d’égalité des deux racines s’exprime par une équation du degré $2(n-1)$ par rapport aux coefficients de l’équation en $x^{(*)}$. Donc, cette équation de condition est du degré $2(n-1)N$ en λ . Donc enfin il existe $2(n-1)N$ courbes de la série qui touchent la droite $L^{(**)}$.

[In footnotes]

(*) Voir, par exemple, la *Note sur l’élimination qui se trouve dans l’appendice du Traité du Rév. G. Salmon sur les courbes supérieures*, p.296.

(**) Cette formule semble être en défaut quand, n étant égal à 2, il y a plus d’une droite parmi les conditions communes aux coniques de la série. Cette anomalie apparente sera expliquée ci après.

De Jonquières’ use of algebraic results is certainly a novelty with respect to his previous work, as well as a stark departure from the methods of his mentor Chasles. The very same year as that of his laudative review of Chasles’ restitution of the lost *Porisms* and his praise of the virtues of “spoken equations” against “written equations³⁰”, De Jonquières seems to blur key epistemological divides, as he synthetizes several traditions, by importing problems and results of Bischoff and algebraic concepts from Salmon.

Furthermore, De Jonquières’ proof deserves special attention here. By fixing a straight line L , and forming a series of points x on L , De Jonquières uses a method which is reminiscent of Chasles’ principle of anharmonic correspondence, and of De Jonquières’ work on the generation of curves³¹. Because Chasles and De Jonquières had only considered homographic sheaves of curves, they had only formed series of points (on a fixed straight line) where the number of coinciding points could be 2 or 3. In 1861, however, by introduc-

straight line L . Indeed, the straight line L intersects the curves of the series in points whose abscissas are, according to the *Lemme*, the roots of an equation of degree n in x , of which certain coefficients (if not all) contain an indeterminate λ at degree N , but not to any higher degree. To each value of λ which makes two roots of this equation equal, there corresponds a curve C_n which touch the straight line L . The condition of equality of two roots can be expressed by an equation of degree $2(n-1)$ in the coefficients of the equation in x . Thus, this equation of condition is of degree $2(n-1)N$ in λ . Therefore, there are $2(n-1)N$ curves in the series which touch the straight line L ”, [de Jonquières, 1861c], pp.115-116. Underlining in the original.

³⁰See section 3.2.3.

³¹See section 3.2.

ing the general concept of condition to characterize collections of curves, De Jonquières put forth the notion of series of curves, thereby extending the notion of sheaf³². To study series with the help of correspondences, he then had to consider correspondences that are not necessarily homographic.

The strategy employed by De Jonquières in order to prove this “Théorème II” in fact runs throughout the entirety of his 1861 paper. A crucial example is that of an important theorem which generalizes his previous work on the generation of geometrical curves through the intersection of homographic sheaves³³. The theorem is as follows³⁴:

Théorème V. – Si à une courbe C_m d'une série d'ordre m et d'indice N il ne correspond qu'une seule courbe C_n dans une autre série d'ordre n et d'indice N , et réciproquement, le lieu des points d'intersection de deux courbes C_m et C_n correspondantes est du degré $N(m + n)$.

To prove this theorem, De Jonquières fixes a straight line L , and shows that L intersects the locus described by the theorem in exactly $N(m + n)$ points. To that end, he takes m to be a “variable point” on L , thus creating a rather ambiguous notation, as m also denotes the order of curves of the first series. To make things clearer for modern-day readers, we will slightly distort De Jonquières' notation, by denoting the variable point m_{var} instead of m . For each position of m_{var} , there are N curves C_m of the first series which pass through m_{var} . Each of these curves intersect L at m points. Therefore, to a point m_{var} correspond N groups of m points on L , whose abscissas De Jonquières denotes with the single letter x .

To each curve C_m considered previously corresponds exactly one curve C_n , per hypothesis. For the same reasons as above, to this curve correspond N groups of n points m'_{var} on L , whose abscissas De Jonquières denotes with the single letter x' . In fact, De Jonquières explicitly repeats sentences to emphasize the similarity in reasonings in both directions.

To each point m_{var} correspond N groups of m points m'_{var} , and to each point m'_{var} correspond N groups of n points m_{var} . Therefore, explains De Jonquières, x and x' are linked by an algebraic equation $P(x, x') = 0$, whose left-hand side must be formed of N similar factors, namely factors of the form:

$$Ax^m x'^n + Bx^m x'^{n-1} + Cx^{m-1} x'^n + ..$$

Indeed, fixing x' must yield an equation whose N factors are each polynomials in x of

³²A sheaf of curves is none other than a series of index 1.

³³“Theorem V. – If, to a curve C_m in a series of order m and index N , there corresponds only one curve C_n in another series of order n and index N , and conversely, then the locus of the intersection points of the two corresponding curves C_m and C_n is a curve of degree $N(m + n)$ ”, [de Jonquières, 1858].

³⁴ [de Jonquières, 1861c], p.117. There is a typo in the original paper, as the first words read “Si à une courbe C_n ..”, whereas it should be a curve C_m . The typo is fixed in the quote given here.

degree m , for to a fixed point m'_{var} correspond N groups of m points m_{var} ; and conversely. De Jonquière concludes by noticing that a point on L is on two corresponding curves C_m and C_n if and only if $x = x'$: this equation is clearly of degree $N(m + n)$.

In this proof, the import and generalization of the notion of correspondence from Chasles and De Jonquière's earlier works is clear. However, this proof-strategy is not identified and thematized by De Jonquière in the way it would be in Chasles' own work on enumerative problems (see 4.2.2 below). This may be due to the fact that De Jonquière's method requires for him to form - if only partially - the algebraic equation for each correspondence, thus not being easily expressible under a unified principle as it would be for Chasles³⁵.

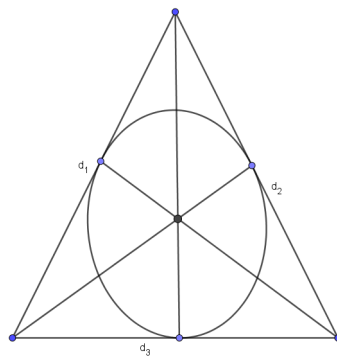
In the last section of his paper, De Jonquière uses these general theorems to obtain formulae which largely resemble those proposed by Bischoff to solve contact problems. However, these formulae are used here to give the orders or classes of certain loci which can be described through series of plane curves. For instance, De Jonquière shows that chords common to a fixed curve C_m and the curves in a series of order n and index N are the tangents to a curve of class $\frac{1}{2}m(m-1)(2n-1)N$. Thus, it must be stressed that De Jonquière did not propose an enumerative theory of plane curves. The encounter with Steiner's and Bischoff's results helped De Jonquière forge a concept of series, expand the notion of correspondences beyond homographic divisions, and introduced a kind of result which he (and Chasles) would try to replicate. However, De Jonquière had merely used Steiner's and Bischoff's formulae for the general study of the properties of plane curves, thus leaving aside concerns for the systematic enumeration of curves.

4.1.3 A public retraction

De Jonquière's theorems are not without problems. One of them, which De Jonquière had noticed himself, is that they seem to be flat out false in the case of curves of order two, that is to say conic sections. Indeed, from theorem II it seems to follow that there are 2^n conic sections touching n given lines and passing through $5 - n$ points, for all $n \in [1, 5]$. This is obviously false, because there is only one conic section touching five given lines, per Brianchon's theorem (and not $2^5 = 32$). More generally, considering series of curves of order two, De Jonquière's results seem to absolutely contradict the principle of duality, in that they don't give symmetric roles to points and lines.

³⁵It is unclear whether Chasles was already using similar techniques in his own teaching. The priority question with respect to Chasles' more general principle of correspondence will be discussed in the next chapter. It was the object of much discussion amongst late 19th-century Italian geometers, see for instance [Segre, 1892], [Loria, 1902]. This is probably due to the fact that methods of correspondence were largely taught to students in Italian universities at that period (see [Brigaglia, 1984], p.36).

However, De Jonquières claims that his formulas merely “seem³⁶” to be contradicted by the example given above. Suppose a conic ought to touch three given straight lines. These three lines form a triangle; now, drawing the lines joining each vertex of the triangle to the point where the conic touches the opposing side of the vertex, one single intersection point is obtained (see fig. below). Therefore, De Jonquières claims, the conics having to touch three given straight lines have a “mutual dependency”, wherefrom it follows that “the number of conics satisfying the question can be diminished, because several of them become coinciding³⁷”. Adding tangent lines, explains De Jonquières, increases the mutual dependency of conics and enlarges the discrepancy between the number provided by his formulas and the number of apparent solutions. The first number counts that real number of solutions, while the second ignores coinciding solutions to the problem. For De Jonquières, this extra difficulty only appears in the case of conics, because other curves do not have equal classes and orders. However unconvincing these explanations



may be to us, De Jonquières' results were much appreciated by Cremona, who reproduced most of them in his 1862 *Introduzione*³⁸. In the wake of these successful forays into the enumerative problems raised by Bischoff, De Jonquières participated to the *Grand Prix de Mathématiques* proposed by the Académie des Sciences for the year 1862. The topic on which memoirs were invited was the following³⁹:

Résumer, discuter et perfectionner en quelque point important les résultats obtenus ici sur la théorie des courbes planes du quatrième ordre.

Chasles was the *rapporteur* for this *concours*, and it is very likely that he was the one who selected this very question. It is very much in the continuation of works he had just published, and it does not seem to match particularly well the research interests of the other members of the jury for this prize, namely Bertrand, Liouville, Lamé and Hermite.

³⁶ “[Cette formule] paraît être en défaut..”, [de Jonquières, 1861c], p.121.

³⁷ [de Jonquières, 1861c], p.121.

³⁸ [Cremona, 1862], pp.63-70.

³⁹ “Summarize, discuss, and perfect in some important point the results obtained here on the theory of plane curves of order four”, [Bertrand et al., 1862], p.124.

Two memoirs were submitted this very year. The unknown author of the second memoir was awarded one third of the prize, that is to say one thousand *francs*. Their work was deemed to be an original and valuable contribution, but far from fully answering the question. The first memoir, which was awarded the remaining two thousand *francs* (but not the honor of being fully prized), was that of De Jonquière⁴⁰. We do not have this memoir at our disposal: however, Chasles' review, as well as later mentions of it make it clear that De Jonquière had elected to showcase the power of his new results and methods, by applying them to the case of curves of order four. The reason this memoir only received two thirds of the total prize money was that Chasles had found a major flaw in De Jonquière's theory, one that he would also find in the aforementioned 1861 paper⁴¹:

Mais, nous sommes obligés de le dire, cet excellent exposé est compromis par **une trop grande extension attribuée à certaines propositions**. C'est par suite d'une première méprise sur le degré d'une équation, qui ne devait être pris que comme une limite et non comme un nombre absolu, que l'auteur s'est retrouvé conduit d'une manière très regrettable à divers résultats qui manquent ainsi de démonstration, et parfois d'exactitude. Cette erreur était fort séduisante par les conséquences faciles qui s'ensuivaient. Aussi entache-t-elle plusieurs parties du travail, qui, à tous autres égards, se recommande par une exposition claire et une connaissance étendue de toutes les parties de la matière.

This degree to which Chasles refers is that of the equations formed by De Jonquière in proofs using correspondences of points on a line L , such as the proofs of theorems II and V given above. Chasles thought that the degree of the equation formed with the abscissas of the points on the line L which De Jonquière had obtained was only a maximum value, as nothing guaranteed that the first coefficient of the polynomial was not actually zero⁴². It must be stressed that Chasles took no issue with De Jonquière's algebraic equation for series of curves $F(x, y, \lambda) = 0$, but only with his proofs via correspondences.

Upon seeing his results rejected by Chasles and the Académie, De Jonquière immediately wrote to retract them. De Jonquière's theory of series of plane curves had been

⁴⁰While no name is given in the *Rapport* published in 1862, Chasles would quote parts of this verdict a few years later during his controversy with De Jonquière, making explicit the identity of the author of this memoir. See [Chasles, 1866b], p.819.

⁴¹"However, we must say that this excellent report is compromised by the attribution of too large an extension to some propositions. Because of a first mistake on the degree of an equation, which had to be taken as a limit and not an absolute number, the author was very regrettably misled into various results which lack a proof, and sometimes exactness. This error was rather attractive, due to the easy consequences ensuing therefrom. Thus, it tarnishes many parts of a work which, in all other respects, is to be commended for its clear exposition and a vast knowledge of all parts of the subject-matter", [Bertrand et al., 1862], p.934.

⁴²In section 4.2.2, we shall see how Chasles fixed this proof.

included and discussed in Cremona's 1862 *Introduzione ad una teoria geometrica delle curve piane*⁴³. On February 6th 1863, that is to say shortly after reading Chasles' verdict, De Jonquières wrote to Cremona, warning him against the fallacy of these results that had already been printed in the *Introduzione*⁴⁴:

Vous avez eu la bonté, dans votre savante Introduction à la théorie des courbes planes, de citer quelques théorèmes que j'ai donnés dans un article inséré au tome VI (2e série) du Journal de Liouville, pour 1861. J'ai l'honneur de vous faire remarquer que plusieurs de ces théorèmes sont énoncés par moi en termes trop absolus.

Furthermore, De Jonquières immediately published a retraction of his results in *Liouville's Journal*, in similar terms⁴⁵:

Dans cet article, qui traite plus particulièrement des propriétés des *séries de courbes d'ordre n et d'indice N* , j'ai donné une forme trop absolue aux énoncés de quelques théorèmes, et il est bon que le lecteur en soit averti.
(..) Si n et N sont à la fois quelconques, les nombres exprimés dans les énoncés de ces théorèmes ne doivent plus être pris comme des nombres absolus, mais simplement comme une limite supérieure.

As we shall see in section 5.1.2, not everyone was convinced by Chasles' criticisms; and, in particular, Cremona spent several months trying to defend De Jonquières' formulae even against the opinion of their author. These discussions, however, would soon take a different turn as Chasles began to publish his own theory of systems of conics, to which we now turn.

⁴³ [Cremona, 1862], pp.63-71.

⁴⁴“You were very kind, in your savant Introduction to the theory of plane curves, to cite a few theorems which I gave in my paper inserted in the tome VI of Liouville's Journal, in 1861. I have the honour of letting you know that some of these theorems are stated by me in terms that are too absolute”, [Israel, 2017], pp.970-971. Underlining in the original. Note how De Jonquières immediately adopts Chasles' description of these results as too “absolute”. We will come back to the correspondence between Cremona and De Jonquières on this issue in 5.1.2.

⁴⁵“In this paper, which deals more particularly with the properties of series of curves of order n and of index N , I gave too absolute a form to the statements of a few theorems, and it is important that the reader be aware of it. [...] If n and N are both arbitrary, the numbers expressed in the statements of these theorems must be taken not as absolute numbers, but merely as upper limit”, [de Jonquières, 1863], p.71.

4.2 Counting and constructing conics

On February 1st 1864, Chasles stepped on the podium during one of the weekly meetings of the Paris Académie des Sciences to make an astounding statement⁴⁶: Steiner’s results, from the enumeration of the 7776 conics touching five other conics to his other formulae, were wrong, and so were the related propositions of Bischoff. For these erroneous results, Chasles substituted a new general formula for the number of conics touching five given curves of order m_1, \dots, m_5 . At the end of his communication, Chasles revealed yet another twist: this general formula itself was in fact but part of a “rather vast theory⁴⁷”.

Over the following weeks, Chasles returned a number of times to this very same podium to present his theory, which he called the theory of characteristics. What this theory achieved was nothing less than to give a complete and general solution to the problem of counting and constructing all (plane) conics which satisfy any given five conditions⁴⁸. By placing, at least for a time, the construction of the conics satisfying five conditions on the same level as their enumeration⁴⁹, Chasles firmly anchored this new theory in his previous work on the generation of curves via homographic sheaves, described in chapter 3.

In what follows, we present this theory, focusing on the key concepts it revolves around, the kinds of computation it involves, and the textual apparatuses it relies on, while stressing the continuity between Chasles’ presentation and his broader epistemological theses as expounded in chapters 1 and 2.

4.2.1 The method of substitution

There are two ways to summarize the content and goal of Chasles’ theory of characteristics; none of which Chasles consistently favored. The first one would be to describe it as a method, or “marche à suivre”⁵⁰ for both the enumeration and the construction of conics satisfying five conditions. Another would be to describe it as the theory of systems of conics, where systems of conics are defined as collections of conics satisfying four given conditions. While mathematically equivalent, these descriptions entail two different Chaslesean practices, as we discussed earlier (see 1.2.2). To assert the value of a method, one must exhibit its uniformity; to guarantee that a theory is adequately built, one must

⁴⁶In this chapter, we focus on the mathematical aspects of the theory of characteristics. In section 5.1.3, we shall come back to the modalities of the publication of this theory.

⁴⁷“Les considérations qui m’ont conduit aux résultats précédents s’appliquent à un grand nombre d’autres questions. [...] Ces questions, on le voit, donnent lieu à une théorie fort étendue” [Chasles, 1864c], pp.225-226.

⁴⁸ [Chasles, 1864b], p.297.

⁴⁹Due to the complexity of the results reached by this theory, these constructions are almost never realistically feasible; they remain what Chasles calls ‘theoretical constructions’.

⁵⁰ [Chasles, 1864b], p.298.

exhibit a minimal set of fundamental properties from which the rest of the theory can be naturally derived. In the context of the theory of characteristics, Chasles maintained both attitudes. His emphasis was alternately on the uniformity of the proofs, enumerations and constructions enabled by his new method, and on the fundamentality of the notions at the heart of his theory, namely the characteristics of a system.

For any system S of conics satisfying four conditions, denoted Z, Z', Z'', Z''' , Chasles defines two numbers μ and ν as the numbers of conics in the system S passing through a given point, and touching a given line respectively⁵¹. An important notational and definitional innovation is brought forth here by Chasles: using symbols for conditions will be useful both for the expression of general enumerative formulae, not bound to any specific condition, but also for outlining a general enumerative procedure. The fact that μ and ν do not depend on the choice of the given point or given line is not discussed by Chasles, but is part of the kind of generality with which he had treated the construction of cubics passing through nine points: the relative positions of these points were irrelevant once one accepts curves to have singularities, complex branches, or degenerations. The Fundamental Theorem of Algebra, which states that the number of roots of a polynomial is always equal to its degree, justifies this fact if we think of conditions as polynomial equations in the coefficients of the general equation of a conic.

The numbers μ and ν are what Chasles calls the characteristics (*'caractéristiques'*) of a system S . Indeed, as the terminology indicates, Chasles claims that “all properties of systems of conic sections can be expressed by means of these two numbers⁵²”. Chasles symbolically represents this assertion by the following equation:

$$(Z, Z', Z'', Z''') \equiv (\mu, \nu)$$

And indeed, in his second communication to the Paris Académie regarding this theory, Chasles included a list⁵³ of properties of a system (μ, ν) . This list is divided into three sections; the first of which is entitled “geometrical loci”, the second “envelopes”, and the third “miscellaneous properties of a system⁵⁴ (μ, ν) ”; with many propositions of the first two sections being the duals of one another (see fig. below).

These properties all express the order or the class of a locus (or an envelope) which can

⁵¹We leave for chapter 5 the question of the similarity of De Jonquières' *indice* N and of Chasles' *caractéristique* μ , as well as that of *séries* and *systèmes*.

⁵²“Ces propriétés [des systèmes de sections coniques], qu'il faut connaître, s'expriment toutes en fonction de deux quantités, disons de deux *éléments* de chaque système. [...] Nous appellerons ces éléments les *caractéristiques* du système”, [Chasles, 1864b], p.298.

⁵³As we shall see in section 4.3.1, list-making practices would become a key component of Chasles' mathematical activity in the 1870s.

⁵⁴“Lieux géométriques”, “courbes enveloppes”, “propriétés diverses d'un système (μ, ν) ”, [Chasles, 1864b], pp.299;301;303.

be constructed from a system of conics. For instance, fixing a straight line in the plane, one can construct the pole of this straight line with respect to each conic of a system (μ, ν) . The resulting collection of points, Chasles asserts, is a curve of order ν . Similarly, one can construct collections of straight lines which will be the envelope of a curve whose class is given by one of these properties. In each case, the number at the heart of the proposition – that is to say, the order or the class of the constructed curve – is expressed as a combination of μ and ν (we shall turn below to the method used for the obtention of these properties, see section 3.2.2). In turn, these properties yield the numbers of conics

PROPRIÉTÉS D'UN SYSTÈME DE CONIQUES (μ, ν) .

Lieux géométriques.

- » I. Le lieu des pôles d'une droite est une courbe d'ordre ν .
- » COROLLAIRE. Si la droite est à l'infini, le théorème prend cet énoncé :
» Le lieu des centres des coniques est une courbe d'ordre ν .
- » II. Dans le cas où les coniques du système (μ, ν) passent toutes par deux points, en satisfaisant à deux autres conditions : le lieu des pôles de la droite qui joint ces points est d'ordre $\frac{\nu}{2}$.
- » COROLLAIRE. Et si les deux points sont à l'infini : le lieu des centres des coniques du système (μ, ν) est une courbe d'ordre $\frac{\nu}{2}$.
- » III. 1° Si de deux points Q, Q' on mène des tangentes à chaque conique d'un système (μ, ν) , les points d'intersection de ces tangentes sont sur une courbe d'ordre 3ν , qui a deux points multiples d'ordre ν , en Q et Q'.
- » 2° Si les coniques du système (μ, ν) sont toutes tangentes à la droite QQ', la courbe, lieu des points de rencontre des deux tangentes menées de Q et Q' à chaque conique, est d'ordre $\left(\frac{\mu}{2} + \nu\right)$, et a deux points multiples d'ordre $\frac{\mu}{2}$ en Q et Q'.
- » COROLLAIRES. Si Q et Q' sont imaginaires, à l'infini, sur un cercle, les points d'intersection des tangentes sont les foyers des coniques. Donc :
» 1° Le lieu des foyers des coniques d'un système (μ, ν) est une courbe d'ordre 3ν , qui a deux points multiples d'ordre ν , à l'infini, sur un cercle.
- » 2° Le lieu des foyers d'un système (μ, ν) de paraboles est une courbe d'ordre $\left(\frac{\mu}{2} + \nu\right)$ qui a deux points multiples d'ordre $\frac{\mu}{2}$ imaginaires, à l'infini, sur un cercle.

Courbes enveloppes.

- » XII. Les polaires d'un point enveloppent une courbe de la classe μ .
- » XIII. Lorsque toutes les coniques du système (μ, ν) sont tangentes à deux droites et satisfont à deux autres conditions : la courbe enveloppe des polaires du point de concours des deux droites est de l'ordre $\frac{\mu}{2}$.
- » XIV. Les cordes que deux droites fixes interceptent dans toutes les coniques d'un système (μ, ν) enveloppent une courbe de la classe 3μ , qui a deux tangentes multiples d'ordre μ coïncidant avec les deux droites.
- » XV. Les cordes communes à une conique U et à chaque conique d'un système (μ, ν) enveloppent une courbe de la classe 3μ .
- » XVI. Les tangentes menées aux coniques (μ, ν) , par les points où elles coupent une droite donnée D, enveloppent une courbe de la classe $(\mu + \nu)$, qui a la droite D pour tangente multiple d'ordre ν .
- » COROLLAIRE I. La courbe de la classe $(\mu + \nu)$ admet $(\mu + \nu)$ tangentes passant par un point quelconque. Prenant ce point à l'infini, sur une perpendiculaire à la droite D, on en conclut que :
» Le nombre des coniques d'un système (μ, ν) , qui coupent à angle droit une droite donnée, est $(\mu + \nu)$.
- » COROLLAIRE II. Si la droite D est à l'infini, le théorème prend cet énoncé :
» Les asymptotes des coniques d'un système (μ, ν) enveloppent une courbe de la classe $(\mu + \nu)$, qui a une tangente multiple d'ordre ν à l'infini.
- » Conséquemment la courbe a ν branches paraboliques.
- » XVII. L'enveloppe des droites dont chacune a le même pôle dans une conique donnée U et dans une conique quelconque du système (μ, ν) , est une courbe de la classe $(\mu + \nu)$.

[Chasles, 1864b], pp.299-301

in a system (μ, ν) satisfying a certain condition. For instance⁵⁵, let us fix an arbitrary conic U in the plane, as well as a system of conics (μ, ν) . Consider the points M of the plane whose polar lines relative to the conics of the system are the same as their polar lines relative to the fixed conic U . One of Chasles' properties states that the order of the locus of the points M is $(\mu + \nu)$. Now, this locus intersects U at $2(\mu + \nu)$ points, which correspond to all the conics in the system (μ, ν) which touch U . Therefore, to the above property of systems (μ, ν) , Chasles adds a “corollary” which states that the number of conics in a system (μ, ν) which satisfy the condition ‘to touch a given conic’ is $2(\mu + \nu)$. Note that Chasles, in his list, does not always include these corollaries which transform properties into conditions, nor does he explain how to move from one to the other. While he seems to think that this is, in general, simple, it remains unclear whether or not this passage is methodical in the sense that he attributed to this term.

Thus, these lists of properties serve to organize all of the properties of systems of conics. These properties, in turn, all depend on the same two numbers. Furthermore,

⁵⁵ [Chasles, 1864b], p.300. This example is discussed briefly in [Dieudonné, 1974], pp.39-40.

they can be transformed into numbers of conics in a system satisfying a given condition. In fact, this list was only the beginning: in order to expand his methods to ever more complex kinds of conditions, Chasles would produce many other such lists of properties: in his third communication, for instance, he gave such a list for angular conditions⁵⁶.

With the help of this list of properties of systems (μ, ν) , Chasles can carry out the construction and enumeration of all conics satisfying any five given conditions (provided the conditions correspond to properties in the list). To do so, Chasles presents a “general solving-process⁵⁷”. This general method, in keeping with Chasles' epistemology of generality, is uniform: in other words, “[its] march is always the same⁵⁸”). Chasles first presented it via an example, which we now reproduce with commentaries.

Chasles begins with selecting not five, but four conditions: indeed, to determine the characteristics of a system defined by four conditions is sufficient, since the number of conics satisfying five conditions will then be a simple matter of applying the right corollary to the system thus formed. More precisely, Chasles considers the following conditions⁵⁹:

- Z : To touch a curve of order m
- Z' : To have a focus on a curve of order p
- Z'' : To be similar to a given conic U
- Z''' : That one of the directrix be tangent to a curve of class q

To obtain the characteristics of the system (Z, Z', Z'', Z''') , Chasles begins by first introducing what he calls “elementary systems” (*‘systèmes élémentaires’*), that is to say the systems of conics passing through i points and touching $4 - i$ lines, i ranging from 0 to 4. Chasles denotes these systems respectively $(4p.,)$, $(3p., 1d.)$, $(2p., 2d.)$, $(1p., 3d.)$, $(, 4d.)$, where p and d of course stand for “point” and “droite”. Chasles also occasionally uses the expression “elementary condition” to refer to the conditions ‘passing through a given point’ and ‘touching a given straight line’. The characteristics of these systems were already well-known at this point; in fact, Chasles does not even attribute their discovery to anyone in particular⁶⁰. For instance, the characteristics of the system $(4p.,)$ are $\mu = 1$ and $\nu = 2$. Indeed, through five points pass one and only one conic; and two conics

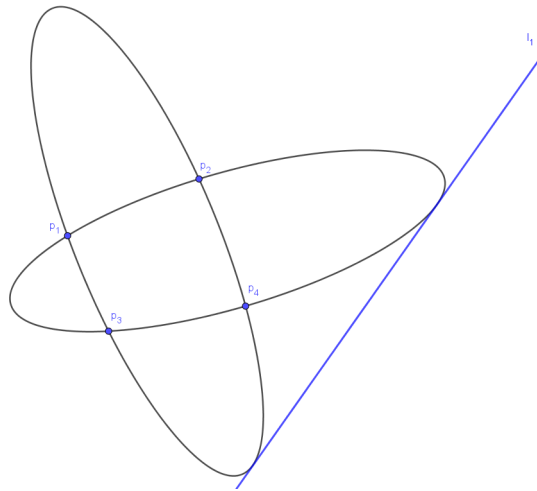
⁵⁶ [Chasles, 1864g].

⁵⁷ “procédé général de solution”, [Chasles, 1864b], p.299.

⁵⁸ “La marche que nous venons de décrire reste absolument la même”, [Chasles, 1864b], p.308. On Chasles' conception of generality as systematicity, see 1.2.2.

⁵⁹ This a rather complicated system, for which algebraic equations seem difficult to produce. Note how the choice in conditions exemplifies the range of the method, and the variety of conditions that Chasles is able to tackle at this point, beyond mere contact conditions.

⁶⁰ Steiner and De Jonquières had given the same numbers in their respective publications surveyed above, but these results are much older.



go through four given points and touch one given straight line (see fig. above). Note that some of the characteristics of these elementary systems are the same: for instance, the second characteristic of $(4p.,)$ (that is to say, 2) is equal to the first characteristic of $(3p., 1d.)$, since both characteristics express how many conics satisfy the same set of five conditions. Furthermore, due to the principle of duality, the first characteristic of the first elementary system is equal to the second characteristic of the fifth elementary system (which is 1 also because there is one and only one conic which touches five given straight lines). These numbers can all be written down within the following system of equations:

$$\begin{aligned} (4p.,) &\equiv (1, 2) \\ (3p., 1d.) &\equiv (2, 4) \\ (2p., 2d.) &\equiv (4, 4) \\ (1p., 3d.) &\equiv (4, 2) \\ (, 4d.) &\equiv (2, 1) \end{aligned}$$

To compute the characteristics of a system defined by four conditions, Chasles explains, one needs to substitute successively each of these conditions to the elementary conditions which form elementary systems. To do so, one must use the list of properties and select the four properties (or rather, their corollaries) which correspond to the four conditions. For instance, in Chasles' list, the corollary of theorem XI states that⁶¹:

Il existe dans un système de coniques (μ, ν) , $m[\mu(m-1)+\nu]$ coniques tangentes à une courbe donnée d'ordre m .

⁶¹“There is, in a system of conics (μ, ν) , $m[\mu(m-1)+\nu]$ conics tangent to a given curve of order m ”, [Chasles, 1864b], pp.300-301.

In other words, this corollary gives the number of conics in a system (μ, ν) which satisfy Z . Applying this corollary to the elementary system $(4p.,)$, one finds that there are $m(1 \times (m-1) + 2) = m(m+1)$ conics which go through four given points, and which touch a given curve of order m . But this number can also be viewed as the first characteristic of the system $(3p., Z)$. Similarly, applying this corollary to the system $(3p., 1d.) \equiv (2, 4)$, one finds $2m(m+1)$ conics which pass through three given points, touch a given straight line, and touch a given curve of order m . This number can also be viewed as the second characteristics of the system $(3p., Z)$. Therefore, one can write:

$$(3p., Z) \equiv (m(m+1), 2m(m+1))$$

This subprocedure is the substitution of an elementary condition by a more complex condition. It must be repeated in the other elementary systems, in the same way as before. Chasles obtains the following equations:

$$\begin{aligned} (2p., 1d., Z) &\equiv [2m(m+1), 4m^2] \\ (1p., 2d., Z) &\equiv [4m^2, 2m(m+1)] \\ (3d., Z) &\equiv [2m(m+1), m(m+1)] \end{aligned}$$

As in the previous set of equations, duality means that there is a symmetry in these equations: the second characteristic of the last system is the first characteristic of the first system, and so on. Furthermore, a form of transitivity also makes it unnecessary to actually carry out every one of these computations: the second characteristic of the first system is the first characteristic of the second system, and so on.

These four systems which include the condition Z can now play the role of elementary systems, as Chasles substitutes the condition Z' to the remaining elementary conditions. This is done in the same manner as for Z : Chasles locates the property corresponding to the condition Z' , then uses it to compute numbers of conics which he then reinterpretes as characteristics of more complex systems. This goes on until all elementary conditions have been replaced, and the characteristics of (Z, Z', Z'', Z''') have been computed. The resulting expressions for these characteristics are integral polynomials in m, p, q ; that is to say the orders and classes of the given curves which form the conditions to be satisfied.

Let us deviate from Chasles' example and suppose we want to compute instead the number of conic sections touching 5 given conics, that is to say curves of order 2. Let us represent this condition by the letter Z . The property we used above (namely the corollary of theorem XI) shows that, in a system (μ, ν) , there are $2(\mu + \nu)$ conics satisfying Z . Then,

from what precedes,

$$\begin{aligned}(3p., Z) &\equiv [6, 12] \\ (2p., 1d., Z) &\equiv [12, 16] \\ (1p., 2d., Z) &\equiv [16, 12] \\ (3d., Z) &\equiv [12, 6]\end{aligned}$$

Now, the number of conics in the system $(3p., Z)$ satisfying the condition Z a second time, which Chasles denotes $N(3p., 2Z)$, is, according to the same property as previously, $2 \times (6 + 12) = 36$. Similarly,

$$\begin{aligned}N(2p., 1d., 2Z) &= 2(12 + 16) = 56 \\ N(1p., 2d., 2Z) &= 2(16 + 12) = 56 \\ N(3d., 2Z) &= 2(12 + 6) = 36\end{aligned}$$

where $2Z$ stands for Z, Z' . Consequently, we have the characteristics of the following family of systems:

$$\begin{aligned}(2p., 2Z) &\equiv [36, 56] \\ (1p., 1d., 2Z) &\equiv [56, 56] \\ (2d., 2Z) &\equiv [56, 36]\end{aligned}$$

Continuing with the same method, we find that:

$$\begin{aligned}N(2p., 3Z) &= 2(36 + 56) = 184 \\ N(1p., 1d., 3Z) &= 2(56 + 56) = 224 \\ N(2d., 3Z) &= 2(56 + 36) = 184\end{aligned}$$

and

$$\begin{aligned}(1p., 3Z) &\equiv [184, 224] \\ (1d., 3Z) &\equiv [224, 184]\end{aligned}$$

where $3Z$ stands for Z, Z', Z'' . Substituting Z one last time, we find:

$$N(1p., 4Z) = 2(184 + 224) = 816 = 2(224 + 184) = N(1d., 4Z)$$

wherefrom it follows that $(4Z) = (Z, Z', Z'', Z''') \equiv [816, 816]$. The number of conics

touching five other conics, therefore, is $2(816 + 816) = 3264$; a result for which Chasles would be long remembered.

4.2.2 The generalized principle of correspondence

The method of substitution is a general and uniform procedure: its application to any five conditions is always the same, and it requires no computations other than elementary additions and multiplications. However, this is only so provided one has access to the adequate properties to actually know how many conics in a system (μ, ν) satisfy a given condition Z . During his fourth communication at the Paris Académie, Chasles presented a “general method for the search and proof of the properties of systems⁶²”, with which one can form lists of properties such as the one mentioned above. This very method of proof, like the method of substitution, “rests on the notion of the two characteristics⁶³”. This is crucial because it means that the theory of systems of conics here envisioned by Chasles is indeed centered around fundamental notions, which serve as the principles of both the computation of numbers of conics, and in the derivation of the properties of systems. Furthermore, this method of proof, like the method of substitution, is uniform; or, as Chasles puts it, when applied to a specific question, “the reasoning is always the same⁶⁴”.

This method of proof is a generalized version of the principle of anharmonic correspondence described in section 3.1.3, and is reminiscent of the proof used by De Jonquières in his 1861 paper which we presented in section 4.1.2. Although Chasles would only make the connection between both principles explicit in 1866, he had in 1864 already suggested a continuity between both kinds of correspondences⁶⁵. As Chasles points out, it is a method whose applicability ranges over curves of any order, and not just conics. It rests on two lemmas, which are the dual versions of one another; the first of them reads as follows⁶⁶:

Soit x et u deux séries de points sur une droite L , entre lesquelles existe une correspondance qui à un point x associe α points u , et qui à un point u associe β points x .

Alors le nombre de points fixés par la correspondance est $(\alpha + \beta)$.

⁶²“Procédé général de recherche et de démonstration des propriétés des systèmes”, [Chasles, 1864a], p.1174.

⁶³“Nos procédés de démonstration [...] [reposent] encore sur la notion des deux caractéristiques”, [Chasles, 1864a], p.1172.

⁶⁴“Le raisonnement que l’on fait pour cela est toujours le même”, [Chasles, 1864a], p.1174.

⁶⁵Chasles mentions the “theory of homographic figures” in [Chasles, 1864a], p.1172.

⁶⁶“Let x and u be two series of points on a straight line L , between which there exists a correspondence which, to a point x match α points u , and to a point u match β points x ”, [Chasles, 1864a], p.1175.

To prove this result, Chasles fixes a straight line L , and lets x and u denote the distances between the points x and u , and an arbitrary origin on L . Chasles uses letters here in a rather ambiguous manner: they represent both points and distances, and one letter represents a mobile point, or indeed a multiplicity of points. This is not entirely different from the literary technology employed for the writing of geometrical equations of curves, as described in section 3.1.2. Chasles states, without further justification, that an equation of the following form holds:

$$x^\beta(Au^\alpha + Bu^{\alpha-1} + \dots) + x^{\beta-1}(A'u^\alpha + B'u^{\alpha-1} + \dots) + \dots = 0$$

Indeed, a correspondence in Chasles' geometry can only be obtained by constructions and transformations which are representable by algebraic equations. Therefore, there holds an algebraic equation between the corresponding points x and u . Once u is fixed⁶⁷, there must be exactly β corresponding points x , that is to say β solutions of the resulting polynomial equation in x , which implies that this polynomial is of degree in β . Similarly, the degree in u must be α . It remains only to factorize and rearrange this equation, and it shall be as expressed above.

To represent the situation where a point x coincides with some corresponding points u , one simply has to rewrite the equation above by equating x and u ; in other words, the coinciding points x (or double points) are solutions of the equation

$$Ax^{\alpha+\beta} + (B + A')x^{\alpha+\beta-1} + \dots = 0$$

This indicates that there are $\alpha + \beta$ coinciding points, provided that the coefficient A is not equal to zero. To show that this can never be the case⁶⁸, Chasles divides the entire equation in (x, y) by u^α , and takes u to be at infinity (on L), thus rewriting the equation as:

$$Ax^\beta + A'x^{\beta-1} + \dots = 0$$

This equation must still have β solutions, for even to the point at infinity correspond β points x . Therefore, $A \neq 0$, and the equations whose roots are the coinciding points $x = u$ is of degree $\alpha + \beta$, which is consequently the number of coinciding points. A dual lemma states that if, about a fixed point, revolve two series of straight lines x and u so that to

⁶⁷Of course, if u is a root of the polynomial $Au^\alpha + Bu^{\alpha-1} + \dots$, then this reasoning seems invalid. However, Chasles seems to practice a generic form of reasoning here, wherein a finite number of exceptions can be disregarded for the sake of generality.

⁶⁸This is Chasles' correction to what he had identified as a mistake in a proof by De Jonquières of a similar result. De Jonquières, however, would later claim that Chasles' fix had in fact been found previously by Cremona. The story of this principle is rather messy, and we leave it aside for now, for the sake of clarity. In 5.1, we will discuss in greater detail the larger priority dispute between Chasles and De Jonquières, in which this principle plays a key role. See also [Segre, 1892].

each straight line x correspond α straight lines u , and conversely to each straight line u correspond β straight lines x , then the number of coinciding lines is $\alpha + \beta$.

How can these two lemmas serve as a uniform and general method for the proof and search of the properties of systems? The mathematical content of this more general principle, like that of the principle of anharmonic correspondence, is not what makes Chasles' method worthwhile. Rather, Chasles' creation is that of a textual dispositif for the systematic writing of proofs of propositions pertaining to systems of conics⁶⁹. Within his series of communications between 1864 and 1867, only a few proofs were actually given by Chasles as examples, but from his students we can gather additional examples. Despite some minor variations, the structure of these proofs is always the same. We first turn to one such example, given in Chasles' fifth communication at the Paris Académie. We present this particular proof, before discussing its general structure⁷⁰.

In this example, Chasles sets out to prove that “in a system of conics (μ, ν) , the diameters which end at the points where a straight line L intersects the conics of the system are the envelope of a curve of class $(\mu + 2\nu)$ ”⁷¹. In other words, in the plane, we have one fixed straight line L , and a system of conics. Each conic of the system intersects the straight line L in two points P_1 and P_2 . From these two points, one can draw two diameters, that is to say two straight lines which pass through the center of the conic. Doing so for all conics in the system, we have a collection of straight lines, which envelope a curve whose class is given by the theorem⁷².

Chasles then reformulates the *demonstrandum*, and writes⁷³: let us prove that, through an arbitrary point I , there pass $(\mu + 2\nu)$ straight lines, each of which join a point a of L to the center of a conic passing through a . Indeed, per definition, the class of a curve is the number of tangents which can be drawn to it from any arbitrary point I . The theorem is true if and only if, from through an arbitrary point I , there pass $(\mu + 2\nu)$ straight lines which belong to the envelope described in the theorem.

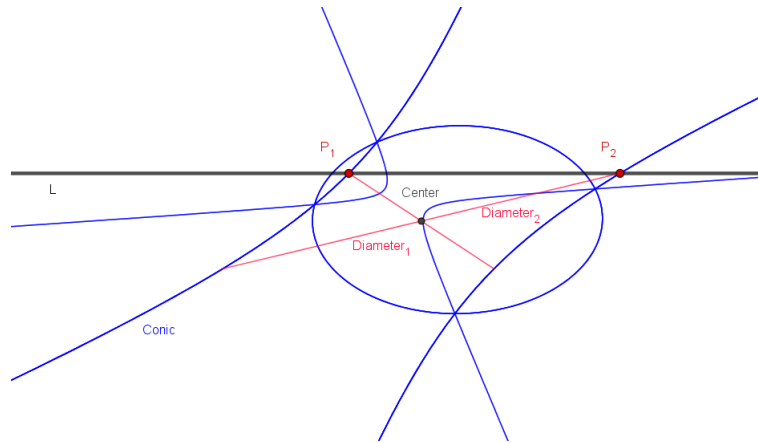
⁶⁹The textual dispositif used by Chasles to this end evolved from his first attempts in 1864 until his later texts in the 1870s; see section 4.3.2 for that later uses of the principle of correspondence.

⁷⁰This proof is adapted from [Chasles, 1864d], p.7.

⁷¹“Dans un système de coniques (μ, ν) , les diamètres qui aboutissent aux points où une droite L coupe les coniques, enveloppent une courbe de la classe $(\mu + 2\nu)$.”

⁷²It is a rather striking feature of a lot of Chasles' propositions, in this text and many later ones, that they are very difficult to parse for the untrained reader. One may try to think of a conic undergoing a continuous transformation, and try to imagine a correlative transformation of the other elements constructed with this conic – here, the intersections with L , the centers, and diameters. This, however, proves to be rather difficult as the constructions get more complex. Furthermore, Chasles systematically replaces properties which are easy to visualize by their more abstract counterparts: instead of demanding that a certain angle between two variable straight lines be constant, for instance, he would fix two other straight lines, and demand that the cross-ratio of these four lines be constant. To even grasp the content of Chasles' propositions, therefore, is something that requires training, and a specific reading practice.

⁷³In fact, Chasles writes a slightly more general proof; but we first present a simpler case for the sake of clarity. We shall come back to the more general proof shortly.



Chasles then forms a correspondence between the straight lines which pass through I . Such a line intersects L at a single point x ; and Chasles denotes this line Ix . According to a property of systems of conics, which had already been stated in an earlier communication on the theory of characteristics⁷⁴, the locus of the centers of the conics in a system (μ, ν) is of order ν . Thus, there are ν centers of conics of the system on the straight line Ix . By forming the intersections of each of these conics with L , Chasles constructs 2ν points u on L . This is the first half of a correspondence between straight lines passing through I . To form the reverse correspondence, Chasles forms the μ conics which pass through a point u . The straight lines joining I and the centers of these conics intersect L at μ points x . Thus, to a straight line Ix correspond 2ν straight lines Iu , and to a straight line Iu correspond μ straight lines Ix . Per the principle of correspondence, there are $\mu + 2\nu$ coinciding straight lines. Each of these coinciding straight lines goes through I . Moreover, it joins a point a of L and the center of a conic which goes through a ; as it satisfies the criterion of construction of both parts of the correspondence. More precisely: as a line Iu , it corresponds to a straight line Ix . This means that u is the intersection of a conic of the system and L . As a line Iu , it corresponds to a Ix , which means that it joins I and the center of a conic which passes through u . Because $Ix = Iu$, this coinciding straight line is as required; and the theorem is proven.

In fact, this is not exactly Chasles' proof. After stating this theorem, he gave another, slightly more general one; with poles of a fixed straight line with respect to the conics of the system instead of their centers. This is because the center of a conic is the pole of the straight line at infinity with respect to said conic. The reason for stating both theorems, and proving only the more general one, is that Chasles wished to stress the structural similarity between both proofs; and the fact that, with the principle of correspondence; proving the general case is just as easy as proving the particular. We shall come back to this issue at the end of this chapter (see 4.3.2). Furthermore, to this theorem, Chasles

⁷⁴ [Chasles, 1864b], p.299.

added some extra information: the straight line L , he showed, is a multiple tangent of the curve constructed here, of multiplicity 2ν .

This proof is very typical of Chasles' uses of the principle of correspondence between 1864 and 1873. When the property to be proven involves the order of a locus, Chasles will consider a fixed line L , and construct a judicious correspondence between two series of points x and u on L , so that the coinciding points of the correspondence be the intersections of L and the locus. When, as in our example, the property involves the class of an envelope, Chasles forms two series of straight lines Ix and Iu passing through a fixed point⁷⁵ I . The notations in these proofs are extremely stable, and so are the sentences that Chasles uses in them. The general structure of the textual apparatus associated to the principle of correspondence can be decomposed in the following manner:

- **Statement of the property.** These statements almost always describe a locus or an envelope, constructed from a system (μ, ν) ; and stipulate its order or its class. Sometimes, Chasles immediately reformulates the property in more general terms, as a way to display the generality of his method: the proof, he shows, remains the same at a structural level when generalizing a property.
- **Conversion of the statement.** If the property pertains to the order of a locus, it will be reformulated to state that any arbitrary straight line L intersects said locus at a certain number of points, which is equal to the order stipulated above. If the property pertains to the class of an envelope, it will be reformulated via a sheaf of straight lines drawn from an arbitrary point I . The notations for I and L are always the same. Often, the intersections of L and the locus (or the tangents to the locus drawn from I) can be reinterpreted geometrically as satisfying a certain condition.
- **Construction of a correspondence.** Chasles always starts with one point x on the straight line L (or with one line IX from the sheaf centered around I). Using the definitions of the characteristics μ, ν , and geometrical properties of conics, he associates to a point x (resp. a line IX) a (finite) collection of points u (resp. lines IU). He then constructs a reciprocal correspondence from u to x (IU to IX). The strategy in constructing this correspondence is to make it so that corresponding points (resp. lines) will be the intersections of the locus and L (resp. the tangents to the locus drawn from I). To that end, Chasles often uses the geometrical reinterpretation of these intersections (resp. tangents) as the points (resp. lines) satisfying certain conditions. Both parts of the correspondence are constructed so that an

⁷⁵Chasles mostly denoted these lines IU and IX , perhaps using capital letters to emphasize the duality in both reasonings.

object satisfying the aforementioned condition must belong to the images of both correspondences.

- **Application of the principle of correspondence.** By adding the two numbers of corresponding points (or lines), Chasles obtains the number of coinciding points (or lines).
- **Interpretation of the coinciding points.** The coinciding points (or lines), when the correspondence has been aptly constructed, can be interpreted as the points of intersection of (the tangents to) the locus (the envelope) at the heart of the property. This means that the number of coinciding points gives the order or the class which was being sought. Sometimes, certain elements of the construction can be interpreted further, as leading to specific types of correspondences.

Some of these steps are sometimes omitted, or left implicit: for instance, in the proof reproduced above, the ‘interpretation’ is presented as a trivial deduction. Correspondences, in the wake of these publications, would become Chasles’ most commonly-used expressive resource to write and prove properties of algebraic curves and surfaces, beyond the theory of characteristics. In this context, a supplementary step would be added from 1871 onwards, in which are counted what Chasles calls “solutions étrangères”, that is to say coinciding points or lines that arise artificially in the correspondence, but which should not be counted in the final result. We shall come back to these special solutions in section 4.3.2.

4.2.3 A general formula for enumerative problems

Chasles’ claim that his method of proof and discovery for the properties of systems of conics is perfectly general and uniform remains at this stage somewhat unjustified. First, it is unclear how we are to know that every condition on conics can be obtained as a corollary of a property of the systems (μ, ν) . Moreover, even if this is the case, it is not totally explicit how one should go about forming a correspondence which leads to a proof of said property. Lastly, we do not know a priori why this property will yield a number depending only on μ and ν .

To these first two objections, one could answer that Chasles demonstrated the generality of his methods by multiplying examples: over the course of his publications, he gave many examples of properties of systems, of proofs thereof via the principle of correspondence, and of their transformations into corollaries pertaining to conditions on conics. Furthermore, he did so whilst using a very systematic mode of exposition.

The third objection is more substantial. Of the dozens of properties listed by Chasles, all expressed a number in terms of the two characteristics of a system of conics. However,

because this objection has to do with the form of a mathematical result, and not with the modalities of the application of a method, it was soon read as an empirical observation, or a conjecture, upon which the entirety of the theory of characteristics rested. This would be the case, for instance, of the German mathematician Alfred Clebsch, for whom Chasles had stated, but not proved, the following theorem⁷⁶:

For each condition Z , there are two numbers α, β so that for each system $S \equiv (\mu, \nu)$, the number of conics in S satisfying Z is $\alpha\mu + \beta\nu$.

This conjecture would be known either as Chasles' theorem, Chasles' formula or sometimes the $\alpha\mu + \beta\nu$ theorem.

Chasles indeed believed such a result to be true. In his seventh communication to the Paris Académie des Sciences, for instance, he wrote⁷⁷:

On n'aurait pas prévu certainement que des questions si variées, et jusqu'ici presque inaccessibles aux méthodes analytiques, se résoudraient toutes par une même méthode, et surtout par une même formule. Cependant la raison en paraîtra bien simple maintenant; car elle dérive d'une remarque à laquelle conduisent les nombreux théorèmes sur les systèmes de coniques, contenues dans mes dernières communications. C'est que les propriétés de ces systèmes s'expriment toujours par une fonction telle que $(\alpha\mu + \beta\nu)$ des deux caractéristiques du système; fonction dans laquelle α ou β peuvent être nuls.

Chasles thought that every property of the systems (μ, ν) could be expressed as a linear function of μ and ν ; and that, therefore, to every condition Z there corresponded an expression $(\alpha\mu + \beta\nu)$ which he called the "module" of Z . This is not an unreasonable observation: after all, every proof per correspondence ends up with a sum, both terms of which are always a function of the characteristics of the system (μ, ν) , as per the general method described above. Of course, this is no proof.

In the pages following the quote above, Chasles used modules to express a general formula for the number of conics satisfying any five conditions. Considering four conditions Z, Z', Z'', Z''' whose modules are respectively

$$\alpha\mu + \beta\nu, \alpha'\mu + \beta'\nu, \alpha''\mu + \beta''\nu, \alpha'''\mu + \beta'''\nu$$

⁷⁶The circulation of this 'formula' is a main focus of the chapters 6 to 8.

⁷⁷"It could not have been foreseen with certainty that such varied questions, heretofore almost inaccessible to analytical methods, can be all resolved by one single method, and especially by one single formula. However the reason will seem rather simple now; for it derives from a remark to which the many theorems on systems of conics, contained in my latest communications, lead. This remark is that the properties of these systems are always expressed by a function like $(\alpha\mu + \beta\nu)$ of the two characteristics of the system; function in which α or β can vanish", [Chasles, 1864f], p.215.

Chasles observed that one can apply the general procedure described previously and successively substitute the elementary conditions. Without providing much detail, Chasles expresses the characteristics of the general system

$$(Z, Z', Z'', Z''') \equiv \left[\begin{array}{l} (\alpha\alpha'\alpha''\alpha''' + 2\Sigma\alpha\alpha'\alpha''\beta''' + 4\Sigma\alpha\alpha'\beta''\beta''' + 4\Sigma\alpha\beta'\beta''\beta''' + 2\beta\beta'\beta''\beta'''), \\ (2\alpha\alpha'\alpha''\alpha''' + 4\Sigma\alpha\alpha'\alpha''\beta''' + 4\Sigma\alpha\alpha'\beta''\beta''' + 2\Sigma\alpha\beta'\beta''\beta''' + \beta\beta'\beta''\beta''') \end{array} \right]$$

where the signs Σ “represent the sum of similar terms⁷⁸”. This means that, for instance, $\Sigma\alpha\alpha'\alpha''\beta''' = \alpha\alpha'\alpha''\beta''' + \alpha\alpha'\beta''\alpha''' + \alpha\beta'\alpha''\alpha''' + \beta\alpha'\alpha''\alpha'''$. From this formula, Chasles easily derives another general formula for the number of conics satisfying five unspecified conditions, whose modules are all known and expressed under the form $\alpha\mu + \beta\nu$.

However, the validity of these general formulae does not hinge upon that of the conjecture above. All that is required to use them is that the four or five conditions at hand have modules of the form $\alpha\mu + \beta\nu$; which, for Chasles, had always been the case so far. Furthermore, Chasles viewed the use for these general formulae to be restricted to faster computations of characteristics or numbers of conics satisfying four or five conditions. Having obtained the modules of certain conditions, this formula allowed Chasles to bypass the method of substitution, and to compute characteristics and solutions only by operating on the coefficients of said modules.

Chasles never sought to study the algebraic or theoretical properties of these general formulae themselves. More importantly, he never sought to prove the $\alpha\mu + \beta\nu$ theorem, nor did he express interest in the attempts of those who took up the theory of characteristics throughout the 1870s. In fact, he probably did not view it as a theorem per se, or at least not as something worth seeking a proof for. Instead, the $\alpha\mu + \beta\nu$ formula was for Chasles a remark, a sign that the theory of systems of conics ought to be founded upon the two fundamental notions that are the characteristics.

Indeed, remember the epistemic portrait of the geometer as sketched by Chasles in the *Aperçu Historique*, as we reconstructed it in chapter 1. For Chasles, mathematical life was not about the difficult obtention of ingenious proofs of difficult theorems; but rather about the search for natural methods and fundamental principles around which to center a theory. At the core of this understanding of mathematical practice was the creed that nature provides simple paths toward general truths; and the surest sign that such a path had been found was the vast number of propositions which were effortlessly derived at the end of the journey. By centering the theory of conics around the two characteristics, and by attaching to it the method of proof based on the principle of correspondence, Chasles had produced long lists of propositions, and had solved problems which theretofore had proven difficult for analytical and synthetic geometers alike. These propositions and these

⁷⁸ [Chasles, 1864f], p.216.

enumerations were a surer sign of the naturalness and perfection of his theory than any elaborate proof of the $\alpha\mu + \beta\nu$ theorem could ever be. Because of his understanding of generality, and of what mathematical life consists in, Chasles identified theorems and conjectures in a different manner than we – or many other geometers in the second half of the 19th century – would.

4.2.4 The analogy with Algebra

Chasles viewed the applicability of the theory of characteristics, based on the method of substitution and the principle of correspondence, as universal. “Within the theory of geometrical characteristics”, Chasles wrote in one of his communications, “its generality means that [this method] participates in what characterizes the *Analytical Art*, whereof Vieta said: Quod est, *nullum non problema solvere*⁷⁹”. This was the conclusion reached by Chasles after finding the general formulae discussed in the previous section.

In fact, in the fourth of his communications on the theory of characteristics, Chasles had already constructed a more intricate account of the “differences between [his general method] and the analytic method⁸⁰”. While Chasles did not explicitly refer to specific texts or authors, which he deemed representative of this analytical method (which he once again identifies with Cartesian geometry), two reasonable guesses of what he likely had in mind can be found in the works of British geometers Arthur Cayley and George Salmon, among others. Not only was Chasles aware of what British mathematicians were producing at the time, and exchanging with the two mentioned above, he also perceived them as typical of modern analytical methods in other contexts⁸¹. Chasles’ understanding and description of contemporary algebraic methods did not include any of the innovations one can find in German authors of that time. When Chasles mentions algebraic ‘equations of condition’, he does so without knowing that authors like Otto Hesse had used this exact same term (‘*Bedingungsgleichung*’) in a way that does not entirely match Chasles’ description⁸². Chasles is equally not aware, or not interested, in the theory of forms and invariants, which is yet another example of an algebraic approach emerging at this moment, and which does not exactly fit Chasles’ description⁸³. Furthermore, Chasles’ diagnosis of the problems that plague analytical methods will seem unfair to any reader

⁷⁹“Dans son domaine actuel de la théorie des courbes géométriques, on reconnaîtra peut-être que par sa généralité, [cette méthode] participe du caractère de l’*Art analytique* dont Viète a pu dire: Quod est, *nullum non problema solvere*”, [Chasles, 1864f], p.218. Vieta’s Latin sentence can be translated as ‘there is no problem that cannot be solved’ (by it).

⁸⁰ [Chasles, 1864a], p.1167-1173.

⁸¹See for instance his description of Salmon’s work on the cubics passing through nine given points, given in [Chasles, 1853a], p.946, and briefly discussed in section 3.1.1.

⁸² [Hesse, 1861], p.129.

⁸³ [Parshall, 1989].

familiar with the contemporary work of Plücker (or even Bischoff, for that matter) - in fact, Chasles' view of 'modern' analytical methods seemingly did not really evolve after the publication of the 1837 *Aperçu Historique*. Nonetheless, our goal in this section is merely to show how an analogy with a certain image of Algebra and analytical methods was key in the shaping of Chasles' own theory of characteristics.

An example of what Chasles has in mind when discussing analytic methods applied to the problem of enumerating conics can be found in Salmon's *A Treatise on Conic Sections*. A very popular treatise, Salmon's *Conics* was first published in 1848, but revised and re-edited three times during the years leading up to Chasles' series of communications. In both the third and fourth editions of this treatise (respectively published in 1855 and 1863), the first section of the tenth chapter, on the general equation of second degree, focused on the *number of Conditions which determine a Conic*⁸⁴.

Salmon opens with "the most general form of the equation of the second degree", which he writes:

$$ax^2 + 2hxy + by^2 + 2gx + 2fy + c = 0$$

A set of coefficients (a, b, c, f, g, h) determines one conic; however, dividing this general equation by c does not change the curve being represented. Indeed, Salmon explains, "the nature of the curve depends not on the *absolute magnitude*, but on the *mutual ratios* of these coefficients". Hence, fixing $c = 1$, to determine a curve is to determine five constants, or coefficients. For Salmon, this means that "five relations between the coefficients" are sufficient to determine the five quantities $\frac{a}{c}$ etc., or as reformulated in the title of the section, that five conditions determine a conic. In his 1852 *A Treatise on the Higher Plane Curves*, which was intended as a sequel to the *Conics*, Salmon reproduced the same reasoning to explain that a curve of the n^{th} degree is determined by $\frac{n(n+3)}{2} - 1$ conditions⁸⁵. In both cases, only one example is given, namely that of a conic (resp. curve of degree n) determined by 5 (resp. $\frac{n(n+3)}{2} - 1$) points. Substituting the coordinates for these points into the general equation, Salmon asserts without any further explanation that one obtains enough relations between the coefficients to determine them. How this was to be done was sufficiently elementary so as to require no supplementary details on Salmon's part. Furthermore, Salmon did not feel the need to express the requisit that no three of the five given points be on a line, lest the relations provide an infinite number of solutions, as most modern textbooks of analytical geometry explain.

⁸⁴ [Salmon, 1855], pp.118-120, [Salmon, 1863], pp.126-127. Both sections present minor differences - the notations are slightly different, and a few paragraphs were rewritten -, which we will not go into. In what follows, we elect to focus on the latest available version at the time of Chasles' publications, namely the 1863 edition. Chasles himself occasionally referred to Salmon's 1850 second edition (under a French title) in other contexts; see for instance [Chasles, 1870], p.118.

⁸⁵ Salmon goes on to explain why this is only valid in some cases, implicitly referring to the well-known Euler-Cramer paradox, see [Salmon, 1852], pp.19-21.

For the sake of clarity, let us fill the gaps in Salmon's exposition: given five points of coordinates $(x_1, y_1), \dots, (x_5, y_5)$, the conditions that the conic passes through each of these five points can be written as the following system of equations (whose unknowns are the coefficients a, b, f, g, h):

$$ax_i^2 + 2hx_iy_i + by_i^2 + 2gx_i + 2fy_i + 1 = 0, \text{ for } i \text{ ranging from } 1 \text{ to } 5$$

This is a linear system with five equations and five unknowns, and we can eliminate the unknowns one after the other. From the first equation, we obtain for instance that $a = \frac{-1}{x_1^2}(2hx_1y_1b + \dots)$. We can then replace a in each of the other four equations, and obtain four equations with only four unknowns (b, f, g, h) . Repeating this method will yield one final equation in, say, h , which will be entirely determined as a (linear) algebraic expression in the given coordinates x_i, y_i . Once h is determined, it suffices to go back and determine successively the other coefficients. Provided the points are in generic position (which, for the sake of simplicity, we shall simply take to mean here that no three of them are aligned), then these equations are indeed independent; and this operation indeeds leads to the computation of one and only one set of coefficients (a, b, f, g, h) .

More generally, this method is theoretically valid for conditions more complex than 'passing through a given point'. Each condition is then taken to be an algebraic relation in the coefficients of the general equation of conics. Thus, to enumerate the conics satisfying five conditions is to find the number of solutions of a system of five (independent) algebraic equations in five unknowns. If one can successfully eliminate the unknowns one after the other, then the degree of the resulting equation in the last remaining unknown will be the number of conics, per the fundamental theorem of algebra. While only the degree of this resulting equation is needed (as opposed to the equation itself), this method of elimination does require that one forms the algebraic relations which translate all five conditions, and that one computes with all of their coefficients.

This seems to be a fairly accurate description of what Chasles took to be the "analytical method" for the enumeration and construction of all conics satisfying five conditions. For Chasles, this "universal instrument of mathematics" that is Analysis fails to adequately treat curves satisfying non-elementary conditions for two reasons⁸⁶. First, the expression of each condition through an "équation de condition" can be very difficult. Unlike the case of the curve passing through a point, more complex conditions require more work, whether by means of the introduction of auxiliary equations or of other variables. This means not only that there is no systematic and uniform method for the formation of these equations, but also that the difficulty can increase depending on the condition, whereas

⁸⁶ [Chasles, 1864a], p.1168.

the principle of correspondence works uniformly for all conditions, and never introduces specially long proofs. Second, the elimination of the coefficients in these equations is not always possible, and is rarely simple. This is obvious from a mere glance at some of the numbers found by Chasles: the elimination in the equations for five contact conditions could lead to equations of degree 3264, as this is the number of conics touching five given conics⁸⁷.

But the theory of characteristics is more than a substitute for the imperfect analytical method: in fact, Chasles constructs an analogy between both methods, whereby the simplicity and uniformity of the former is made to contrast with the difficulty and untractability of the latter. In Chasles' view, the "formation" of *équations de condition* and the procedure of "elimination" correspond respectively to the "selection" of relevant properties of systems of conics and the method of "substitutions"⁸⁸. Indeed, instead of forming equations for a condition, one can apply the principle of correspondence to obtain (in a systematic manner) a property of systems (μ, ν) , of which a corollary will yield the module of said condition. Furthermore, instead of eliminating equations, one introduces conditions one by one into systems whose characteristics are already known, starting with elementary systems. If one has already introduced a condition Z , and wishes to introduce a second condition Z' , then it suffices to compute the numbers of conics $N(3p., Z, Z'), N(2p., 1d., Z, Z')$ etc., which is always a simple computation if one knows the module of Z' (in that only additions will be required).

The comparison runs deeper: the consideration of systems (μ, ν) , Chasles explains, replaces "the general equation which represents conics in Analysis"⁸⁹. By having two numbers stand in for four conditions, with all the data which define them (such as the position of the given points or curves), systems introduce in pure geometry the kind of abstraction that used to characterize analytical methods. Where the degree of an equation characterizes a family of curves, and allows for a general treatment thereof, so do the two characteristics μ, ν suffice to treat systems of conics irrespective of the contingent properties which define them, such as the relative position of the points through which conics ought to pass⁹⁰ (and the two characteristics can respectively be viewed as the

⁸⁷Of course, Bischoff had produced similarly large numbers, by means other than this naive method of elimination.

⁸⁸"Ce procédé d'opérations, toujours le même, est excessivement simple. Il remplace, comme on voit, les éliminations de l'Analyse. On peut dire que c'est une méthode de *substitution*, au lieu d'une méthode d'*élimination*, dans le sens technique du mot", [Chasles, 1864a], p.1171.

⁸⁹"Les systèmes font l'office de l'équation générale qui représente les coniques, en Analyse", [Chasles, 1864a], p.1169.

⁹⁰"L'étude des propriétés des systèmes de coniques acquiert le caractère d'abstraction et de généralité des théories analytiques, puisque l'on n'a pas à tenir compte des conditions variées auxquelles satisfont les systèmes que l'on considère, mais seulement des deux caractéristiques abstraites qui les représentent", [Chasles, 1864a], p.1169.

order and the class of the system⁹¹). In fact, because the analytical method envisaged by Chasles does need to form equations for each condition, it does not possess this level of abstraction when applied to systems of conics. This means that the “geometrical formulae” produced by the principle of correspondence and the consideration of systems (μ, ν) are fundamentally different from the “formulae of the analytical method”: the former “ignore particular cases”. This means that, from the module of a condition, one cannot derive any information regarding the nature or position of the actual conics in a given system which satisfy said condition⁹². By contrast, having formed equations for conditions, and carried out the method of elimination, one can form the equations of the conics which satisfy all five conditions, and discuss their positions or nature. In return, Chasles explains, this abstraction is responsible for the “immense advantage” that the theory of characteristics has over analysis, in that it provides a uniform, general, and simple method for the enumeration of conics.

Thus, the theory of characteristics is a clear outgrowth of the broader research programme undertaken by Chasles in his lectures at the Faculté de Paris: systems (μ, ν) , like sheaves of conics and straight lines before them, enable the writing of geometrical equations as an alternative to the equations of algebra. These geometrical equations, however, are intrinsic: in lieu of artificial coordinate systems and variables, they require only the consideration of notions which naturally *characterize* the figure at hand. That the two numbers μ and ν do that for a system of conics is made manifest by the simplicity and generality of the method of characteristics.

4.2.5 Exceptional conics and geometrical beings

Chasles' search for geometrical generality and abstraction brought several new difficulties and hurdles. The theory of characteristics, and its uniform mode of enumeration, necessarily yields numbers which include solutions whose status is problematic. Complex curves, for instance, must necessarily be counted, since no specific consideration is granted to the relative position of the given elements which define systems and conditions. In fact, amongst Chasles' 3264 conics touching five other conics, most are complex curves – except in very rare situations⁹³. Similarly, Chasles had realized that his systems, invariably, contained degenerate conics.

Chasles, in his lectures at the Faculté de Paris, had founded his general theory of

⁹¹This analogy is made explicit in [Chasles, 1871b], pp.577-578.

⁹²“Il y a [...] une différence entre nos formules, que j'appellerai *géométriques*, et les formules de la méthode analytique. Celles-ci se prêtent à la discussion des cas particuliers, parce qu'elles sont empreintes de toutes les données de la question”, [Chasles, 1864e], p.357.

⁹³A set of five conics which give rise to 3264 real solutions has been given in [Ronga *et al.*, 1997]. More recently, it has been shown that at least 32 of these 3264 conics are always real, see [Welschinger, 2005].

conics upon two “fundamental properties⁹⁴”, which served as the punctual and tangential (geometrical) equations of these curves. These two properties are none other than the descriptions of conics as, respectively, the locus of the intersection of two homographic sheaves of rays centered around two fixed points, and the envelope of the straight lines joining the points of two homographic divisions on two fixed straight lines.

From these two equations, Chasles would derive the description of two special kinds of conics, which he called “exceptional conics” (*coniques exceptionnelles*⁹⁵), or sometimes “special conics”. The first kind is a conic “represented by two straight lines”, which we shall sometimes call line-pairs, in keeping with the terminology used by Cayley, which is still in use today. The second kind is a conic “reduced to a straight line limited by two points”, and which Chasles calls a “conique infiniment aplatie” – we shall call this form a point-pair. How can one make sense of these degenerate conics through their “geometrical equation”? In other words, how to describe them in terms of intersections of homographic sheaves of rays, and of envelopes of homographic series of points⁹⁶?

In the *Traité des Sections Coniques*, Chasles provided a simple answer: a system composed of two straight lines can be viewed as a conic section because it satisfies the punctual geometrical equation of conics – as well as all other punctual properties of conics. For instance, Chasles explains, fixing two points on the first of these straight lines, one can form two sheaves of rays drawn from these two points, which intersect on the points of the other straight line. These sheaves generate the special conic made of two straight lines. Similarly, Chasles adds, one can easily show that Pascal’s mystical hexagram or Desargues’ involution are true for systems of two straight lines, therefore these systems can be viewed as conics. Tangential properties, however, cannot be extended to these special conics; as tangents to these curves are defined as all straight lines passing through the intersection of the two straight lines. The same holds for systems composed of two points, except that in this case tangential properties of conics are preserved.

This question of the geometrical description of special conics was actually discussed in British circles at about the same time, including by readers of Chasles. We briefly pause our discussion of Chasles to present a contemporary account by English geometer

⁹⁴ [Chasles, 1865b], pp.2-4. While the *Traité des Sections Coniques* was published in 1865, that is to say after the first communications on the theory of characteristics, it is very likely that the content of this book had been written prior to Chasles’ work on systems of conics. In fact, this *Traité* was first intended to be the first of two volumes, the second volume being precisely a book on systems of conics. This second volume was never written, perhaps due to the Vrain-Lucas affair which burst out in 1867, and took up much of Chasles’ time and effort.

⁹⁵ [Chasles, 1864a], pp.1173-1174; [Chasles, 1865b], pp.30-33.

⁹⁶ Classifications of degenerate conics have been fluid throughout history, and the one given here by Chasles and Hirst certainly does not agree with some which can be found in modern textbooks. For instance, if one wishes to classify conics *modulo* an affine coordinate transformation in \mathbb{C}^2 , one will have to distinguish between three kinds of degenerate conics, namely those given the equations $X^2 - Y^2 = 0$, $Y^2 - 1 = 0$, and $Y^2 = 0$; see [Brugallé, 2008], p.4.

Thomas Hirst of the presence of these degenerate conics, in the hope that it might capture something not entirely foreign to Chasles' own geometrical practice⁹⁷. According to Hirst, degenerate conics arise either from particular positions of the bases of the sheaves of rays or series of points (that is to say, the two fixed points or the two fixed straight lines), or from a particular kind of correspondence⁹⁸ between the elements of the sheaves.

Consider the first description of conics, as the intersection of two sheaves of rays $[p]$ and $[p']$ centered around two fixed points p and p' . In general, no two rays a, b in the first sheaf can have the same corresponding ray α in the other sheaf, since the correspondence between rays is $(1, 1)$. This is true, except in one "special case", Hirst explains. Suppose that, to each ray of the first sheaf, except for one ray a , corresponds the same ray α of the second sheaf. The ray corresponding to a , Hirst explains, is indeterminate. To each ray of the second sheaf, that is not α , there corresponds the ray a . The ray corresponding to α is also indeterminate. This correspondence, Hirst claims, generates a *line-pair*, formed by the two rays a and α . Indeed, each ray of $[p']$ intersects its correspondent on a , while each ray of $[p]$ intersects its correspondent on α . Furthermore, the point at the intersection of a and α is a double point of the degenerate conic, as it is both the intersection of a and its correspondent, and of α and its correspondent⁹⁹. Furthermore, the points p and p' in this conic do not play any special role. At any point of the conic that is not the intersection of a and α , the tangent to the conic is either a or α .

If now we construct this special correspondence so that a and α coincide (that is to say, that a and α both be the ray joining p and p'), then yet another kind of degenerate curve is generated. Now, the intersection of a and α is indeterminate; but now, p and p' play special role. Every ray of $[p]$ (except for a) intersects its correspondent at p , and every ray of $[p']$ (except for α) intersects its correspondent at p' ; thus, the conic is made of two points and one straight line joining them. This is the point-pair, or Chasles' *conique infiniment aplatie*¹⁰⁰. Note that, here as well, we can describe the tangents to this conic: in fact, every straight line passing through p or p' is a tangent to the conic. This conic can be viewed as a double line terminated by p and p' .

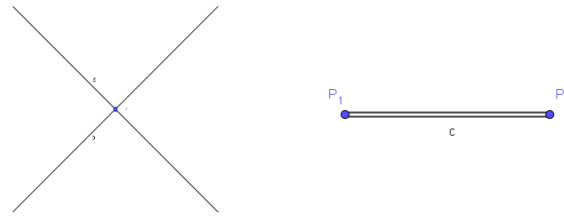
Whether or not Chasles had this exact construction in mind, the way he integrated

⁹⁷We shall come back to Hirst's broader engagement with Chasles' geometry in section 6.1.1.

⁹⁸ [Hirst, 1866], pp.167-168. Remember that two homographic sheaves of rays are simply two sheaves of rays in a $(1, 1)$ -correspondence, and that two homographic divisions are simply two series of points in a $(1, 1)$ -correspondence.

⁹⁹While these correspondents had been said to be indeterminate by Hirst, the intersections in question cannot be anywhere else, lest the curve we construct be something other than a conic. Hirst does not really justify this passage.

¹⁰⁰Hirst's paper then considers the third possibility of having both modes of degeneration at the same time, by having simultaneously a special correspondence and two coinciding centers. Chasles never discussed such degenerate conics, but they shall prove very important in the reception of the theory of characteristics. We shall come back to this issue in chapters 6 and 7.



Line-pairs and point-pairs

both kinds of degenerate conics in his proofs certainly corresponds to these descriptions – especially with respect to the definition of the tangents of degenerate conics constructed as loci¹⁰¹. The theory of characteristics, he would show, can integrate these curves; in fact, it can even enumerate them. Indeed, Chasles was able to show that, in a system (μ, ν) , there was always a finite number of each of these two kinds of degenerate conics¹⁰². More precisely, in a system, there are $2\mu - \nu$ point-pairs, and $2\nu - \mu$ line-pairs, for a total of $\mu + \nu$ degenerate conics.

In his initial series of communications, Chasles finds and justifies these numbers by counting the same number of conics in two different ways, one of which discounts exceptional conics. Remember from section 4.2.2 that, in a system (μ, ν) , there are $2(\mu + \nu)$ conics which touch another fixed, given conic, denoted U . This number can be derived directly from a certain proposition of Chasles’ list: this number was the order of the locus of the points whose polar line with respect to the conics of the system was also their polar line with respect to U . However, this number can also be derived from other propositions in Chasles’ lists, in a manner which attributes a special role to exceptional conics. Another proposition in Chasles’ lists states that¹⁰³ the common chords of U and the conics of a system (μ, ν) form the envelope of a curve of class 3μ . Since U is of class 2 (as a conic), U and this curve have 6μ common tangents (per the dual version of Bézout’s theorem). These tangents can be interpreted as infinitely small chords of a conic of the system, which means that there are seemingly 6μ conics in the system touching U . The difference between these two numbers of conics touching U , Chasles explains, is due solely to the existence of “exceptional conics”. The first value, namely $2(\mu + \nu)$, is taken by Chasles to be correct. The difference, $6\mu - 2(\mu + \nu) = 2(2\mu - \nu)$ is therefore the number of line-pairs conics, which always form with U two infinitely small chords. Thus, this difference can be interpreted as twice the number of line-pairs conics, wherefrom the result ensues. In fact, Chasles comments, this method for the obtention of the number of exceptional conics in a system (μ, ν) can be checked with the help of a variety of properties of such a system;

¹⁰¹Conversely, if we had constructed degenerate conics as envelopes, we would have had to discuss what their ‘points’ are.

¹⁰² [Chasles, 1864a], pp.1173-74.

¹⁰³ [Chasles, 1864b], p.301.

and not necessarily that relative to the number of conics satisfying a contact condition.

In a summary of the theory of characteristics written in 1866 by Eugène Prouhet for the *Nouvelles Annales*, which he then edited, another computation of these two numbers is given, which relies solely on the principle of correspondence¹⁰⁴. To count line-pairs conics in a system (μ, ν) , for instance, Prouhet takes a straight line L , and a point x on L . There are μ conics in the system passing through x , and these conics, in turn, intersect L at μ points u . Conversely, μ other points x correspond to a point u on L . Therefore, there are 2μ points x which coincide with a corresponding point u . This means that there are 2μ conics in the system which touch the line L , for a coinciding point for this correspondence is a point on L that the conic intersects twice. Per definition, there are ν such conics. The difference $2\mu - \nu$ is caused by the presence of line-pairs conics, which always cross L at two coinciding points, without actually being tangent to L . Therefore, Prouhet concludes, there are $2\mu - \nu$ line-pairs conics.

These proofs may not be entirely satisfactory to modern readers, for various reasons. For instance, in the first proof, how are we to know that the line-pairs are the sole source of error in the second computation of the number of conics touching a given conic? In the second proof, the seemingly *ad hoc* character of the correction of the number provided by the principle of correspondence, as well as the lack of explanation for the assertion that line-pairs conics are the sole source of error, are symptomatic of the difficulties that come as the cost of generality. Just like algebraists had to deal with the irruption of meaningless or artificial solutions in their computations, Chasles' principle of correspondence gives rise to numbers which must always be critically checked. Chasles mastered his principle, and made few mistakes: in later texts, he attempted to include this checking stage into the textual apparatus associated with these proofs by correspondence, as we will see below.

Chasles' discussion of exceptional curves was not limited to the case of conics. In 1867, whilst trying to generalize the theory of characteristics to curves of order m , Chasles had to make similar provisions to account for degenerate solutions to enumerative problems¹⁰⁵. A system of curves of order m is defined by $\frac{m(m+3)}{2} - 1$ conditions, and one can also define its characteristics μ, ν in the same manner as for conics. Furthermore, the principle of correspondence also applies to it – that is to say, one can also construct correspondences to investigate the properties of these systems. Chasles' remark is the following: it may appear, in the course of a proof relying on the principle of correspondence, that a solution, that is to say a curve of order m , be actually formed by a couple of curves of order k and $m - k$ (for some integer $k < m$). For instance, a curve-solution of the fourth order may

¹⁰⁴ [Prouhet, 1866], p.196. The article is signed 'P'; however, several authors, including Chasles' Danish student Hieronymus Zeuthen, refer to this text as being Chasles'. Prouhet attended some of Chasles' lectures in person, see [Israel, 2017], p.1345.

¹⁰⁵ [Chasles, 1867c], pp.800-805. We shall come back to this project in section 4.2.7.

actually be a pair of conics. It may even be that these two conics coincide, so that the curve of the fourth order is a double conic; which Chasles readily accepts as a valid curve of the fourth order in general. In the context of the theory of systems of curves of the fourth order, however, this seems to lead to a paradox, as a conic can only satisfy five of the thirteen conditions which define the system of curves of order 4. Chasles concludes from this paradox that¹⁰⁶:

Il faut donc qu'il y ait quelque autre élément qui représente, conjointement avec la conique, l'*être géométrique* qui satisfait aux dix conditions.

To explain what this “geometrical being” consists in, Chasles had to return to exceptional conics. For Chasles, a “conique infiniment aplatie”, or point-pair, is “represented” by two points and one straight line. This is not merely a description of the set of points composing the figure; otherwise, the two points would be superfluous, as they necessarily lie on the straight line. Instead, what this means is that, as a curve of order and class 2, this conic can be defined in terms of this straight line and these two points. Indeed, from any point P in the plane, one can draw two straight lines by joining P to each point of the point-pair; and these straight lines are the two tangents to the conic drawn from P . This means that the point-pair is defined as a curve of the second class. Moreover, any straight line intersects the point-pair in two infinitely near points, namely the intersection of the straight line and the axis joining the two points, provided this axis be understood as a double line. Chasles comments¹⁰⁷:

On peut dire encore qu'une conique représentée par deux points est un *être géométrique* formé d'une droite double représentant deux droites coïncidentes, et de deux points situés sur la droite, avec cette condition que toute droite menée par un des deux points sera considérée comme une tangente à cet *être géométrique*.

Similarly, Chasles argues, the double conic in a system of curves of the fourth order must be regarded as “composed of arcs, which represent, in some sense, infinitely flat crescents¹⁰⁸”, so that any line passing through these crescents' vertices is a tangent of the

¹⁰⁶“Consequently, there must be some other element which, jointly with the conic, represents the *geometrical being* which satisfies the ten conditions”, [Chasles, 1867c], p.801. Chasles mentions only ten conditions, because he previously had given an example wherein three conditions can be reduced to one. The argument stays the same in the case of thirteen independent conditions.

¹⁰⁷“In other words, a conic represented by two points is a *geometrical being* composed of a double line representing two coinciding lines, and of two points located on this line, with the condition that any line drawn from one of these two points be regarded as a tangent to this *geometrical being*”, [Chasles, 1867c], p.801.

¹⁰⁸“Une conique faisant partie d'un système de courbes du quatrième ordre doit être considérée comme composée d'arcs, qui représentent, en quelque sorte, des croissants infiniment aplatis dont les pointes seraient des sommets; de sorte que toute droite passant par un sommet serait une tangente”, [Chasles, 1867c], p.701.

double conic.

Exceptional curves had been left out of Chasles' and De Jonquières' investigations in the generation of curves via correspondences. In the context of enumerative problems, they rise out of necessity; as their presence among the solutions to such a problem cannot be avoided. To integrate them, however, requires new linguistic stipulations. What a curve consists in, what its elements (points, tangents) are; these questions required new answers within the language of correspondences and sheaves of homographic rays and points.

4.2.6 Multiple conditions and the extended notion of elementary system

We conclude this presentation of Chasles' 1864 theory of characteristics by introducing his treatment of what he called “multiple conditions¹⁰⁹” (*une condition multiple*), not least because it would prove particularly important in later reinterpretations of this theory¹¹⁰. Indeed, so far, we have only considered conditions as the geometrical equivalent to a single algebraic equation in the coefficients of the general conic. However, one can rather simply introduce conditions which do not fit this description, at least verbally: consider, for instance, the condition ‘to pass through two given points’. Of course, this condition can easily be decomposed into two ‘simple’ conditions; but this is not always the case. For instance, Chasles explains, a condition such as ‘to touch a given curve at a given point’, or ‘to have a double contact with a given curve’, are both examples of conditions of “multiplicity” two (*ordre de multiplicité*). And yet, these are classical conditions, treated by Bischoff and De Jonquières in their earlier papers; and, at this point, it is unclear how they can be incorporated within the methods described above. Furthermore, considering the epistemic norms with which Chasles measures the worth of a mathematical theory, it would not be enough to show that one can include these conditions: this inclusion has to be simple and systematic.

To do so, Chasles proposes two methods. In the first one, he extends the notion of elementary system, and of characteristics. For any three (simple) conditions Z, Z', Z'' (which, for the sake of brevity, Chasles abbreviates as $3Z$), one can form the following elementary systems¹¹¹ (in an extended sense):

$$\begin{aligned}(3Z, 1p.) &\equiv (\mu', \nu') \\ (3Z, 1d.) &\equiv (\mu'', \nu'')\end{aligned}$$

¹⁰⁹This notion is introduced in [Chasles, 1864e].

¹¹⁰See chapter 7.1 in particular.

¹¹¹ [Chasles, 1864e], p.345.

Note that $\mu'' = \nu'$, as both numbers represent how many conics satisfy $3Z$, pass through a given point, and touch a given straight line. The three remaining numbers, i.e. μ', ν', ν'' will suffice to characterize $3Z$. In particular, Chasles explains, every (indecomposable) double condition θ will be a function of these three numbers. For instance, given the double condition ‘having a double contact with a given conic U ’, denoted W , Chasles states that:

$$N(3Z, W) = \mu' + \nu'' - \frac{1}{2}\nu'$$

In other words, instead of having a system (μ, ν) , a (simple) condition Z , and a general expression of the form $\alpha\mu + \beta\nu$; we now have a system (μ', ν', ν'') and a (double) condition W (we shall see in a moment where this particular result comes from). Chasles did not specify whether he thought the number of conics in such a system satisfying ϑ would always be a linear combination of these characteristics; however, Cremona claimed that this was generally the case¹¹². For this reason, several authors would speak of “Cremona’s theorem” as the result for double conditions analogous to Chasles’ $\alpha\mu + \beta\nu$ formula.

Another method, which Chasles generally favours, is to directly complete multiple conditions with the right number of elementary conditions; and thus to form another kind of elementary system. For instance, considering the same double condition W , Chasles writes¹¹³ (without further justification):

$$\begin{aligned}(2p., W) &\equiv (4, 4) \\ (1p., 1d., W) &\equiv (4, 4) \\ (2d., W) &\equiv (4, 4)\end{aligned}$$

Considering two (simple) conditions Z, Z' of respective modules $\alpha\mu + \beta\nu$ and $\alpha'\mu + \beta'\nu$, it follows from the equations above that

$$\begin{aligned}(1p., Z, W) &\equiv [4(\alpha + \beta), 4(\alpha + \beta)] \\ (1d., Z, W) &\equiv [4(\alpha + \beta), 4(\alpha + \beta)]\end{aligned}$$

and therefore $(Z, Z', W) \equiv [4(\alpha\alpha' + \Sigma\alpha\beta' + \beta\beta'), 4(\alpha\alpha' + \Sigma\alpha\beta' + \beta\beta')]$.

In a sense, this is nothing more than an application of the method of substitution to a broader notion of elementary systems. Chasles draws other results from such computations. For instance, considering a third simple condition Z'' of module $\alpha''\mu + \beta''\nu$, and

¹¹² [Cremona, 1865], p.117.

¹¹³ [Chasles, 1864e], p.352.

denoting $3Z$ the condition composed of Z, Z', Z'' , he obtains:

$$N(3Z, W) = 4(\alpha\alpha'\alpha'' + \Sigma\alpha\alpha' + \beta'' + \Sigma\alpha\beta'\beta'' + \beta\beta'\beta'')$$

Comparing this expression to the general formulae obtained in section 4.2.3, which gave the number of conics satisfying 5 conditions in terms of their modules, Chasles shows that¹¹⁴:

$$N(3Z, W) = N(3Z, 2p.) + N(3Z, 2d.) - \frac{1}{2}N(3Z, 1p., 1d.)$$

which is precisely the module of the condition W expressed above as $N(3Z, W) = \mu' + \nu'' - \frac{1}{2}\nu'$.

It remains unclear how exactly Chasles computed the characteristics of the elementary systems given above. In the case of W , these computations are not out of reach; in general, however, it remains to be discussed how they can be carried out. Besides this (non-trivial) point, however, Chasles has suggested a way to integrate multiple conditions to his method in a general and systematic manner.

4.2.7 A research programme for the years to come

Between February 1864 and April 1867, Chasles read some 18 communications on the theory of characteristics before the Paris Académie des Sciences. In our presentation, we have left out several aspects of this theory, such as the extension of his theory to conics in (three-dimensional) space¹¹⁵, and the collaboration with Cayley and Zeuthen to extend the principle of correspondence to series of points on a unicursal curve instead of a straight line¹¹⁶. Among all these possible avenues for generalizations of this theory, Chasles had singled out two, which he thought were particularly deserving of attention.

The first one is the move from the study of systems of (plane) curves, to that of systems of (spatial) surfaces. In particular, in one of his communications, published in 1866, Chasles adapted the theory of characteristics to the theory of surfaces of the second degree¹¹⁷ (in what follows, abbreviated as SSD). In fact, in Chasles' scientific archives preserved at the Académie des Sciences, one can find the proof version of the introduction of what was supposed to be an entire memoir on the theory of systems of SSD¹¹⁸. Dozens of undated draft pages of this memoir can also be found in the same folder, indicating that the redaction of the manuscript was at an advanced stage. A SSD can be algebraically

¹¹⁴ [Chasles, 1864e], p.353.

¹¹⁵ [Chasles, 1865a].

¹¹⁶ A unicursal curve is a curve of genus 0. We shall come back to this question briefly in 6.1.

¹¹⁷ [Chasles, 1866d].

¹¹⁸ Archives de l'Académie des Sciences, Dossier Chasles, 35J/8. We provide a transcription of part of this memoir in Appendix A.

defined by the general equation

$$aX^2 + bY^2 + cZ^2 + dXY + eXZ + fYZ + hX + iY + jZ + 1 = 0$$

These surfaces are thus determined by nine conditions. A system of SSD is, consequently, determined by eight conditions. To each system, Chasles associated not two, but three characteristics: μ, ν, ρ are respectively the numbers of surfaces in a system which pass through a given point, touch a given line, and touch a given plane. Chasles claimed that these three numbers characterized every property of the systems of SSD, just as the two characteristics μ, ν characterized the properties of systems of conics. Consequently, the very same method of substitution can be carried out, provided the characteristics of elementary systems are known. But what exactly are elementary systems of SSD?

There are three elementary conditions surfaces can satisfy, as the number of characteristics indicates; and these are to pass through a point, to touch a line, or to touch a plane. Combining these conditions to form a system, that is to say to choose eight times amongst these three possibilities, yield 45 different elementary systems, that is to say $3 \times 45 = 135$ characteristics to compute. We noticed, in the case of systems of conics, that not all characteristics were necessary to compute, due to the duality of the systems, as well as the fact that some characteristics of different systems represent the same numbers. For instance, the first characteristic μ of the system $(3p., 1d.)$, is none other than the second characteristic ν of the system $(4p.)$. In the case of elementary systems of SSD, Chasles finds that only 55 characteristics actually need to be determined. However, things are not so simple, as there are some multiple conditions which cannot be decomposed, yet are elementary enough that they “constitute special and independent questions, and not particular cases, unlike in analytical Geometry where they may be considered as such¹¹⁹”. This would lead Chasles, in his 1866 paper, into intricate and lengthy enumerations of these multiple conditions, which in turn he organized via large tables of elementary systems (see fig. below)¹²⁰.

¹¹⁹“chasles1866b”, p.406.

¹²⁰In the manuscript found at the Académie des Sciences, all the possible cases are dealt with, and listed by dimensions, totalling more than a hundred elementary systems. Chasles’ attempt at a classification of curves, or systems of curves, does not stand out in the geometrical landscape of these decades. See [Lê & Paumier, 2016] for a discussion of some attempts at classifying cubic surfaces in the second half of the 19th century.

Caractéristiques des XVIII classes de systèmes.

I. $(\Sigma) = (1, 1, 1).$	XIII. $(G, K\theta, 2p.) = (1, 2, 2),$ $(G, K\theta, 1p., 1d.) = (2, 4, 4),$ $(G, K\theta, 2d.) = (4, 4, 4),$ $(G, K\theta, 1p., 1P) = (2, 4, 2),$ $(G, K\theta, 2P) = (2, 2, 1).$
II. $(\Sigma, \Sigma') = (1, 2, 2).$	XIV. $(\Sigma, 3p.) = (1, 2, 2),$ $(\Sigma, 2p., 1d.) = (2, 4, 4),$ $(\Sigma, 1p., 2d.) = (4, 8, 8),$ $(\Sigma, 3d.) = (8, 8, 8),$ $(\Sigma, 2p., 1P) = (2, 4, 4),$ $(\Sigma, 1p., 2P) = (4, 8, 8),$ $(\Sigma, 3P) = (8, 8, 8),$ $(\Sigma, 2d., 1P) = (8, 8, 8),$ $(\Sigma, 1d., 2P) = (8, 8, 8),$ $(\Sigma, 1p., 1d., 1P) = (4, 8, 8).$
III. $(\Sigma, G, G') = (1, 2, 2).$	
IV. $(\Sigma, K\theta) = (1, 2, 2).$	
V. $(\Sigma, G\theta K) = (1, 2, 2).$	
VI. $(G, G', K\theta) = (1, 2, 1).$	
VII. $(G, G', G', G') = (1, 2, 1).$	
VIII. $(\Sigma, G, 1p.) = (1, 2, 2),$ $(\Sigma, G, 1d.) = (2, 4, 4),$ $(\Sigma, G, 1P) = (2, 4, 4),$	
IX. $(G, G', G', 1p.) = (1, 2, 1),$ $(G, G', G', 1d.) = (2, 4, 2),$ $(G, G', G', 1P) = (1, 2, 1).$	XV. $(C, 3p.) = (8, 8, 8),$ $(C, 2p., 1d.) = (8, 8, 8),$ $(C, 1p., 2d.) = (8, 8, 8),$ $(C, 3d.) = (8, 8, 8),$ $(C, 2p., 1P) = (8, 8, 4),$ $(C, 1p., 2P) = (4, 4, 2),$ $(C, 3P) = (2, 2, 1),$ $(C, 2d., 1P) = (8, 8, 4),$ $(C, 1d., 2P) = (4, 4, 2),$ $(C, 1p., 1d., 1P) = (8, 8, 4).$
X. $(K\theta G, G', 2p.) = (1, 2, 2),$ $(K\theta G, G', 1p., 1d.) = (2, 4, 4),$ $(K\theta G, G', 2d.) = (4, 4, 4),$ $(K\theta G, G', 1p., 1P) = (2, 4, 2),$ $(K\theta G, G', 2P) = (2, 2, 1).$	XVI. $(K\theta G, 4p.,) = (1, 2, 3),$ $(K\theta G, 3p., 1d.) = (2, 4, 6),$ $(K\theta G, 2p., 2d.) = (4, 8, 12),$ $(K\theta G, 1p., 3d.) = (8, 12, 16),$ $(K\theta G, 4d.) = (12, 12, 12),$ $(K\theta G, 3p., 1P) = (3, 6, 5),$ $(K\theta G, 2p., 2P) = (5, 10, 5),$ $(K\theta G, 1p., 3P) = (5, 6, 3),$ $(K\theta G, 4P) = (3, 2, 1),$ $(K\theta G, 2p., 1d., 1P) = (6, 12, 10),$ $(K\theta G, 1p., 2d., 1P) = (12, 16, 12),$ $(K\theta G, 3d., 1P) = (16, 12, 8),$ $(K\theta G, 1p., 1d., 2P) = (10, 12, 6),$ $(K\theta G, 2d., 2P) = (12, 8, 4).$
XI. $(G, G', 2p.) = (1, 2, 1),$ $(G, G', 1p., 1d.) = (2, 4, 2),$ $(G, G', 2d.) = (4, 8, 4),$ $(G, G', 1p., 1P) = (1, 2, 1),$ $(G, G', 2P) = (1, 2, 1).$	
XII. $(K\theta, K'\theta, 2p.) = (1, 2, 2),$ $(K\theta, K'\theta, 2p., 1d.) = (2, 2, 2),$ $(K\theta, K'\theta, 2d.) = (2, 4, 2),$ $(K\theta, K'\theta, 1p., 1P) = (2, 2, 2),$ $(K\theta, K'\theta, 2P) = (2, 2, 1).$	

[Chasles, 1866d], p.408.

The second line of research pursued by Chasles is that of a theory of characteristics of curves of any order¹²¹. To that end, Chasles considered systems of (plane) curves of order m , determined by $\frac{m(m+3)}{2} - 1$ conditions¹²². To each of these systems, he associated the same two characteristics μ, ν , and once more asserted that the same procedure allows for the enumeration and construction of curves satisfying $\frac{m(m+3)}{2}$ conditions. Once more, the crux of the problem lies in the search of elementary systems, as there are a large number of elementary systems, as well as even more complex lists of multiple conditions. Even for cubics, that is to say curves of order three, Chasles would not be able to completely replicate the success of the theory of characteristics for conics – and there would seemingly be no equivalent to the $\alpha\mu + \beta\nu$ formula for these curves. However, from these two attempts, he drew a clear-cut research programme for the years to come, destined to himself as well as to his successors. Several of his students would indeed take up the study of systems of cubics: Zeuthen, Schubert, or the lesser-known Maillard all wrote papers and dissertations attempting to compute the characteristics of elementary systems

¹²¹ [Chasles, 1866c].

¹²²The general equation for such curves is $\sum_{i,j \leq m} a_{i,j} x^i y^j = 0$. The number of coefficients in this equation is $(m+1) + m + \dots + 1 = \frac{(m+1)(m+2)}{2}$. However, we can fix for instance $a_{0,0} = 1$, thereby reducing the number of independent coefficients to $\frac{(m+1)(m+2)}{2} - 1 = \frac{(m+1)(m+2)-2}{2} = \frac{m^2+3m+2-2}{2} = \frac{m(m+3)}{2}$, which is also the number of conditions required to determine a finite number of such curves. A system is defined by this number minus one conditions.

of surfaces or algebraic curves, using various new methods¹²³.

4.3 Towards a formal geometry

Between 1867 and 1870, Chasles' attention was mostly focused on the constant effort required by the now infamous Vrain-Lucas affair¹²⁴. According to Hirst, this even led to a deterioration of Chasles' lectures and teaching abilities¹²⁵. The redaction of a *Rapport sur les progrès de la géométrie*, upon request of the Ministry of Education, which was published in 1870, also impeded Chasles' scientific productivity, which Chasles himself lamented¹²⁶. For these reasons, he was never able to finish the second volume of his *Traité des sections coniques*¹²⁷. However, from the year 1870 onwards, Chasles was both free of the Vrain-Lucas affair and of the task of writing the *Rapport*. Over the next six years, he wrote a great number of papers, exclusively published in the C.R.A.S., with some summaries later published in Liouville's journal. These publications mark a departure from the theory of characteristics and the enumeration of curves, and instead focus on the principle of correspondence¹²⁸.

4.3.1 The otherness of Chasles' list-making practices

The principle of correspondence was the main, almost sole, focus of Chasles' research throughout the 1870s. Between 1871 and 1877, that is to say until his very last scientific papers, he wrote (mostly for the C.R.A.S.):

¹²³See section 6.2.

¹²⁴In 1867, Chasles announced that he had obtained letters from Pascal to Boyle, proving the former's priority with respect to the discovery of the law of universal gravitation. A public polemic ensued, first at the Paris Académie des Sciences, then across Western Europe. For a long period of time, Chasles would go almost weekly to the Académie to produce new explanations and new 'proofs' of the validity of his claims, in the face of mounting evidence that his letters were indeed forgeries. See [Alder, 2004] for more on this affair.

¹²⁵Letter from Hirst to Cremona, dated June 17th 1867, cited in [Israel, 2017], pp.933-934. It must be kept in mind, however, that Hirst was no neutral observer of this affair: he was one of the vocal opponents of Chasles' claims in the Vrain-Lucas affair, despite his admiration for the aging geometer. See [Higgit, 2003], and section 6.1.1.

¹²⁶[Chasles, 1870], p.269. On the context for the redaction and request of this report, see [Barbin *et al.*, 2009].

¹²⁷For instance, English mathematician Thomas Archer Hirst writes in his diary, on September 17th 1865: "The second volume of this work [Chasles' new work on conic sections] will contain a full exposition of his recent most important contribution to the theory of conics. He has found that the properties of a system of conics satisfying any four conditions whatever may be most naturally expressed in terms of two elements or characteristics.", Hirst Diaries, preserved at the Royal Institution in London, Journal XI, entry dated September 17th 1865, p.1741.

¹²⁸However, some of Chasles' results in these papers would later be used by Schubert in his 1879 book on enumerative geometry; see [Schubert, 1879b], for instance p.337, Lit.16.

- Two papers on systems of conics under orthogonality conditions¹²⁹, in April 1871
- Two papers on the principle of correspondence applied to the theory of curves of order¹³⁰ m and class n , between April and May 1871, followed by one more in 1872¹³¹, and one¹³² in 1873
- Five papers on harmonic axes¹³³, mostly between November 1871 and January 1872
- One paper on polygons inscribed or circumscribed to curves in April 1874, followed by a paper on similar triangles satisfying three conditions¹³⁴ in May 1874
- Two papers on the principle of correspondence itself¹³⁵, in April 1874 and May 1875
- Four papers on pairs of segments¹³⁶, between July and September 1876
- Three papers on triplets of segments¹³⁷, between September and November 1876
- Four papers on triangles¹³⁸, between January and May 1877

All of these papers mobilize the principle of correspondence in crucial manner: by this point, it has become Chasles' unique mode of demonstration.

These series of papers are remarkably similar in their structure. Most series begin with an introduction composed of historical remarks on the long-standing importance of the problem at hand, the assertion that the principle of correspondence yields an infinity of theorems regarding said problem in the easiest of ways, and result in very long lists of theorems demonstrating said easiness. Only one or two of these theorems are proven, for each paper; usually the first one, and sometimes the last one, so that Chasles may display the principle applied to more complex problems. The lists of theorems are almost always structured and divided into chapters or sections; the theorems themselves display a strong textual stability. Their wording is extremely consistent and monotonous, as if to show the systematicity of Chasles' method even in the writing of results.

It may be interesting to ponder the choices of topics in these papers: Chasles departs quickly from systems of conics to study curves in the most general fashion possible, without specifying their class or order. Soon enough, however, he even departs from the study

¹²⁹ [Chasles, 1871d], [Chasles, 1871c].

¹³⁰ [Chasles, 1871a], [Chasles, 1871b].

¹³¹ [Chasles, 1872a].

¹³² [Chasles, 1873].

¹³³ [Chasles, 1871e], [Chasles, 1871f], [Chasles, 1871g], [Chasles, 1871h], [Chasles, 1872b].

¹³⁴ [Chasles, 1874c], [Chasles, 1874b].

¹³⁵ [Chasles, 1874a], [Chasles, 1875].

¹³⁶ [Chasles, 1876c], [Chasles, 1876d], [Chasles, 1876b], [Chasles, 1876a].

¹³⁷ [Chasles, 1876e], [Chasles, 1876g], [Chasles, 1876f].

¹³⁸ [Chasles, 1876e], [Chasles, 1876g], [Chasles, 1876f].

of geometrical curves: polygons, then pairs or triplets of segments, and ultimately triangles of constant lengths, fall under the scope of the principle of correspondence. Through these series of papers, Chasles attempts to systematically expound the complete theory of a wide range of geometrical figures, in the most general and systematic way possible. Each series of papers is to represent such a theory: from the systematic application of a single principle, all properties of a certain figure flow effortlessly and systematically.

While we do not know the content of his actual communications, we can easily imagine, and hope, that they did not consist in long recitations of monotonous lists of theorems. No accounts of how readers actually engaged or understood these lists could be found; they were, however, seemingly very little read, and only rarely mentioned. It seems that Chasles' idiosyncratic list-making practice was not really *en prise* with what the new generation of geometers was interested in: as we will see in the second part of this dissertation, most readers of Chasles' theory of characteristics would identify key theorems to prove and important problems to solve in a totally different manner from what is on display here. Such lists, for that matter, would most likely be rejected by most contemporary mathematical journals: the obsolescence of their content notwithstanding, they almost seem to put the emphasis on the 'wrong' side of things, that is to say on the variety of small results one can obtain in these questions, rather than on the central results that allow for further research to be conducted. Thus, to delve into these lists is to experience what Robert Darnton, in his influential study on French cultural history in the 18th century, has called the "otherness" of past ways of thinking¹³⁹. By focusing on texts which seem radically alien to us, such as this tale, judged to be extremely hilarious by its authors, of a slaughter by some apprentices of their master's cats, Darnton highlighted the benefits to be reaped for the historian from local studies of such cultural practices, which shock and puzzle the modern readers. In a similar way, to overcome the initial surprise experienced at the sight of Chasles' lists yields insights into how this mid-nineteenth-century geometer understood mathematical activity and its goals.

¹³⁹ [Darnton, 1984], p.4.

132. L'axe harmonique de chaque point a de $U_{m'}$, relatif à U_m , touche sa courbe enveloppe en un point z ; l'axe harmonique de ce point, relatif à $U_{m'}$, coupe l'axe harmonique de a' , relatif à U_m , sur une courbe de l'ordre $m'(m-1) + 2(m_1-1)(2m'm-3m'-1)$.

CHAPITRE V.

» On considère les axes harmoniques de deux courbes $U_m, U_{m'}$, auxquels donnent lieu les éléments, points, tangentes, normales, cordes aa' d'une courbe $U_{m'}$ unicursale.

» 133. Les axes harmoniques de deux points a, a' de $U_{m'}$, relatifs, l'un à U_m et l'autre à $U_{m'}$, se coupent sur une courbe de l'ordre $m'(m+m_1-2)$.

» 134. La droite menée du point a au point d'intersection des deux axes harmoniques des points a et a' enveloppe une courbe de la classe $m'(m+m_1-1)$.

» 135. Les axes harmoniques des deux points a, a' , relatifs aux deux courbes $U_m, U_{m'}$, touchent leurs courbes enveloppes en deux points; la droite qui joint ces points enveloppe une courbe de la classe $2m'(m+m_1-2)-4$.

» 136. Le lieu d'un point dont les axes harmoniques, relatifs à deux courbes $U_m, U_{m'}$, passent respectivement par deux points a, a' de $U_{m'}$, est une courbe de l'ordre $m'(m+m_1-2)$.

» 137. Les axes harmoniques des deux points a, a' , relatifs à U_m et $U_{m'}$, respectivement, touchent leurs courbes enveloppes en deux points; les normales en ces points se coupent sur une courbe de l'ordre $3m'(m+m_1-2)-4$.

» 138. Les droites menées de chaque point a de $U_{m'}$ aux points dont les axes harmoniques relatifs à U_m et $U_{m'}$ passent par le point a' , enveloppent une courbe de la classe $m'(mm_1-1)$.

[Chasles, 1871h], p.1410

Indeed, these lists form a very clear case of a historical actor's attempt at crafting a literary technology suited for conveying knowledge in a manner which they saw as epistemologically valuable. At a fundamental level, this textual practice is used by Chasles to provide his readers with a tool to be used while solving specific problems, not unlike logarithm tables. They also serve, in the context of the theory of systems of curves, to run the general procedure described above. But this is not enough to explain Chasles' persistence in relying on such list-making practices, in contexts as diverse as the geometry of triangles, conics, or harmonic axes. More fundamentally, these lists serve to display the systematic and uniform generality of this mode of discovering and proving the whole of the geometrical theory of a given figure: the principle of correspondence has turned into a universal, epistemically-virtuous instrument, for geometers to use effortlessly. It may be useful, here, to recall Chasles' admiration of Pascal's mystical hexagram, as expressed in the *Aperçu Historique*¹⁴⁰:

Nous concevons parfaitement, d'après la fécondité éprouvée des théorèmes que nous venons de citer, que Pascal en ait fait, comme il l'annonçait, la base d'*Éléments coniques complets*; et qu'en les déduisant de son hexagramme mystique, il ait tiré de ce seul principe quatre cents corollaires, comme le dit le P. Mersenne.

Chasles' principle, through these many lists, has yielded a lot more than 400 corollaries; but it was not enough for the principle to be powerful. For geometry as an activity and a

¹⁴⁰“We understand perfectly well, from what we know of the fruitfulness of the theorems we just quoted, that, as Pascal announced it, he made them the basis of his Complete Elements on Conics, and that, by deducing them from his mystic hexagram, he derived from this single principle 400 corollaries, as Father Mersenne said”, [Chasles, 1837], p.73.

practice to be guided by the epistemic virtues that Chasles had identified already in 1837, even a new publishing method was required, and such was the purpose of these lists.

In Chasles' scientific archives, preserved by the Académie des Sciences, one finds many more documents which bear witness to Chasles' intense research activity on this very topic from 1864 until late into the 1870s. One specific activity, namely the generation of hundreds of similar propositions via the principle of correspondences, was of paramount importance for the theory of characteristics to be applicable in a wide range of cases. A plethora of brief, handwritten notes let us peek through Chasles' mathematical laboratory, and observe him hard at work, producing these lists of propositions.

4.3.2 A formal principle of correspondence

Alongside the uniformity of his lists, Chasles sought to make apparent the uniformity of their mode of production. The properties contained in these lists were all derived from the generalized principle of correspondence, which Chasles had constructed in the context of the theory of characteristics. While there was little change or improvement of the mathematical content (provided that such a notion is even meaningful) attached by Chasles to the principle of correspondence from 1864 onwards¹⁴¹, the textual apparatus attached to this principle changed considerably from 1874 onward. Like the lists described above, through the constant reworking of this textual apparatus, Chasles attempts to devise a way for geometrical practice, including its writing, to be as systematic and uniform as possible.

We have already discussed in detail the structure of Chasles' 1864 proofs by correspondence. Chasles' proofs up to 1873 would follow the same outline, with the addition of an extra step at the end of the proof to count and discard the “solutions étrangères”, that is to say coinciding points of the correspondence which do not come from a proper solution to the problem being solved¹⁴². A typical example of this is Chasles' proof by correspondence of Bézout's theorem, that is to say of the fact that two curves of order p and p' always have pp' intersection points¹⁴³. Chasles first fixes two points I and O (this is not really a departure from the proof-structure as described previously, as the point O does not play the same role as I , as will be made clear below), and considers a variable line IX . This line intersects the first curve at p points α , per definition of the order of a curve.

¹⁴¹The sole exception being the extension of this principle to correspondences on unicursal curves, that is to say curves whose genus is zero.

¹⁴²The term “solution étrangère” is not new: in fact, it occurs somewhat frequently in texts of analysis throughout the 1840s and 1850s to denote solutions provided by algebraic equations which are not applicable to the original problem. A typical example is Bertrand's *Traité élémentaire d'algèbre*, [Bertrand, 1850], p.88, where one of two roots of a quadratic equation cannot lead to a meaningful solution of an elementary geometrical problem.

¹⁴³ [Chasles, 1872a], p.737-738.

From the points α , one can draw p lines $O\alpha$, each of which intersects the second curve at p' points. This forms a total of pp' points of intersection of the second curve and the lines $O\alpha$. From these points, one can draw pp' lines passing through I , that is to say that to one line IX correspond pp' lines IU . To construct the reciprocal correspondence, Chasles forms the p' intersection points α' of a line IU and the second curve, and from these points are drawn p' lines passing through O . Each of these lines intersects the first curve at p points, totalling $p'p$ of intersections on the first curve, wherefrom can be drawn $p'p$ lines IX . Per the second lemma¹⁴⁴ regarding correspondences, there are $pp' + p'p = 2pp'$ coinciding lines for this correspondence. A coinciding line, as the construction shows, is one that passes through two coinciding points α and α' , that is to say an intersection point of the two curves. However, there are not $2pp'$ such points, for amongst the $2pp'$ coinciding lines, pp' are coinciding with the line IO . Since O is arbitrary, it does not have to be an intersection point of the two curves, and these solutions are discarded, leaving Chasles with pp' intersection points, that is to say Bézout's theorem. Chasles' never shows that the "solutions étrangères" he identifies are the only ones; nor does he really discuss why the solutions he discards are indeed to be rejected.

The few proofs given in the papers published between 1871 and 1873, however, are not as highly structured as they would become in 1874. Indeed, until this point, they remain mostly verbal, with the addition of these ambiguous symbols for series of points and lines that we have already described. In 1874, a new literary technology would be introduced without any explanation by Chasles, under the form of what we could call a 'correspondence tableau'. Here is a typical example of the new form that proofs by correspondence adopted at this period:

» Si autour d'un point O , multiple d'ordre ν , d'une courbe U_m , on fait tourner deux droites OA, OA' faisant toujours avec deux droites fixes OE, OF un même rapport anharmonique λ , les cordes aa' interceptées dans la courbe par ces deux droites enveloppent une courbe de la classe $2(m-1)(m-\nu)$, et lorsque $\lambda = -1$, la classe de la courbe est sous-double, c'est-à-dire $(m-1)(m-\nu)$.

» Démonstration. — Il s'agit de démontrer que $2(m-1)(m-\nu)$ cordes aa' passent par un point quelconque I . On pose immédiatement

$$\begin{array}{l} IX, \quad m \ a, \quad m(m-\nu) \ a', \quad IU, \\ IU, \quad m \ a', \quad m(m-\nu) \ a, \quad IX, \\ m(m-\nu) + m(m-\nu) = 2m(m-\nu). \end{array}$$

C'est-à-dire : Une droite IX menée par un point I rencontre la courbe U_m en m points a ; les m droites Oa donnent lieu à m droites OA' qui rencontrent la courbe en $m(m-\nu)$ points a' , par lesquels on mène $m(m-\nu)$ droites IU . Une droite IU menée arbitrairement rencontre U_m en m points a' qui donnent lieu de même à $m(m-\nu)$ points a et à $m(m-\nu)$ droites IX passant par ces points. Il y a donc $2m(m-\nu)$ coïncidences de IX et IU , et conséquemment $2m(m-\nu)$ cordes aa' passant par le point I .

[Chasles, 1874a], p.579

Let us describe precisely this textual device. First comes the statement of the theorem.

¹⁴⁴Instead of considering correspondences between two series of points on a line, one can consider correspondences between rays turning about a point.

Symbolic notations are used to represent every datum of the statement, to be used in the body of the proof: for instance, letters are chosen to represent fixed points or lines in the plane, to denote the orders of the curves at hand etc. Then Chasles moves on to the proof itself: first, he rephrases the *demonstrandum* into either a certain number of intersections between a locus and an arbitrary line L , or a certain number of tangents or chords to a certain locus, and passing through an arbitrary point I . To that end, Chasles always uses a sentence of the form “il s’agit de démontrer que..”. Then, Chasles produces a *tableau*, in which each line represents one side of the correspondence between either two series of points on L , or two series of lines drawn from I , depending on the nature of the previous reformulation. Below these two lines is written down the addition of the numbers of corresponding points or lines in each direction, that is to say the number of coinciding points and lines, per the principle of correspondence. Lastly, Chasles writes “c’est-à-dire”, and describes the correspondence that had been representend by the *tableau*. The tenses and the sentences used in the wording of the description of correspondences are always the same: present tense, moving from one line to other parts of the figure along the lines of a theoretical construction. This literary technology changes slightly in the following years. In 1875, for instance, Chasles reworks slightly the central *tableau* (see fig. below). Furthermore, when needed, Chasles adds an extra paragraph below the proof, in which the “solutions étrangères” are enumerated.

» I. *Le lieu d’un point x d’où l’on peut mener à deux courbes U' , U'' deux tangentes $x\theta$, $x\theta'$ faisant en longueur une somme constante ($x\theta + x\theta' = \lambda$), est une courbe de l’ordre $2(m'n'' + m''n' + n'n'')$.*

$$\begin{array}{ccc|c} x, & n'(2m'' + 2n'') & u & \\ u, & n''(2m' + 2n') & x & \end{array} \quad \left| \quad 2(m'n'' + m''n' + 2n'n''). \right.$$

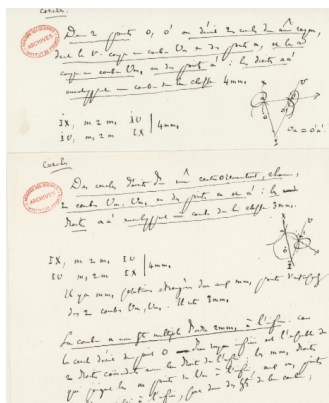
C’est-à-dire : D’un point x de L on mène à U' n' tangentes; chacune d’elles donne lieu à $(2m'' + 2n'')$ tangentes $\theta'u$ égales à $\lambda - x\theta$ (*), ce qui fait $n'(2m'' + 2n'')$ points u . De même, à un point u correspondent $n''(2m' + 2n')$ points x . Donc

$$2(m'n'' + m''n' + 2n'n'')$$

coïncidences de x et u .

» Il y a $2n'n''$ solutions étrangères dues au point x de L situé à l’infini, parce qu’alors, chaque tangente $x\theta$ étant infinie, il y a n'' tangentes de U'' passant par ce même point x , sur chacune desquelles on prend deux segments $\theta'u = \lambda - \theta x$, infinies, ayant donc chacune ses deux extrémités u coïncidant avec x , ce qui fait deux solutions étrangères; donc $2n''$ à raison des n'' tangentes de U'' , et $2n'n''$ à raison des n' tangentes $x\theta$ de U' . Il reste $2(m'n'' + m''n' + n'n'')$. Donc, etc.

In Chasles' scientific archives, there can be found hundreds of handwritten notes using this apparatus. Most of the times, the textual part of the proof is omitted, or replaced by a small diagram.



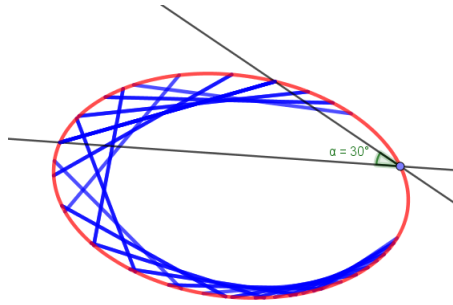
Archives of the Paris Académie des Sciences, Chasles 35J/4

Through this technology, Chasles aims to craft an instrument that will yield an infinity of results in each and every geometrical theory possible; such seems to be the intent behind his frenzy of publications in the 1870s. However, he also attempts to embody a certain ideal of generality as systematicity, which goes back to his very first writings on the history of Geometry, such as the *Aperçu Historique*. Recall how, in section 1.2.2, we described the “stability criterion” often employed by Chasles to measure the generality of a past method. If a proof or a method is such that one can substitute in it all occurrences of a figure by a more general one (or by another one of the same kind), and still have a valid proof for the new figure, then the method was deemed particularly general. In the 1874 paper on the principle of correspondence, in which the ‘correspondence tableaux’ are first introduced, Chasles mobilized once more this criterion, but this time to display the power of his own method. To display this stability of the proof (and of its textual materiality) under the operation of generalization, was key in the creation of the textual apparatus associated to the principle of correspondence.

Indeed, Chasles opened his communication by quoting a theorem of Poncelet, stated and proven in 1822, in the *Traité des propriétés projectives*. This theorem, in Chasles' paper, reads as follows¹⁴⁵:

Quand un angle de grandeur constante tourne autour de son sommet situé en un point d'une conique, la corde que ses côtés interceptent dans la courbe enveloppe une autre conique, laquelle se réduit à un point quand l'angle est droit.

¹⁴⁵“When an angle of constant magnitude turns about its summit, which lies on a point of a conic, the chords of the conic intercepted by the sides of the angle inside the curve form the envelope of another conic, which is reduced to a single point is the angle is right”, [Chasles, 1874a], p.579. See fig. below for a diagram representing Poncelet's proposition.



In blue, the chords formed by a constant angle ($\alpha = 30^\circ$) rotating about a point of a conic (in red). The chords envelope another conic.

It is not difficult to produce a proof by correspondence of this proposition. Let us consider a conic C in the plane, one point O on C , and a constant magnitude α for the angle viewed from C . Choosing arbitrarily one point I in the plane, any straight line IX intersects the conic in two points a . The two straight lines Oa give rise to 2 straight lines OA' , which form an angle of magnitude α with Oa . These two straight lines OA' intersect the conic at two points a' (in total), through which two straight lines IU can be drawn. Conversely, to a straight line IU , one associates 2 straight lines IX in the same manner. This means that there are 4 coinciding straight lines IX , two of which are “solutions étrangères”, which are the two tangents to C drawn from I . Therefore, from an arbitrary point I , one can draw $4 - 2 = 2$ straight lines which are tangent to the envelope described by the proposition; therefore, the curve described here is a conic.

This proposition, Chasles explains, can be generalized in many ways. Conics can be replaced by curves of order m ; angles between two lines by cross-ratios formed by these two lines and two other fixed lines; and the fixed point on the conic can be a point of multiplicity ν . The crucial property of the principle of correspondence, for Chasles, was to produce a proof(-text) in which each of these terms could be ‘translated’ by its more general counter-part, leaving the proof of the general theorem intact and valid. Indeed, the very proof given above can be stated for a more general proposition, provided one carries out the adequate substitutions. What’s more, Chasles comments, the general proof is no more difficult than the simple one¹⁴⁶.

The principle of correspondence, for Chasles, had become a universal and virtuous instrument in the geometer’s hands. Not only is it applicable in “an infinite variety of questions¹⁴⁷”, and of the utmost simplicity (as it involves nothing more than the addition of two numbers), but it also serves the effortless generalization of propositions, which Chasles had identified as one of two central goals of mathematical activity¹⁴⁸. Through a

¹⁴⁶In fact, the enumeration of the “solutions étrangères” somewhat puts the lie to Chasles’ claim, as it requires some more work than for conics.

¹⁴⁷ [Chasles, 1874a], p.578.

¹⁴⁸See section 1.2.5.

reflection on notations and textual devices, Chasles had systematized the very language of geometry, both for its propositions and its proofs. This device, however, was geared toward specific epistemic goals, which not everyone shared: what mattered to Chasles is less overarching structural theorems, but rather the effortless and methodical multiplication of particular truths.

“A proposition sometimes true, sometimes false”: the controversy between Chasles and De Jonquières

The similarities between Chasles’ theory of characteristics, and the theory of plane curves proposed by De Jonquières in his 1861 paper, are quite apparent. De Jonquières had introduced *series of curves*, which are collections of planar geometrical curves of degree n which all “share” $\frac{1}{2}n(n+3) - 1$ conditions, that is to say one less than the number of conditions that are required to completely determine one such curve. For a given *série*, he defined the “indice” (*index*) N as the number of curves in the *série* which pass through a given point. Chasles, on the other hand, built a theory of *systèmes* of plane conics, which satisfy four conditions. He then introduced two characteristics, μ and ν , respectively defined as the numbers of conics passing through a given point, and touching a given straight line. He later expanded these concepts to systems of curves of any order m : characteristics were defined in the same way, although Chasles pointed to greater difficulties in adapting his theory to these curves, for which the degenerate cases are more difficult to characterize.

Of course, the results at the heart of both theories are contradictory. In particular, one of De Jonquières’ theorem stated that in a series of *indice* N , there were $2(n-1)N$ curves that touched a given line L . In the case of conics, an observation at the center of the theory of characteristics stated that for any condition Z , and any system of characteristics (μ, ν) , the number of conics in the system satisfying Z was $\alpha\mu + \beta\nu$. It appears thus as if *series* and *systèmes* are the same thing, and $\mu = N$. Taking both theories to be true, one seems therefore entitled to write $\nu = 2(n-1)N$, wherefrom it follows that any condition Z is satisfied in a system (μ, ν) by $k\mu$ conics, where k solely depends on Z . Chasles’ theory would be greatly simplified, but also profoundly transformed. Where the two numbers

μ and ν were said to characterize systems of conics, playing the role of their equation, now μ entirely characterizes systems of conics, and the two numbers μ and n characterize systems of curves of order n .

Of course, Chasles thought the proof of the $\nu = 2(n - 1)N$ formula to be seriously flawed, and that the right-hand term only yielded a maximal value, and not an equality. This would be Chasles’ justification not to include, in his initial series of papers on the theory of characteristics, any acknowledgement or discussion of this possible debt toward his student, save for a brief footnote in the second of these communications¹. At any rate, by presenting his theory through a rapid succession of papers delivered at the Académie des Sciences, Chasles favoured a mode of publication that allowed for a quick and large-scale priority claim over his results. Nor did De Jonquières express issues with Chasles publishing a theory similar to his: in fact, between 1864 and 1866, De Jonquières even communicated to the Académie des Sciences texts which explicitly built on and acknowledged Chasles’ theory without mentioning his own past research on the same topic. There were reasons for that: De Jonquières thought his own results inexact, or not as absolute as those of Chasles; and he viewed himself as a disciple of Chasles, despite having only set foot in the latter’s lecture hall a handful of times at best.

However, actors removed from the Parisian circles certainly did not feel the same pressure to leave Chasles’ priority undisputed. Prior to Chasles’ initial communication on the theory of characteristics, Luigi Cremona had already privately communicated with De Jonquières in order to cast doubts on the validity of Chasles’ criticisms. As De Jonquières was increasingly convinced of the worth of papers he had by then already publicly disavowed, he also obtained the opportunity to go back to Paris, after several years at sea. The return to Paris, and to the Académie des Sciences, set off a priority dispute that had been looming large: as soon as he set foot within the walls of the Académie, a mere passing remark of De Jonquières’ was enough for Chasles to feel threatened, and an intense *querelle de priorité* started. Shortly thereafter, the controversy went beyond the confines of the Académie des Sciences. As it took new forms, new arguments were mobilized, and the priority quarrel turned into a full-fledged dispute over the validity and generality of some of De Jonquières’ and Chasles’ results. This chapter is devoted to this year-long dispute.

5.1 Situating the controversy

This dispute started, like many others, as a priority claim expressed during the weekly meetings of the Académie des Sciences. In this section, we set up the stakes of the

¹ [Chasles, 1864b], p.308.

debate, by discussing the similarities between both theories as perceived by both actors, the locations of the discussion, and the rhetorical postures of both parties toward the community of geometers which they sought to convince.

5.1.1 Scientific controversies as transformative episodes

For several decades now, controversies have been of major interest to historians of science at large². Simon Schaffer and Steve Shapin, in their widely influential *Leviathan and the Air-pump*, wrote for instance³:

Historical instances of controversy over natural phenomena or intellectual practices have two advantages, from our point of view. One is that they often involve disagreements over the reality of entities or propriety of practices whose existence or value are subsequently taken to be unproblematic or settled. [...] Another advantage afforded by studying controversy is that historical actors frequently play a role analogous to that of our pretend-stranger: in the course of controversy they attempt to deconstruct the taken-for-granted quality of their antagonists' preferred beliefs and practices, and they do this by trying to display the artifactual and conventional status of those beliefs and practices. Since this is the case, participants in controversy offer the historian resources for playing stranger.

The controversy between Chasles and De Jonquières certainly embodies many of these virtues for the historian that Shapin and Schaffer attributed to controversies. De Jonquières, in his struggle to publicly battle a much-respected and admired geometer, of whom everyone knew he had been a faithful student, sought to undermine several of Chasles' assumptions regarding the validity of some facets of his geometrical practice. As we shall see below, he attacked Chasles' belief in an analogy between the concepts of algebra and those of geometry, suggesting that these two sciences might have different forms of exactness and generality. Furthermore, this debate allows us to deconstruct historical actors' understanding of mathematical notions which were very much without explicit and complete definitions. Notable amongst these notions is the multiplicity of curves or points, which were usually dealt with by way of verbal expressions that are either meaningless or contradictory when taken at face-value. Chasles' textual apparatus for the setting up of correspondences often resorts to sentences whose meaning hinges

²In addition to the specific papers and books mentioned below, see the whole issue of *Science in Context* devoted to this topic, especially the introductory paper [Dascal, 1998], where it is argued that controversies are an essential component of the development of science.

³ [Shapin & Schaffer, 1985], p.7.

upon semantics whose rules remained largely implicit⁴: this is in particular the case when Chasles stipulates tangencies for degenerate curves, thereby introducing sentences such as ‘a conic made of one line and two points’. More generally, this dispute sheds light for instance on how De Jonquières made sense of Chasles’ proofs per correspondence, and of the enumeration of “solutions étrangères”.

There are limits, however, to this picture of controversies as revelatory episodes in the history of science. Indeed, there are controversies which lead to no productive discussion. Chasles, for one, did not produce any counter-epistemology to that of De Jonquières, nor did he really clarify the methodological or ontological underpinnings of his own geometrical theory. Rather, he repeated his belief that De Jonquières was wrong and misguided throughout the dispute, and used his advantageous social position to assert his authority and priority without engaging with his student’s progressive departure from their common research program. More pointedly yet, a limit to the image of controversies as revealing tacit knowledge and implicit practices lies in the idea that controversies are places where differences are constructed rather than spelled out. It seems difficult to assert that De Jonquières’ anti-Chaslesean understanding of the configuration of the algebraic and the geometrical was there all along, to be revealed by a clash of results, egos, and postures. This is a controversy between two geometers who seemed to share mathematical methods, scientific goals, and epistemological values, up until one formula tore them apart. To understand the stark divide which grew between them over the course of one year, one has to look at this controversy not as the clash of pre-existing incompatible epistemic practices or ideals, but also as an episode in which actors build novel epistemological distinctions to support key results or practices with which they cannot afford to depart in the face of criticism. Debating scientific knowledge and its modalities is always a transformative process for epistemic ideals and practices alike.

One specific epistemic ideal in particular would be put under heavy strain throughout the course of this controversy, namely the value of generality for geometrical propositions⁵. Defending his formulae, De Jonquières would first contest the absolute falsity

⁴A broader point in this direction, concerning the language of nineteenth-century geometry, has been made by the American philosopher Mark Wilson: “Insofar as surface syntax goes, the truth of the sentence ‘ L meets C in two points’ seems to require a semantical interpretation where the denotation of ‘point’ contains two objects common to L and C , some of which must lie infinitely near to one another. A mathematician of mid-century - George Salmon, say - would have happily acquiesced in this ‘apparent semantics’ for the language. But as geometrical experience subsequently enlarged, it became evident that unconstrained reasoning about infinitely near objects can lead to all sorts of muddles. But Bézout’s rule cannot be simply junked; it forms an integral part of some of the most important, and seemingly correct, geometrical results. Somehow the traditional, Bezout-based proofs find their ways to perfectly correct conclusions, despite the fact that their mediating steps seem false, insofar as they rely upon an untenable group of objects. The natural suggestion is that perhaps the ‘meaning’ of these intermediate statements ought to be reconsidered; they should be reparsed so that correct information is carried from premises to conclusion”, [Wilson, 1994], pp.522-526.

⁵Another scientific dispute between mathematicians who had two different conceptions of the value of

which Chasles attributed to them by pointing to the large amount of cases in which they were correct. This was unacceptable for Chasles, for whom the generality of a result was not something to be measured by its domain of validity. To strengthen his priority claim, De Jonquières had to renegotiate the value of generality, which he did by severing the bond between algebra and geometry. The analogy between these two sciences had underpinned Chasles' research for decades: while his methods were supposed to have certain epistemological advantages over analytical methods, they nonetheless captured several features of the algebraic science, and were modeled after it in many regards. De Jonquières, to defend the generality of his formulae in face of the many exceptions that Chasles had pointed to, would define a new generality for algebraic results which was incompatible with geometrical verifications. Algebra, as a pure language devoid of any imperfection, provides absolute certainties, which sometimes geometry seems to contradict in specific cases: this, for De Jonquières, is only so because geometry surreptitiously introduces impurities into the question at hand. At stake is the interplay of algebraic forms and geometrical figures, or, in other words, the question of the geometrical significance of results obtained by algebraic methods whose relation to figures is not constantly under check by intuitive grasp. As he sharpened his epistemological theses on the role of algebraic language within geometry, De Jonquières eventually criticized the very identity between μ and N , two numbers whose definitions are yet verbally identical. While the reform of language had thus far been tied to the quest for a means for expressing abstract and general truths, now generality was becoming dependent upon the very choice of a language.

This episode is not one where two incompatible epistemic ideals for geometrical practice collide. Rather, it is one where a central, inner tension of a geometrical practice long in the making was brought to the fore, and led to a consequential epistemological divide. As such, this controversy unfolded both on a social, a mathematical, and a philosophical plane, in a way that can't be comprehensively described by a watertight separation of these aspects⁶.

5.1.2 Cremona's defense of De Jonquières' formulae

To understand this dispute, we must first discuss the evolution of De Jonquières' opinion on the validity of his 1861 formula, which was mostly impacted by his correspondence

generality is that of Kronecker and Jordan in 1874, studied in [Brechenmacher, 2016b].

⁶“The conceptual, technical, normative, or epistemological arguments used to talk about mathematics cannot be accounted for by social factors alone: one has to take into account the relative autonomy of the mathematical field, which means that people cannot say ‘anything they want’ on mathematical topics at stake during a debate. Thus, the transversal nature of [controversies] leads to questions about the way in which mathematical theorems are validated, about what exactly makes a proof.”, [Ehrhardt, 2011], p.391.

with Luigi Cremona.

Cremona wrote two letters to De Jonquière on April 19th and September 20th 1863, as an attempt to save these supposedly faulty results and proofs⁷. This was important for Cremona, not least because he had used and reproduced these results in his 1862 *Introduzione*⁸. However, Cremona’s letters were only able to arrive in De Jonquière’s hands in January 1864. Indeed, the letters had been sent to Mexico, where De Jonquière was taking part in the Franco-Mexican war which started in 1861; but by the time the letters had crossed the Atlantic, De Jonquière had returned to France. Cremona had written to mention his recent publications on series of conics, in which he had obtained in new ways some of De Jonquière’s formulas⁹, and managed to explain why conics were an exception to the generality of these formulas.

Cremona, in fact, had come up with a different explanation for the apparent falsity of De Jonquière’s formula. Cremona thought that the lemma through which De Jonquière converted series into a general equation was false, but that it was not the main reason for the contradiction between the formulae and what was known about numbers of conics satisfying certain tangency conditions. Instead, in a brief 1863 paper, Cremona had attributed this contradiction (in the case of conics) to the presence of degenerate curves. For instance, De Jonquière’s *Théorème II* claims that in a series of conics of index N , there are $2N$ conics which touch a given straight line. Using slightly different notations, Cremona explained why this number was only a maximal value in certain series of conics¹⁰:

Il numero M' è in generale eguale a $2M$; ma può ricevere una riduzione quando dalle coniche risolventi il problema si vogliano separare i sistemi di rette sovrapposte, che in certi casi vi figurano. Questo non può evidentemente accadere se le coniche della serie denovo passare per quattro o per tre punti dati.[..]

Per determinare tale riduzione, ricordiamo che le coniche passanti per due punti dati e tangenti a due rette date formano una serie d’indice 4, nella

⁷From August 1863 to December 1864, Bischoff would write very regularly to Cremona as well, on the same problems as discussed here. It is unclear whether Bischoff also discussed this matter with De Jonquière directly, and if so, to what extent, [Israel, 2017], pp.155-170.

⁸ [Cremona, 1862], pp.63-70.

⁹ [Cremona, 1863a], [Cremona, 1863b].

¹⁰“The number M' is in general equal to $2M$; but it can receive a reduction if we want to separate the systems of superimposed straight lines. This obviously cannot happen if the conics of the series have to pass through four or three given points. [...] To determine the diminution [of the number provided by De Jonquière’s formulae], remember that the conics passing through two given points and tangent to two given lines form a series of index 4, in which, instead of eight, there are only four conics (effectively) tangent to a third line. If the line joining the given points meets the two given lines in a, b , the segment ab , viewed as a conic (whose dimension is null) tangent to the given lines in a, b , also manages to be tangent to any third line; and, as such, it represents four (coinciding) solutions to the problem: two of its points being given, describe a conic tangent to the two given lines and to a third line”, [Cremona, 1863b], pp.17-18.

quale, invece di otto, vi sono solamente quattro coniche (effettive) tangenti ad una terza retta. Se la retta che unisce i punti dati incontra le due rette date in a, b , il segmento ab , risguardalo come una conica (di cui una dimensione è nulla) tangente alle rette date in a, b , riesce tangente anche a qualsivoglia terza retta; e, come tale, rappresenta quattro soluzioni (coincidenti) del problema: descrivere pei due punti dati una conica tangente alle due rette date e ad una terza retta.

In other words, a certain kind of degenerate conics (namely, double lines) appear as conics which are always tangent to the given straight line. If one wants to discount them as true tangent conics, then a certain number, which at this point was not easily expressible, must be subtracted from $2N$. Furthermore, these degenerate conics do not appear in every series of conics: for instance, conics which ought to pass through four given points cannot be double-lines, as no straight line goes through four given points in general. While the formula was thus false in the case of conics, Cremona thought that it had a broader value. For general systems of curves of order m , these degenerate curves which falsify De Jonquière's results are not always present.

On January 23rd 1864, De Jonquière was finally able to reply to these remarks of Cremona. He first remained convinced of the fallacy of his methods and of his results¹¹:

Le mode de démonstration que j'avais cru pouvoir employer et que vous avez reproduit dans votre livre, n'est pas correct. Voici en quoi il consiste : «à un point \underline{x} , sur une droite, il correspond $M \cdot n$ points \underline{y} , et à chaque point \underline{y} , il correspond Nm points x . Donc les x et les y sont liées entre elles par une équation de la forme $A \cdot y^{Mn} \cdot x^{Nm} + etc.$, qui devient du degré $M \cdot n + N \cdot m$ en x seul, quand on fait $x = y$ ». Mais qui dit que l'équation ne devient pas $A \cdot y^{Mn} \cdot B \cdot x^{Nm} + etc.$, ou toute autre combinaison intermédiaire entre ces deux hypothèses également possibles ? Qui dit, en un mot, que la première équation

¹¹“The mode of proof which I thought I could use, and which you reproduced in your book, is not correct. Here is what it consists in: «to a point \underline{x} on a straight line, there correspond $M \cdot n$ points \underline{y} , and to a each point \underline{y} , there correspond $N \cdot m$ points x . Therefore the x and the y are linked to one another by an equation of the form $A \cdot y^{Mn} \cdot x^{Nm} + etc.$, which becomes an equation of degree $M \cdot n + N \cdot m$ in x only, once one sets $x = y$. »However, who's to say that the equation does not become $A \cdot y^{Mn} \cdot B \cdot x^{Nm} + etc.$, or any intermediary combination between these two hypotheses, which are all equally possible? Who's to say, in a word, that the first equation does not have several of its coefficients equal to zero, among those which contain the variables in the highest degrees? This mode of reasoning, which is appealing, and has often been used by many very estimable [peers] (which fooled me), is not rigorous; it is not even exact, or at least it only proves one thing, namely that the final free is in general and at most a certain number. It only indicates an upper limit, and not an absolute number”, [Israel, 2017], pp.973-975. In the edited correspondence, one reads “qui devient du degré $M \cdot n + N \cdot M$ en x ”, but this is most likely a mistake made in the transcription, which we have corrected here. Similarly, the second equation in x and y here seems to have been wrongly transcribed, as it should be one with a total degree strictly inferior to $Mn + Nm$.

n’a pas plusieurs coefficients nuls, parmi ceux qui contiennent les variables aux degrés les plus élevés ? Ce mode de raisonnement, qui est séduisant, et qui a été assez souvent employé par des autres forts estimables (ce qui m’a trompé), n’est pas rigoureux ; n’est même pas exact, ou du moins il ne prouve qu’une chose, c’est que le degré final est en général et au plus un certain nombre. Il indique donc une limite supérieure et non un nombre absolu.

In the rest of this letter, De Jonquières reasserted his belief that his analytical translation of the concept of series of conics as a rational equation $F(x, y) = 0$ was beyond doubts, a belief which Cremona obviously did not share.

Cremona replied immediately with another letter, sent to De Jonquières on January 29th 1864, containing more detailed responses to the problems that had been identified in this proof. While we do not know exactly what Cremona wrote, it is likely that De Jonquières understood something other than the intended meaning. De Jonquières, in his reply from February 9th, finally accepted Battaglini’s refutation of the analytical translation; and claimed that perhaps the fact that not all series have rational analytical representations could be the reason for the lack of generality of his formulas. De Jonquières still did not reconsider the validity of his previous results, which was the intent of Cremona’s letter: in fact, De Jonquières even recounts having shared Cremona’s arguments with Chasles, who was not impressed with them. This defense of the $2(n-1)N$ formula cannot reside in the question of the analytical representation of series: as Cremona had already noted by this point, the problem lay in the multiple counting of degenerate conics.

By this point, Chasles had started to publish his theory of characteristics, and Cremona immediately reacted to it. From April 1864 onward, Cremona would send formulas, proofs, and even corrections of some of Chasles’ results on a regular basis, to be published in the *Comptes-Rendus de l’Académie des Sciences*. All these texts used Chasles’ notations and methods faithfully. And yet, at the same time, Cremona kept arguing for the rehabilitation of De Jonquières’ earlier results in private. Indeed, in a letter sent on April 6th 1864, De Jonquières even asked Cremona to give up on his formulas¹²:

J’admire votre dévouement à ma formule $\nu = 2(m-1)\mu$, et le courage que vous déployez pour une mauvaise cause. Il n’y a pas de relation générale, dépendante du degré des courbes de la série, entre les deux caractéristiques μ et ν ; cette relation varie dans chaque cas. Or que signifie une règle qui ne s’applique jamais sans exception ? Abandonnez moi donc, en me laissant, si vous voulez, l’honneur d’avoir le premier songé à caractériser géométriquement les systèmes de courbes. Mon indice N , qui est la caractéristique μ de M .

¹² [Israel, 2017], pp.977-978.

Chasles, était bon, et il reste. Mais j'ai eu tort de croire que l'autre caractéristique en était une conséquence exprimable par une formule générale, et ç'a été un trait de lumière que d'introduire la seconde caractéristique ν , sans aucune subordination à la première. Et, de fait, pourquoi le nombre des courbes qui passent par un point jouerait-il un rôle exclusif, aux dépens du nombre des courbes qui touchent une droite ? La loi de dualité attribue une importance égale aux deux nombres, et il ne fallait qu'un peu de philosophie pour y songer ; comme pour l'œuf de Christophe Colomb.

This was a busy period for De Jonquières, whose scientific output between 1861 and 1865 would be at a low ebb. Having gone back to France for a few months only, De Jonquières was sent in May to Saïgon, where he would act as “chef d'état-major général” (*Chief of Staff of the Armies*) for two years¹³. Far from Paris, he was still able to receive news from the mathematical communities of Europe: Battaglini, for instance, sent him every issue of the *Giornale di Matematiche* as soon as it was printed¹⁴. In the second half of the year 1865, De Jonquières was able to devote time once more to his mathematical studies, and he would send three notes to three European journals¹⁵. The first one, written on July 16th for the *Comptes-Rendus de l'Académie des Sciences*, and communicated by Chasles and Liouville, dealt with systems of surfaces of arbitrary order, and was very much in the lines of what Chasles had done until then. In the other two papers, however, De Jonquières went back to his 1861 formulas with a new twist: he now found them to be perfectly exact. Around the same time, on November 30th, De Jonquières sent a letter to Cremona containing a similar reversal of his past doubts¹⁶:

Les sincères reproches que je me faisais “de vous avoir induit en erreur dans votre introduction à la théorie des courbes au sujet des séries à indices” sont sans fondement. Mon mémoire d'avril 1861 (Journal de Liouville) n'a pas les torts que je croyais, et les scrupules que j'avais admis (faute de temps pour en examiner les causes) au moment où j'allais quitter Paris pour venir en Cochinchine, tombent devant l'examen plus sérieux de la question, que j'ai eu tout récemment le loisir de faire. Ainsi tout ce que vous avez, dans l'ouvrage dont je parle, emprunté à mon mémoire est exact. Il est parfaitement vrai que les séries ou systèmes de courbes peuvent, si leur degré commun est indiqué, être définis par un seul indice ou caractéristique, et que la seconde caractéristique est toujours une même fonction simple de la première. Ainsi

¹³ [de Jonquières, 1883], p.14.

¹⁴ [Israel, 2017], p.980.

¹⁵ [de Jonquières, 1865b], [de Jonquières, 1865a], [de Jonquières, 1865c].

¹⁶ [Israel, 2017], pp.982-984.

μ (ou ce que j'appelais l'indice N) exprimant le nombre des courbes du système (que j'appelais la série) qui passent par un point quelconque du plan, on a toujours (je veux dire généralement) $\nu = 2(n - 1)\mu$, n étant le degré commun des courbes de la série, et ν exprimant le nombre des courbes qui touchent une droite quelconque. Mon seul tort (que je confesse humblement) a été d'attribuer à des raisons de polarité réciproque l'exception apparente que présentent les coniques, exception qui ne se rencontre pas dans les courbes des autres degrés, et n'existent dans celles du deuxième qu'à cause des systèmes de deux droites et des coniques inf^t aplaties qui se rencontrent parmi celles-ci. Il s'ensuit que mes autres théorèmes sont pareillement exacts [...].

Ceci ne veut pas dire qu'il n'ait été fort ingénieux, fort utile même, d'introduire dans la géométrie la notion simultanée de la deuxième caractéristique ν ; car, en donnant plus de symétrie et de simplicité aux formules, elle a permis d'aborder aisément un plus grand nombre de problèmes (surtout dans la théorie des coniques). Mais il demeura acquis qu'il n'y a pas une erreur [...]. Car soit h une droite quelconque ; par un point x de cette droite il passe μ courbes du degré n ; donc il correspond à ce point $\mu(n - 1)$ points y ; et réciproquement à chacun de ces points y , il correspond $\mu(n - 1)$ points x . Donc les points x et y sont liés par une équation dont le terme de l'ordre le plus élevé est de la forme $Ax^{\mu(n-1)}y^{\mu(n-1)}$, dans laquelle le terme A n'est pas nul généralement. Si l'on fait, dans cette équation, $x = y$ l'équation $Ax^{2\mu(n-1)} + ..$ fait connaître le nombre des courbes tangentes, dont le nombre est par conséquent $2(n - 1)\mu$. Donc ... Ce raisonnement est inattaquable.

De Jonquières' analysis of his 1861 paper in his letter to Cremona differs from that presented in his two notes from Saïgon. In his third note, sent to Battaglini's *Giornale*, De Jonquières wrote¹⁷:

Ces formules sont exactes, analytiquement parlant ; cependant leur démonstration reposait sur un *lemme*, qui a excité quelques doutes. En outre, plusieurs théorèmes, quand on les appliquait aux sections coniques, offraient d'apparentes anomalies dont je ne sus pas rendre compte ; ce qui était de nature à entretenir l'incertitude.

The lemma mentioned here is none other than the analytical representation of series of curves, which Battaglini himself had criticized (as well as Cayley). De Jonquières, when writing to Battaglini's and Liouville's journals, located the problem of his past results

¹⁷ [de Jonquières, 1865c], p.45. The exact same paragraph can be found in [de Jonquières, 1865a], p.412.

with this very lemma, and then claimed to give new, valid proofs of his past results. These proofs, however, are similar to the one sent to Cremona: it is a mere correction of the 1861 proof, with changes having little to do with the faulty lemma. Furthermore, De Jonquières had in fact noticed the anomalies of the case of conics: we discussed in the previous chapter his own attempt at explaining them.

By the end of the year 1865, De Jonquières had set aside the problem of his lemma on the analytical representation of series. The geometrical definition came first: if, for certain geometrical conditions, eliminating the equations of conditions could not result in a rational equation $F(x, y) = 0$, then, De Jonquières claimed, it was not a series of curves in the proper sense¹⁸. Doing away with this definitional problem, and fixing what he thought was wrong in his initial proofs, De Jonquières concluded that his 1861 formulas were true all along.

In October 1866, De Jonquières even sent a memoir for publication in *Crelle's Journal*¹⁹, in which he gave general formulas for the number of curves satisfying contact conditions, as well as a method for the computation of characteristics of elementary systems of curves and surfaces, all of which were based on the results which Chasles had rejected. And yet, Chasles' theory of characteristics was, for De Jonquières, equally true: in fact, it was a mere simplification of his own theory of curves – it had symmetrical expressions, and perhaps an easier method for the computation of numbers of curves, but it was no more general than De Jonquières' own theory. Thus, through his correspondence with Cremona, and then Battaglini, De Jonquières had entirely reversed his opinion regarding the validity of his formulae. Through the controversy with Chasles, he would also put forth new epistemological theses to accommodate the validity of his formulae with the apparent counter-examples it received, especially in the case of conics.

5.1.3 Scientific communication at the Académie des Sciences

Chasles did not publicly react to these papers, and kept on with his own research as if nothing had happened. This status quo would be shattered in 1866, as De Jonquières returned to France from Saïgon and attended some of the weekly meetings of the Académie des Sciences during which Chasles frequently presented updates on his research on systems of curves and surfaces. While De Jonquières had published papers which contradicted Chasles' priority claim, albeit implicitly, this was no sufficient cause for public controversy. The same iconoclast theses, once uttered at the Académie des Sciences, had a completely

¹⁸“[M. Battaglini] fait voir comment, étant données les équations de condition, on peut arriver à l'équation finale, dans laquelle le paramètre arbitraire ne figurera pas, en général, sous forme rationnelle. Fort bien ; mais alors cette équation ne représentera pas une série telle que je la définis d'abord 'géométriquement'”, [Israel, 2017], p.985.

¹⁹ [de Jonquières, 1866a].

different ring to them. To understand why, it is crucial to understand the role and status of the weekly meetings of the Académie, as well as the journal attached to them. In other words, the controversy between Chasles and De Jonquières was not merely a clash of technical ideas: it mattered greatly where, when, and how contradictory opinions were expressed. As Caroline Ehrhardt wrote in a discussion of a similar polemic which started at the Académie des Sciences and ended up in general newspapers, “historicizing a scientific controversy also means historicizing the very nature of controversy and the role science could play within the public space²⁰”.

To do so, it is crucial to understand what kind of outlet for scientific communication the *Comptes-Rendus de l'Académie des Sciences* were. In particular, it matters to this controversy to understand what the modalities of their publishing were, what social and scientific purposes they served, and why a certain class of scientists would select them as their main venue to publish results.

Indeed, on his theory of characteristics, Chasles never published outside of the *Comptes-Rendus de l'Académie des Sciences* - with the notable exception of his 1870 *Rapport sur les progrès de la géométrie*, which features a brief section on this subject²¹. More generally, since becoming a member of the Académie in 1851, Chasles had been constantly favoring the *Comptes-Rendus* as an outlet to publish new results²²; in that regard he was no exception amongst Académiciens²³. Between February 1864 and April 1867, Chasles gave 16 ‘communications’ in front of the Paris Académie, all of which were subsequently published. Furthermore, he communicated letters on this topic which he had received from foreign correspondents, and answered to communications of other geometers on the same topic (including De Jonquières, Cremona and Cayley). The *Comptes-Rendus* were therefore a central outlet for the circulation of the theory of characteristics, and of enumerative problems at large – but one over which Chasles exerted near-total control with respect to papers pertaining to geometry. It is no surprise that out of De Jonquières’ three notes from Saïgon, the only one which does not purport to rehabilitate the 1861 formulas is the one sent to the Académie des Sciences. As Chasles wrote exclusively for the *Comptes-Rendus*, most other European mathematical journals had to resort to other authors publishing their own summaries of Chasles’ theory of characteristics²⁴. As a result,

²⁰ [Ehrhardt, 2011], p.392.

²¹ [Chasles, 1870], pp.256-266.

²² Between 1851 and Chasles’ death in 1880, he published only 8 times in *Liouville’s Journal*. All of these publications are, in fact, summaries or copies of communications previously given at the Académie. Similarly, Chasles’ 9 publications in the *Nouvelles Annales de Mathématiques* are all abridged copies of communications given but a few months prior at the Académie, except for one lengthier memoir, which is a reprint of an earlier work of Chasles’, completed in 1829. While no complete survey of all of Chasles’ publications exists as of yet, he does not seem to have ever published in foreign journals either.

²³ [Crosland, 1992], p.295.

²⁴ For instance, [Cremona, 1864], [Prouhet, 1866], [Schubert, 1870]. We shall come back to the issue of

the theory of characteristics was published under a highly specific and constrained form, which is not that of the typical memoirs a scientist would circulate through other outlets outside the Académie des Sciences, or the form of the lengthy, detailed and carefully constructed books, treatises and reports which Chasles otherwise wrote.

Communications at the Académie des Sciences could not be arbitrarily long, and not just every communication could be printed²⁵. On June 23rd 1862, a new set of rules was voted, which fix the maximum length of a printed communication by a member (8 pages) as well as the annual maximum total length of all printed communications by the same member (50 pages). During the year 1864, Chasles gave 8 communications which were later printed, totalling about 70 pages. The second of these communications²⁶ even includes a footnote explaining that Chasles had obtained a special authorisation from the Académie to print a note that was considerably longer than the rule permitted (12 pages - still three pages shorter than the last communication given during the same year²⁷, for which no such authorisation is mentioned). While Chasles' prestigious status among Académiciens may have let him be granted the freedom to publish more than these rules state, it is likely that his flurry of communications stopped toward the end of August for these technical reasons, and not because he had presented the full extent of his research.

Chasles' published communications built on one another: it would have been difficult to make sense of the later texts for readers who hadn't obtained the previous ones²⁸. Chasles' rhetorical and argumentative strategy in these papers is worth dwelling on. His initial communication acted as a general announcement: Chasles opened by stating that the question of determining the number of conics which touch 5 other given curves "had preoccupied geometers for a long time²⁹". He then surveyed the results obtained thus far which fell under this general question, and Steiner's results, which he asserted were wrong. He then announced rather dramatically that he had solved the general problem, and gave a list of formulas (most of which were redundant) for the numbers of conics touching 5 given curves of orders m_i . While no proof or justification for these results was given in this initial paper, Chasles teased his audience by announcing that "the considerations which led [him] to the previous results can be applied to a great number of other questions", and that "these questions give rise to a rather large theory³⁰", which

the European reception of Chasles' theory of characteristics in section 6.1.

²⁵For a description of the Académie des Sciences as a scientific institution, and of the evolution of its publishing practices, [Crosland, 1992], pp.279-299.

²⁶ [Chasles, 1864b].

²⁷ [Chasles, 1864e].

²⁸In fact, one journalist writing for the *Journal des Débats* explicitly lamented the fact that communications such as Chasles' were impossible to follow for those who did not belong to the mathematical section of the Académie, see [de Rémusat, 1866], p.1.

²⁹ [Chasles, 1864c], p.222.

³⁰ [Chasles, 1864c], pp.225-226.

he would make public in later communications. The second communication introduced the main concepts and the central algorithm upon which the theory of characteristics is founded, and treated one example in detail. The rest of the theory of characteristics was then published across 16 other short papers, each of which dealt with one or more specific aspects of said theory. For instance, some papers focused on a specific kind of conditions (angular conditions, multiples conditions), while others sketch avenues for generalizations. None of these papers went in-depth into a specific subject; instead, these publications were aimed toward the quick announcement of a wide range of discoveries – as well as toward the presentation of a research programme for others to take up.

For this reason, Chasles’ theory of characteristics would remain an unfinished symphony. The very format and goal of publications in the *Comptes-Rendus* was, for Chasles as for others, to allow for an almost instantaneous announcement of novel results, methods and breakthroughs. In a *milieu* which valued creativity and novelty above all else, to which testify the many priority disputes for which the Académie provided a stage in these decades, and to which Chasles himself was prone more often than not, the *Comptes-Rendus* served for rapid communication first and foremost. That they could not provide enough space for in-depth discussions of scientific matters was even a point that was regularly made by critics of these weekly publications³¹. Indeed, these communications were printed on an extremely tight schedule, under high pressure from copy-editors who had to typeset redacted versions of the oral communications, run proof versions by the authors, and then print the corrected version, all within a week³². The contrast is stark with the sort of minutious polishing that Chasles was able to do on his book, to which testify the minute corrections on the proof version of the introduction of his unpublished volume on second-degree surfaces, preserved at the Académie des Sciences. The rhetorical strategy elected by Chasles in the ordering and crafting of his papers makes sense in this context. He first announces new results, stamps his mark on a field of research by carefully delineating the range of his discovery. Second, he demonstrates the efficiency and generality of his method on a difficult example. At this point only is he free to publish the full extent of his results, but in a rather contrived form: lists of results without proof, sorted out in papers, each of which covers one specific subproblem. The last papers of the series then lay out a research programme for others to build on his work, whilst still having to acknowledge his priority and crucial role in the development of this theory. This does not match the rhetorical strategy and mathematical architecture displayed in Chasles’ books. Compare the level of details given for instance in the 1852 *Traité*, in which every single property of the cross-ratio is painfully detailed, to the speed at which Chasles introduces

³¹ [Crosland, 1992], p.283.

³² [Crosland, 1992], pp.290-291.

his generalized principle of correspondence and uses it in many intricate ways, which do not necessarily match the brief, introductory account he had just given. As we saw in the previous chapter, some of Chasles' propositions were, by themselves, hard to read, let alone to prove. To a reader not trained in Chasles' writing of geometrical equation, as described, some of Chasles' papers were simply meaningless, or at best uninteresting.

At the Académie des Sciences, Chasles occupied a position of prestige and authority. His communications served to announce to the rest of the mathematical world that he had first solved some important and widely discussed problems, that he was in possession of a powerful theory for everyone to admire and expand upon, and to communicate a carefully outlined research programme for others to take on. Furthermore, he did all of this through the course of public communications, open to all. The weekly meetings of the Académie were regularly attended by journalists and scientists alike. Under the influence of Arago, science at the Académie had become something of a spectacle, in which were to be made public technological innovations, polemics between savants, and knowledge for all³³. Of course, the content of these sometimes harsh disputes was turned into milder, polite exchanges in the *Comptes-Rendus*³⁴.

In such a context, Chasles had been notoriously prompt to react to any perceived slight to his priority claims, or to his historical theses. His controversy with Libri, which we discussed in section 2.1.2, mostly happened within the walls of the Institut de France. When De Jonquières came back from Saïgon and sat in these same rooms, a mere passing remark was enough to trigger a salvo of hostile remarks from his former professor and friend.

5.1.4 A priority dispute at the Académie

On November 5th 1866, De Jonquières presented to the Académie des Sciences recent research, seemingly in line with the broader research program outlined by Chasles. In his communication, De Jonquières recalled the definitions of the characteristics for systems of curves or surfaces of any order, as well as the importance of determining the characteristics of “elementary systems”, that is to say systems formed by conditions such as passing through a point or touching a line. De Jonquières went on to present a way to easily compute the characteristics of elementary systems of curves of order m in the case of systems determined by a sufficiently large number of points³⁵. A footnote to the first sentence of the version of this paper printed in the *Comptes-Rendus* would introduce for the first time in the Académie traces of De Jonquières' dissent³⁶:

³³ [Belhoste, 2006], [Fox, 2012], pp.189-191.

³⁴ [Appel, 1987], pp.143-201, cited in [Ehrhardt, 2011], p.390.

³⁵ We reserve the discussion of the technical details for the second half of this chapter.

³⁶ [de Jonquières, 1866d], p.793.

La caractéristique μ , dont la seule notion permettait déjà d’aborder, par les procédés de la Géométrie pure, l’étude des propriétés des familles de courbes et de surfaces assujetties à des conditions communes, sans que ces conditions fussent exprimées explicitement, a été introduite pour la première fois dans la science par un Mémoire que nous avons publié au mois d’avril 1861 dans le *Journal de Liouville* ; qu’on nous permette de le rappeler ici.

Not only did De Jonquières refer to a memoir which Chasles had condemned, he also used the equation $\nu = 2(m - 1)\mu$ in his new method, assuming its validity. He did not go as far as to publicly claim before the Académie that Chasles’ theory of characteristics was a mere simplification of his own, as he had done in private letters. However, toward the end of his communication, De Jonquières made a series of general remarks regarding the very research program outlined by Chasles. In particular, he wrote³⁷:

Mais on se tromperait si, par ce seul motif qu’il reste beaucoup à faire, on supposait que la solution complète de la question n’a pas fait un pas, et si l’on croyait que la connaissance des caractéristiques de *tous* les systèmes élémentaires peut seule constituer un progrès dans cette partie de la Géométrie.

On the contrary, De Jonquières continued, important and difficult results could be obtained by completely other means. He justified this assertion by citing Bischoff’s 1858 papers on the enumeration of curves satisfying contact conditions, whose formulae De Jonquières had found by different means in 1861. This was an even more pointed attack against Chasles’ program, all the more because it echoed a communication made by Chasles in the very same room, on the very same topic, and only a couple of weeks prior.

Indeed, on October 22nd, Chasles had presented a paper written by Cayley for the Académie, after which he had offered some remarks of his own³⁸. Discussing the difficulty of reproducing in the theory of curves of order m the results he had achieved in the theory of conics, Chasles wrote:

Lorsqu’il s’agit, comme ci-dessus, de courbes C_p d’ordre quelconque, au lieu de coniques, il peut y avoir aussi des solutions étrangères; mais il y a une autre difficulté, ou plutôt une impossibilité presque générale, dans l’état actuel de la théorie des Courbes; c’est qu’il faudrait connaître le nombre des courbes d’un même ordre, déterminées par les conditions élémentaires de passer par des points et de toucher des droites; en d’autres termes, il faudrait connaître les *caractéristiques* des systèmes élémentaires des courbes de l’ordre donné car

³⁷ [de Jonquières, 1866d], p.796.

³⁸ [Cayley, 1866b]. Chasles’ remarks can be found pp.670-673.

ce sont ces caractéristiques élémentaires qui feraient connaître celles d'un système défini par des conditions données. La recherche des caractéristiques des systèmes élémentaires de courbes d'ordre supérieur est donc une des questions les plus importantes et qui méritent le plus de fixer l'attention des géomètres.

In fact, the very last sentence of De Jonquières' communication, namely "qu'on nous permette de le rappeler ici", was an almost exact echo of Chasles' opening line in his remarks on Cayley's paper, thus making clear the target of his criticism (which Chasles did not fail to notice³⁹).

Through the concluding remarks to his November communication, De Jonquières implicitly attacked Chasles' authoritarianism and his right to set the scope and direction of further research into the theory of characteristics, and the theory of curves and surfaces at large. To do so, De Jonquières pointed to previous research which he claimed was still relevant for the subject at hand, while not pertaining to the theory of characteristics proper – in fact, research whose results Chasles himself had either disparaged, or not even bothered to mention. This attack was not left unanswered by Chasles: the following week, on November 12th, he acknowledged the implicit criticisms of De Jonquières' and replied to them, effectively triggering the dispute⁴⁰.

Chasles reproduced the content of De Jonquières' 1861 paper, using extensive quotations, as well as that of the memoir submitted in 1862 as an entry for the Grand Prix de Mathématiques, which he himself had reviewed. Chasles then formulated the same criticism he had expressed against these previous texts. While Chasles finds the "lemma" (that is to say the analytical translation of the concept of series of curves) to be "obvious⁴¹" and denies having ever raised doubts about it (despite it being actually false!), he however finds the theorem stating that there are $2(n-1)N$ curves of order n in a series of index N tangent to a given line L to be "absolutely false⁴²", as $2(n-1)N$ is merely a maximal value. For Chasles, De Jonquières had defined systems of curves "in the same way as everyone else⁴³", but had been misled in his investigation thereof. This is a rather curious claim: there are indeed instances where the expression "system of conics" was used by Chasles and Poncelet much earlier⁴⁴, in a way that encompasses similar objects. However, it was never identified as an object or a figure around which to build a theory, nor was it explicitly defined as a collection of conics satisfying four arbitrary conditions.

After recalling these past judgements, to which he still held, Chasles proposed a new

³⁹ [Chasles, 1866d], p.818.

⁴⁰ [Chasles, 1866b].

⁴¹ "Cette proposition est évidence ; elle est la première, et il n'y en a pas de plus élémentaire dans toute la théorie des courbes", [Chasles, 1866b], p.818.

⁴² [Chasles, 1866b], p.819.

⁴³ [Chasles, 1866b], p.820.

⁴⁴ For instance [Chasles, 1837], pp.223, 372, 396.

narrative for the development of his own theory. In his own retelling, as Chasles turned to the theory of systems of curves, he naturally began with conics. He recognized immediately that points and lines were to play equal roles in this theory, and that the properties of elementary systems depended equally on the two numbers μ and ν . He pursued this insight by defining the characteristics of systems of conics defined by any four conditions. Chasles went on by recalling that, in 1855, he had presented a principle of correspondence, which he had predicted would be very useful in the theory of curves. And indeed, he only had to apply it to characteristics to obtain a variety of properties of systems of conics.

This is a novel narrative for the development of the theory of characteristics: in 1864, no reference to the 1855 paper was made, other than the name “principle of correspondence” being reused. In fact, these principles are rather different: the first is deeply tied to higher geometry, and the concepts of homographic division and involution, while the second says nothing of these projective properties. The link between these principles is subtler than a mere generalization from $(1,1)$ – or $(1,2)$ –correspondences to (α, β) –correspondences; it lies at the level of the method used to find and prove properties of curves. In both cases, correspondences are tools for the study of relations between series of points or lines, which serve to describe certain curves or surfaces. Furthermore, De Jonquières played a crucial role in the development of this principle, and in its extension to curves of order n , as we saw in section 3.2.2. The goal of this narrative is clear: Chasles aims to substantiate the claim that De Jonquières’ theory is in contradiction with his own, and in no way a predecessor to acknowledge. Chasles does so by describing his own theory as deriving from past work with which De Jonquières is said to have had nothing to do, and as being grounded upon the key insight that the two dual numbers μ and ν are equally necessary in the enumeration of conics, which is precisely the claim that De Jonquières contested. Concluding this self-serving narrative, Chasles explained why De Jonquières was not cited in his paper: as the latter’s results were fundamentally flawed, any citation would have had to be critical, and it was to avoid being “discourteous” (*désobligeant*) that he left out the name of his student from his own publications.

The exchange between Chasles and De Jonquières continued throughout the two subsequent meetings of the Académie des Sciences. On November 19th 1866, De Jonquières presented a rebuttal of Chasles’ last communication, after which Chasles himself made some remarks. The following week, on November 26th, Chasles added to his previous remarks, which he justified by what he took to be major changes between what had been said by De Jonquières on Monday 17th, and what had been printed in the *Comptes-Rendus*⁴⁵. De Jonquières maintains his priority claim over the introduction of series or systems of curves, and of the first characteristic. Another priority claim is introduced,

⁴⁵ [de Jonquières, 1866b], [Chasles, 1866a].

which concerns the method used to prove the $\nu = 2(n-1)\mu$ formula. De Jonquières claims it derives from his 1859 memoir on generalized involution⁴⁶, and refers to the works of Poncelet and Cremona as his main influences. De Jonquières, in this article, had considered two algebraic equations $X = 0$ and $X' = 0$ of degree n , whose roots represent the distances of n points on a line L with respect to an arbitrary origin. The equation $X + \lambda X' = 0$ was then taken to represent an infinity of groups of n points, each of which corresponds uniquely to one value of λ . These groups were said to be in a n^{th} -order involution, and notions such as the cross-ratio of four such groups, or the central point of such an involution, were introduced in an analogous manner to what Chasles had done in the 1852 *Traité de Géométrie Supérieure*. De Jonquières had used these notions to deal once more with the problems that Chasles was busy with at this time, such as constructing curves and equations of order three and four. This text, like Chasles' and De Jonquières' previous texts on the theory of curves, uses correspondences of series of points on a line; however, it is rather unclear why the 1861 proof of the $\nu = 2(n-1)\mu$ formula would be said to derive from the theory of generalized involution. Chasles was not convinced by this argument at all, and instead, asserted that the origin of this method for proving properties of curves was his 1855 principle of correspondence. None of them was willing to recognize that, more than a specific result, what this proof relied upon was a jointly created way of conducting proofs and writing geometrical properties.

To substantiate his priority claim, De Jonquières also discusses the validity of the $\nu = 2(n-1)\mu$ formula, which Chasles had openly and fully denied⁴⁷:

La condamnation si *absolue* que M. Chasles fait peser sur le théorème dont il s'agit n'est pas fondée. Je me suis trop avancé, il est vrai, en le présentant d'abord comme *toujours exact*, méconnaissant ainsi l'influence de certaines solutions singulières. De son côté, Chasles me paraît en faire autant quand il le regarde comme *absolument faux*. Car il faudrait pour cela que les solutions singulières existassent toujours, ou tout au moins que leur absence fût *exceptionnelle*, ce qui n'a pas lieu.

For De Jonquières, the formula is not false: it simply requires more qualification to take into account exceptional cases. As a result, against Chasles, De Jonquières maintains that both theories are not contradictory. Chasles repeats his claims that the theorems are false, the theories incompatible, the notion of system of curves older than De Jonquières' work, and the principle of correspondence which he had introduced in 1855 at the heart of the innovations brought by the theory of characteristics.

⁴⁶ [de Jonquières, 1859a].

⁴⁷ [de Jonquières, 1866b], p.873.

To make their cases, De Jonquières and Chasles both mobilize oral discussions, either with one another, or epistolary exchanges with third parties such as Cremona. Not a single geometer outside of Chasles and De Jonquières would publicly weigh in on this matter, including Cremona whose work and role in reframing De Jonquières’ results were at the center of the debate. Nevertheless, many such as Bischoff, Prouhet, or Thomas Archer Hirst, who all had been involved in one way or another with the theory of characteristics and its European diffusion, would write privately about this controversy. Hirst, an English geometer trained in Marburg in the synthetic geometry of Steiner, would for instance write in a letter to Cremona dated December 26th 1866⁴⁸:

I have read with great pain the controversy between Chasles and De Jonquières. I am afraid there are faults on both sides. Chasles is far too sensitive about his own reputation. His position in Science is so high and incontestable that one cannot but regret that he is not more generous toward younger Geometers. On the other hand de Jonquières ought, I think, to have shown more gratitude to the Tutor to whom he owes so much.

He deserves credit for his introduction of the index of a system of curves and had he been silent he would have obtained it. But his conception would have been almost unfruitful had it not been supplemented by Chasles’ magnificent researches. Moreover I do not think it is so original as de Jonquières supposes. I remember well that Steiner in his lectures at Berlin made use of the conception to some small extent.

The controversy, from the outside, was viewed as a regrettable matter of honor between a student and his former master, none of which was fully in the right.

5.1.5 A public trial without a jury?

The controversy was not pursued within the walls of the Académie, nor would it be in the *Comptes-Rendus*. As the dispute was getting stale, claims were being repeated without further justification and no ground was won. This changed, however, on December 8th 1866, when De Jonquières published the memoir he had sent to the Académie prior to the start of the dispute, entitled *Recherches sur les séries ou systèmes de courbes et de surfaces algébriques d’ordre quelconque*. In this memoir, De Jonquières’ priority claims were directly inserted within the mathematical exposition, and presented in a non-polemical fashion. For instance, De Jonquières opens by introducing *series* of curves, refers to his 1861 paper, then claims that Chasles called these objects *systems* in 1864. The rest of the memoir solely uses the terms *series*. Furthermore, the formulas attacked by Chasles

⁴⁸ [Dimitolo, 2016], p.4.

are stated with the added clause “en général et au plus”, in accordance with what De Jonquières held to be the absolutely correct formulation at this point of the debate. Very few new results are contained within this memoir: instead, it is a new exposition of past results, including Chasles’ theory of characteristics, which is largely praised. Discussing elementary systems, and Chasles’ suggestion that geometers focus their effort on the complete and systematic determination of their characteristics, De Jonquières gives a more comprehensive presentation of his new method, which relies heavily on the $2(n-1)\mu$ formula. De Jonquières, therefore, writes a text which seemingly follows the agenda set by Chasles, albeit with methods the latter had proscribed.

At the end of the version published by Gauthier-Villars, he had added an extra section to respond in detail to Chasles’ criticisms⁴⁹. This section constitutes a turning point in the controversy. On the back of a renewed epistemology of mathematical proofs, and novel reflections on the dependency between the truth of a theorem and its verification, De Jonquières changes subtly yet crucially his understanding of the logical status of his formulae and how they relate to Chasles’. Indeed, De Jonquières now asserts his theorems to have been “exactly true” all along, and point to Chasles’ inability to find a specific flaw in the proofs of these theorems as bearing witness to an insufficient understanding of the status of their apparent exceptions. This constitutes a key important epistemological evolution in De Jonquières’ thought, as he even ends up casting doubt on the identity between the index N and the first characteristic μ (without discussing the repercussions this move may have on the priority claim described above⁵⁰).

Through these communications, therefore, the claims of both protagonists seem to stabilize, and new sorts of arguments come into play. Both men extensively quote each other’s communications in order to find logical fallacies or inconsistencies. At some point, for instance, Chasles refers to a sentence in which De Jonquières asserted that he had not started this fight, before quoting another sentence where De Jonquières wrote “Toutefois j’ai profité de cette circonstance [his first communication, given on November 5th] pour rappeler un titre de priorité”. Chasles concludes that these statements are contradictory, and that he himself is not to blame for triggering this whole affair⁵¹. This display of proofs and evidence is reminiscent of a trial: in fact, judicial terms are employed by both actors on several occasions, and several communications are concluded with a call for others to judge on this controversy with the help of the evidence presented during these weekly meetings. De Jonquières concluded his memoir on series of curves with the following sentence: “Le public a maintenant sous les yeux les pièces principales du procès; c’est à lui qu’il appartient d’apprécier et de juger”.

⁴⁹ [de Jonquières, 1866c], pp.17-23.

⁵⁰ We shall come back to this point in section 5.2.3.

⁵¹ [Chasles, 1866a], p.909.

In the following months, both men published texts in turn in order to make their respective cases stronger. Chasles had a *Réponse à une revendication de priorité* published by Gauthier-Villars in January 1867, to which De Jonquières answered by penning on February 4th a handwritten text, of which he had copies printed as litographs⁵². Chasles had another booklet printed by Gauthier-Villars early in May, to which De Jonquières replied with an open letter, published on the 31st of the same month, in which he intentionally misspelled Chasles’ name⁵³! This back and forth exchange stopped, not because one had yielded or decisively won the fight; but because both men had other business to tend to. De Jonquières would soon have to leave France to head the battleship *La Savoie*, aboard which he would take part in several wars, including the 1870 defeat against Prussia. Meanwhile, Chasles would be embroiled in the infamous Vrain-Lucas affair, another scandal which started within the walls of the Académie, and which provided journalists in the attendance with material for juicy stories for years. On the contrary, it is unclear whether anyone other than these two protagonists found interest in this public display, which was hardly discussed in newspapers or scientific journals. This exchange is referenced by few, other than the geometers and historians who took up this topic in the immediate aftermath of the controversy⁵⁴.

The form of these documents display a real intent by both men to convince their peers of their priority, but also to paint the other one as failing to abide by the rules of the mathematical profession. Indeed, Chasles had thus far adopted the posture of the intellectual authority, and had, at several occasions, criticized De Jonquières for not acting properly as mathematician, for instance claiming that “his manner of acting is so contrary to scientific habits, that M. de Jonquières must have some reason to justify it for himself⁵⁵”. In fact, for Chasles, the flaws of De Jonquières’ mathematics find their very origin in the flaws of his scientific and scholarly conduct⁵⁶. The dispute, for Chasles, is not merely mathematical, but also moral. Amongst De Jonquières’ less consequential moral flaws, Chasles finds the hastiness and imprecision of his writings⁵⁷.

De Jonquières certainly noticed these attacks: in fact, he even agreed to have the dispute be partially held on moral grounds. Concluding his memoir, De Jonquières would contrast his “very modest revendication”, and the “moderation, reserve and deference of his own argumentation” to the “irritated tone” (*ton irrité*) of Chasles⁵⁸. Later, he would

⁵²For another case of a mathematical ‘polemic in public’, see [Lorenat, 2015b].

⁵³ [Chasles, 1867a], [de Jonquières, 1867a], [Chasles, 1867b], [de Jonquières, 1867b].

⁵⁴ [Segre, 1892], [Loria, 1902].

⁵⁵ [Chasles, 1866b], p.877.

⁵⁶“Ce sont ces écarts des règles observées par les géomètres qui, à mon sens, ont conduit M. de Jonquières aux erreurs qu’il m’a mis, bien volontairement, dans la nécessité de signaler”, [Chasles, 1866b], p.876.

⁵⁷“Je dois croire que M. de Jonquières a écrit sa note très à la hâte..”, [Chasles, 1866a], p.908.

⁵⁸ [de Jonquières, 1866c], p.23

contrast his “self-defense” with the “attacks” of Chasles, despite having himself much to fault with his master’s theory⁵⁹. Throughout his writings, De Jonquières depicts Chasles as a stubborn, jealous mathematician, one who is too proud of his discoveries to share them with the rest of the scientific community, thus failing on moral grounds. This moral failing is then shown to have dire consequences on an aging Chasles’ ability to keep track of recent developments in a science over which he had for too long reigned⁶⁰:

Quand je songe à certains incidents qui ont agité, dans ces dernières années, le domaine de la géométrie, et aux tendances diverses qui s’y manifestent, je comprends qu’il puisse y avoir là pour vous des causes de trouble et de malaise. Cette ardeur que vous montrez pour les luttes de la discussion, cette irritation profonde dont le Compte rendu du 12 novembre 1866 a vu la première explosion, ont peut-être des racines plus étendues qu’on ne croirait d’abord. S’il en était ainsi, je ne serais guère pour vous qu’un prétexte, ou du moins (passez-moi cette expression familière) ce n’est pas moi seul que vous auriez l’intention de battre en frappant sur mon dos.

Furthermore, replying to the accusations of hastiness and of failing to abide by the professional code of mathematicians, De Jonquières placed himself under the protection of Poincaré⁶¹:

Des critiques de M. Chasles, il ne resterait donc à ma charge que l’emploi de quelques locutions hasardées, telles que “pas toujours exact”, ou “le plus souvent”, qui dénoteraient de ma part, soit un manque d’explications, soit une révision trop hâtive de divers textes, soit même une certaine inexpérience du style mathématique (*).

(in a footnote) (*) Quelques réflexions introduites par l’illustre Poincaré dans sa *Théorie nouvelle de la rotation des corps* (2^e partie) sont bien propres à rassurer les auteurs (pourvu qu’ils ne se déconcertent pas tout d’abord) sur les conséquences exagérées qu’on pourrait déduire de cette inexpérience, en quelque sorte professionnelle; car il y prend hardiment leur défense contre ceux qui seraient tentés d’en tirer avantage contre leurs découvertes.

Referring to Poincaré is a meaningful move: he was a constant ally of Chasles’, both institutional and philosophical. In particular, the very text to which De Jonquières refers is one that Chasles had extensively read and praised in his own *Aperçu Historique*. While

⁵⁹ [de Jonquières, 1867a], p.9.

⁶⁰ [de Jonquières, 1867b].

⁶¹ [de Jonquières, 1866c], p.22. In fact, De Jonquières relies on Poincaré’s writings in his mathematical and philosophical argument as well, as we shall see below.

no explicit quotation is given, it is likely that De Jonquières is referring to the following passage⁶²:

Sitôt qu’un auteur ingénieux a su parvenir à quelque vérité nouvelle, n’est-il pas à craindre que le calculateur le plus stérile ne s’empresse d’aller vite la rechercher dans ses formules, de la découvrir une seconde fois, et à sa manière, qu’il dit être la bonne et la véritable; de telle sorte qu’on ne s’en croie plus redevable qu’à son analyse, et que l’auteur lui même, quelque fois peu exercé, ou même étranger à ce langage et à ces symboles sous lesquels on lui dérober ses idées, ose à peine réclamer ce qui lui appartient, et se retire presque confus, comme s’il avait mal inventé ce qu’il a si bien découvert ? Singulier artifice, que je n’ai pas besoin de caractériser davantage, mais qu’il est bon de signaler comme un des plus nuisibles aux progrès des sciences, parce qu’il est sans contredit un des plus propres à décourager les inventeurs !

Poinsot, in line with his quasi-empiricist account of mathematical knowledge which Chasles so admired, opposed the expert and sterile analyst to the sometimes inexperienced geometer, whose knowledge proceeded from the direct contemplation of things⁶³. De Jonquières purposely ignored this specific subtext to draw from another facet of Poinsot’s text, namely the attack against the primacy of analytical methods and the unwarranted superiority granted by the high level of technicity and training that their mastery required. Indeed, after the 1820s, as two historians recently put it, “the decline of descriptive geometry and the rise of mathematical analysis and mechanics were to transform the very image of the engineer: (...) the engineer–draughtsman–artist of the Age of Enlightenment gave way to the modern engineer, author and practitioner of ‘mathematized’ applied sciences, who resembled a scientist much more than an artist⁶⁴”. Poinsot’s argument is not merely epistemological: it is also political, a defense of the amateur’s ability to obtain truth by the direct contemplation of nature, against the growing hegemony of highly-trained engineers who derive their truths from the blind operation of a flawed instrument. This political charge is what De Jonquières reactivates, except this time, it is aimed at an Académicien⁶⁵.

De Jonquières, at the time of the controversy, occupied a much more marginal position within the social space of mathematicians - or at the very least, he described and per-

⁶² [Poinsot, 1851], p.89.

⁶³Earlier in the same article, and in way reminiscent of Chasles’ epistemology of geometry, Poinsot had opposed the “long and difficult computations” and the “auxiliary quantities” of the analyst to the “idée claire” obtained by those who “consider things in themselves”, [Poinsot, 1851], pp.10-11. This quote can be compared with Chasles’ portrait of the geometer, which we discussed in chapter 1.

⁶⁴ [Belhoste & Chatzis, 2007], p.221

⁶⁵De Jonquières addresses Chasles as an Académicien, or as a respected savant, at several occasions through the course of this controversy. See for instance [de Jonquières, 1866c], p.17.

ceived himself as such, both in his private correspondence and in his 1884 autobiography as part of his application to the Académie. In fact, his remoteness from Paris is even invoked as a reason for his rapid renunciation of his 1861 results, as he received Chasles' criticism at a time and place which left him incapable of studying them closely⁶⁶. By and large, De Jonquières was perceived as a disciple of Chasles. Chasles, on the other hand, had just won the Copley medal⁶⁷, was member of dozens of scientific societies and Academies across Europe, and enjoyed a relative fame even amongst non-scientists that few mathematicians could pretend to. When he died some thirteen years later, general newspapers in various countries would print detailed obituaries, describing him as one of the most notable mathematicians of the century⁶⁸. And yet, this was a time when the moral and intellectual authority of the Académie was beginning to show signs of wear⁶⁹, and De Jonquières felt capable of proudly standing his ground as an *amateur* in his own right against the rigid and proud *académicien*.

This imbalance in social positions and institutional power was clear to all: De Jonquières' responses to Chasles' criticisms all constitute attempts to find means of counter-acting it. Several strategies are employed by De Jonquières. In his *Documents relatifs etc.*, he had extensively quoted from a letter by Cremona, in which the Italian geometer had attempted to convince him of the validity of his previous results, and of the rightfulness of his priority claim. A few months later, De Jonquières concluded his open letter to Chasles by a selection of excerpts from letters which, he claimed, had been spontaneously sent to him by mathematicians worldwide, and which all supported his priority claim⁷⁰. In the very same text, De Jonquières also addressed a challenge to Chasles, by way of a mathematical problem which he thought the theory of characteristics was not as equipped to tackle as his own theory⁷¹:

Question à résoudre. – Combien y a-t-il de courbes proprement dites du septième degré qui passent par 26 points données, qui touchent quatre courbes d'ordres respectifs m, m', m'', m''' , et qui sont normales à cinq autres courbes données d'ordres respectifs n, n', n'', n''', n^{iv} ?

What had started as a rather banal priority claim soon turned into a dispute over the validity of some results, and eventually over the respective merits of two distinct geometrical theories. As the dispute escaped the Académie, it turned into a public trial, with its spectacular apparatus made of presentations of evidence and counter-evidence,

⁶⁶ [Chasles, 1867a], p.4.

⁶⁷ On the attribution of this medal, see 6.1.1.

⁶⁸ See for instance the obituary published in *Nature*, [Tucker, 1881].

⁶⁹ [Fox, 2012], pp.236-237.

⁷⁰ [de Jonquières, 1867b]

⁷¹ [Chasles, 1867b], p.12.

and of negative portrayals of the other side. However, this trial had no jury: whatever mathematicians may have written privately to De Jonquières, no authority would weigh in publicly on this dispute.

While no geometers publicly took a stance on this controversy, several would later publish their readings of this dispute. Italian geometers after Cremona would share a reading of this dispute, wherein the wrongs were divided. Chasles would be blamed for being too proud, and for refusing to acknowledge his debt toward De Jonquières. In this view, the student had made important contributions, which Chasles had only perfected. For instance, Corrado Segre, in a paper on the history of the principle of correspondence, wrote⁷²:

Ora si può ben dire che in ciò l'illustre scrittore dell'*Aperçu historique* aveva torto. Poiché, ad onta delle inesattezze od imperfezioni che vi si trovano [De Jonquières' early papers], quel lavoro ebbe indubbiamente una notevole influenza sulla scienza, alla quale dava non solo dei teoremi, ma dei concetti e dei metodi nuovi ; e varie proposizioni importanti *sui sistemi di coniche* esposte dallo CHASLES nel 1864 non sono che un *perfezionamento* — grazie all'introduzione fatta da CHASLES del *secondo* indice o caratteristica — di cose già date dal JONQUIÈRES *per sistemi d'ordine qualunque* nel 1861 (23). Che se poi si riguarda alle critiche speciali fatte dallo CHASLES, di esse (dopo qualche esitazione) poté giustamente scagionarsi il JONQUIÈRES (prima ancora che nascesse il litigio, e cioè nelle Note di Saigon) osservando che i numeri incriminati, come il già citato $2(n-1)N$, sono da modificare solo quando si vogliano sottrarre da essi le *soluzioni singolari* che possono comparire e che effettivamente nel determinarli non furono escluse.

Similarly, Italian historian of mathematics Gino Loria would later attempt to rehabilitate De Jonquières, whom he viewed as Chasles' most important student. In one of two papers on De Jonquières, he wrote⁷³:

⁷²“Now it can be said that the illustrious writer of the *Aperçu historique* was wrong about this. Since then, and despite the inaccuracies or imperfections found [in De Jonquières' early papers], this work undoubtedly had a considerable influence on science, to which it gave not only theorems, but also new concepts and methods; and various important propositions on systems of conics exposed by Chasles in 1864 are only an improvement - thanks to the introduction made by Chasles of the second index or characteristic - of things already given by Jonquières for systems of any order in 1861. If we then consider the specific criticisms made by Chasles of this work, Jonquières could (after some hesitation) rightly exonerate himself (even before the quarrel was born, that is in [Saigon's Notes]) by observing that the incriminated numbers, like the already mentioned $2(n-1)N$, are to be modified only when we want to subtract from them the singular solutions which can appear, and which were effectively not excluded in determining this number”, [Segre, 1892], pp.190-191.

⁷³“In the theory of systems of curves, it is necessary to consider two periods ; the first one, which must be named after De Jonquières, the other one after Chasles ; **the first one prepares the second**, but the

Dans la théorie des systèmes de courbes, il est donc nécessaire de considérer deux périodes ; l'une, qui doit porter le nom de De Jonquières, l'autre qui doit porter celui de Chasles ; **la première prépare la seconde**, mais celle-ci a **un caractère de perfection**, dont celle-là était dépourvue. C'est ce qu'aujourd'hui tout le monde accorde sans peine, mais que ni Chasles ni De Jonquières ne voulurent jamais reconnaître. Le maître méconnaissait à tort que l'introduction de l'indice d'une série a marqué dans la science un progrès important ; mais l'élève, irrité d'attaques trop vives, arriva à nier la fécondité de définir par deux caractéristiques tout système de coniques, (..) dont on ne peut plus douter.

Zeuthen, for instance, in his 1905 entry on *abzählenden Methoden* for Klein's and Meyer's *Encyklopädie der mathematischen Wissenschaften*, would also mention this controversy in a historical account of the development of enumerative methods, and for instance present De Jonquières as the true introducer of the principle of continuity⁷⁴ in such questions. In all of these readings, the focus is on a few isolated matters of priority: whether De Jonquières or Chasles had first introduced the principle of correspondence, who had introduced the notions of systems and characteristics etc. Behind this priority quarrel so typical of Parisian scientific life of this period, however, a more profound dispute on the very nature of generality in mathematics was brewing.

5.2 A clash of generalities

The mathematical and epistemological arguments expressed within the course of this dispute seem to have had little sway on the discussion. Chasles' responses focused on dismantling the historical narrative proposed by De Jonquières, and they mostly relied on a juxtaposition of handwritten notes, letters, and quotations from previous papers, aimed to show the incoherence of De Jonquières' claims. On the other hand, De Jonquières elected to paint Chasles as an old and proud *académicien*, either jealous of or blind to the achievements of his own student. However, besides these personal attacks, De Jonquières also constructed a two-fold line of defense of his results, by means of a redefinition of generality in geometry. On the one hand, De Jonquières would argue for the generality of some of his results by measuring their domain of validity, introducing a mathematical

latter is **characterized by a perfection** of which the former was devoid. This is what today everyone grants easily, but which neither Chasles nor De Jonquières ever wanted to acknowledge. The master wrongly misunderstood the important progress that the introduction of the index of a series brought to science ; but the student, irritated by too lively an attack, ended up denying the fruitfulness of defining with two characteristics any system of conics, a fruitfulness which can't be doubted anymore", [Loria, 1902], p.298.

⁷⁴ [Zeuthen, 1905], p.279.

practice foreign to Chasles’ conception of geometry. On the other hand, De Jonquières would mobilize a distinction between the languages of geometry and algebra, to drive a wedge between the kinds of generality and exactness both sciences can achieve, thereby trying to explain away the remaining inconsistencies between his theory and Chasles’.

5.2.1 Theoretical numbers of solutions

In his notes from Saïgon, De Jonquières claimed that Chasles himself had found the origin of the discrepancy between the numbers produced by the 1861 formulae, and the numbers of conics one can actually count in elementary cases. The same explanation was copied in two of these notes⁷⁵:

Les anomalies dont il s’agit ont été expliquées par M. Chasles, dans l’un de ses beaux Mémoires sur les systèmes de courbes (*Comptes Rendus de l’Académie des Sciences* pour 1864). Elles tiennent à ce que, dans tout système de coniques, il y a un certain nombre de ces courbes qui se réduisent exceptionnellement à deux droites ou à deux points (coniques infiniment petites et coniques infiniment aplaties). Ces coniques exceptionnelles sont, à plusieurs égards, étrangères à la question, comme on a coutume de dire en analyse, et pourtant il faut les compter, si l’on veut retrouver le nombre théorique donné par les formules générales qui les comprennent toutes indistinctement.

It is unclear what exactly De Jonquières is referring to. Chasles discussed degenerate conics (which he also calls “exceptionnelles”), as well as “solutions étrangères” (see sections 4.2.4 and 4.3.2). However, he only used these concept to subtract to numbers provided by the principle of correspondence (within proofs, and before stating the final result), or to specify how many curves among those given by his methods were proper curves. Instead, the justification to which De Jonquières alludes seems rather to be that given by Cremona in 1863, which we discussed earlier (see section 5.1.2). Cremona’s explanation had not only been published before Chasles’ 1864 communications, but it had been sent directly to De Jonquières in private letters.

Regardless of the authorship of this justification of the discrepancy between De Jonquières’ formulae and the numbers of curves which can actually be computed in simple cases, it is worth stressing how De Jonquières actually frames it. In the last sentence of this quote, he introduces the idea of a “theoretical number”, which is that provided by

⁷⁵ [de Jonquières, 1865a], pp.412-413; [de Jonquières, 1865c], p.45. It must be noted that, in his polemical publications, De Jonquières almost systematically employs the expression “series or systems of curves and surfaces”, as a way to make clear the equivalence of the two notions, and thereby his priority claim. In what follows, we shall only refer to systems of curves for the sake of brevity, except where further precautions are needed.

general formulae. For instance, in the case of the condition ‘touching a given straight line’, and of a system of curves of order m and of index N , this number is $2N(m-1)$. This number, for De Jonquière, is to be differentiated from the actual number of curves present in the system which satisfy the elementary condition. The difference between these two numbers is caused by the occasional presence of “solutions étrangères”, in this particular case double-lines.

This distinction is framed by De Jonquière as one between the theoretical and the applied⁷⁶:

C’est le propre des formules algébriques d’exprimer à la fois tous les cas possibles, singuliers ou non, que présente une même question.

Mais dans les applications géométriques, on écarte naturellement les cas trop particuliers, disons les solutions étrangères, qui ne conservent plus aucun des caractères apparent qu’offrent les cas généraux, et c’est ainsi que la formule $\nu = 2(m-1)\mu$ se trouve parfois en désaccord avec les *faits*.

De Jonquière’s own 1861 formulae, and in particular the $\nu = 2(m-1)\mu$ formula, were now likened to the formulae of analysis, in that it enumerates curves without any regard for their validity as a geometrical solution to a problem; whereas Chasles’ method has the extra benefit of directly enumerating proper solutions. In other words, De Jonquière’s methods enumerates curves purely as polynomials in two variables, including polynomials such as $(ax + by + c)^n$ which are only a power of a linear term⁷⁷. Geometrical formulae, for De Jonquière, state “facts”; they count figures that happen to be and satisfy certain conditions. Algebra, on the contrary, operates at a theoretical level, detached from actual figures. While De Jonquière thought that both Chasles’ and his own theory were valid⁷⁸, he attributed to them different kinds of generality. Indeed, De Jonquière was not merely writing that his formulae were true outside of certain cases: he was both asserting their generality, and that they sometimes ran into anomalies.

Whilst equating μ and N , and by holding the formula $\nu = 2(m-1)N$ to be true, De Jonquière also maintained that Chasles’ theory of characteristics was equally valid. For De Jonquière, this latter theory was a mere simplification of his own, one with more symmetry and perhaps easier methods of computation. However, he also drew some theoretical conclusions from the equivalence of both theories. For instance, from De

⁷⁶“Algebraic formulae, by nature, express at the same time all possible cases, singular or not, of one same question. But in geometrical applications, we naturally discard cases that are too particular, let us say foreign solutions, which do not retain the apparent characteristics that general cases present, and this is why the formula $\nu = 2(m-1)\mu$ is sometimes at odds with the facts”, [de Jonquière, 1865c], p.48. Emphasis in original.

⁷⁷ [de Jonquière, 1865c], p.48.

⁷⁸ [de Jonquière, 1865c], pp.48-49.

Jonquières’ formula given above, it follows that $m = 1 + \frac{1}{2}\frac{\nu}{\mu}$, which is always an integer. As a result, not every pair of numbers (μ, ν) can represent a system of curves, as there is a supplementary condition on the ratio between the two characteristic numbers.

Another, more important conclusion of De Jonquières’ is that two numbers suffice to characterize a system of curves⁷⁹, but that these numbers are not unique: the pair (μ, ν) and the pair $(\mu, m) = (N, m)$ fulfil this task equally well. This is a strong claim, as it implies that for every pair of integers (a, b) , there can only be a system of curves of characteristics (a, b) for a single order m . In particular, given a pair of integers, there is no guarantee that there is a system of conics of which these integers are the characteristics. This claim, while hardly discussed by contemporary commentators, would later be levied by De Jonquières’ against Chasles’ $\alpha\mu + \beta\nu$ formula. Indeed, when Chasles opposed to De Jonquières the fact that the principle of correspondence had shown that numbers of conics in a system (μ, ν) satisfying an arbitrary condition depended both on μ and on ν , De Jonquières would respond that the order $m = 2$ played an implicit role in these formulae, for only specific pairs (μ, ν) can serve as the characteristics of a system of conics (as opposed to higher-order curves)⁸⁰.

5.2.2 Delineating the domain of validity of a formula

To the distinction between theoretical numbers and facts, De Jonquières attached a new mathematical practice which had been absent from his (or Chasles’) geometrical papers theretofore: the practice of delineating the domain of validity of a formula. For De Jonquières, one could hold a formula to be generally true whilst acknowledging that its application to concrete cases occasionnally leads to anomalies. To remedy this situation, De Jonquières would try to pinpoint the boundaries within which these anomalies can arise, so as to more precisely qualify the sort of generality and applicability of his formulae.

To that end, De Jonquières divided the totality of elementary systems of curves of order m into two categories. Such a system being defined by $\frac{m(m+3)}{2} - 1$ conditions (either passing through a point, or touching a straight line), De Jonquières let T denote the number of these conditions which are of the form ‘passing through a given point’. Elementary systems of the first category are those for which

$$T > \frac{m(m-1)}{2} + 1$$

Ranking these systems by decreasing values of T , De Jonquières claimed, one obtains the

⁷⁹ [de Jonquières, 1865a], pp.414-415.

⁸⁰See for instance [de Jonquières, 1867b].

following sequence of characteristics:

$$(1, \alpha), (\alpha, \alpha^2), (\alpha^2, \alpha^3), \dots \text{ where } \alpha = 2(m-1)$$

The first characteristic is 1, as a curve is uniquely determined by $T_{max} + 1 = \frac{m(m+3)}{2}$ points. The $\nu = 2(m-1)\mu$ formula yields the other values. A similar distinction, and sequence of characteristics, is given for systems of surfaces⁸¹. The characteristics of systems of the second category, for which T is smaller than $\frac{m(m-1)}{2} + 1$, are not given by such a simple progression.

When T is too small, De Jonquières explains, “elementary systems contain singular solutions, which, by their very presence, oppose the determination [of characteristics] with a difficulty that is most of the time insurmountable⁸²”; a claim which he had substantiated in the third note from Saïgon⁸³. Indeed, De Jonquières had claimed that only a specific kind of exceptional curve could lead to discrepancies between his formulae and Chasles’ in the case of elementary systems, namely curves “infiniment aplaties”, that is to say curves of which one branch is a straight line of multiplicity $r > 1$ ⁸⁴:

Quant aux coniques infiniment petites, et aux courbes d’ordre supérieur réduites à des points, elles sont sans influence sur la formule $[\nu = 2(m-1)\mu]$; elles ne pourraient en exercer sur elle, que si cette formule se rapportait à des séries de courbes envisagées relativement à leur classe, donc si l’on intervertissait en conséquence la signification des deux caractéristiques ; ce seraient au contraire les coniques infiniment aplaties et les courbes doubles multiples qui seraient alors sans effet sur elle⁸⁵.

Furthermore, these infinitely thin curves, which are the sole cause for error in De Jonquières’ formulae, only happen in systems of the second category, with T sufficiently small. Indeed, consider an elementary system of curves, that is to say a system defined by a collection of T given points and straight lines. Any “infinitely thin curve” necessarily touches every given straight line; so to be in the system, it only has to pass through the given points. Furthermore, such a curve can be viewed as composed of one infinitely thin conic, and a curve of order $m-2$. However, a curve of order $m-2$ is uniquely defined by

⁸¹ [de Jonquières, 1866d], p.795.

⁸² [de Jonquières, 1866d], p.794.

⁸³ [de Jonquières, 1865c], pp.49-50.

⁸⁴ “Infinitely small conics, and curves of higher order reduced to points, have no influence over the $\nu = 2(m-1)\mu$ formula; they could exert some influence only if this formula was concerned with series of curves viewed with regard to their class, that is to say if we interverted the meaning of both characteristics; in this case, it would be the infinitely thin conics, and the multiple double curves which would not have any effect on the formula”, [de Jonquières, 1865c], p.49.

⁸⁵ Once again, De Jonquières’ theory is explicitly not dual.

$\frac{(m-2)(m-2+3)}{2}$ points; therefore for an infinitely thin curve to be in an elementary system with T given points, it is necessary that

$$T \leq \frac{(m-2)(m-2+3)}{2} = \frac{m(m-1)}{2} + 1$$

When T is sufficiently large, that is to say strictly above $\frac{m(m-1)}{2} + 1$, the points are too numerous for one curve of order $m-2$ to pass through all of them, and no curve of order m comprised of a line-pair conic and a curve of order $m-2$ can be in the system. Therefore, only systems of the second category have infinitely thin curves, and De Jonquières’ formulae are valid in systems of the first category.

For De Jonquières, the crucial import of this proof is that it gives an *a priori* delination of the domain of validity of the general, algebraic formula; that is to say, the domain in which the geometrical exactness thereof is preserved⁸⁶. In particular, De Jonquières frames this discovery as the proof that his past works can be put to use within the research program outlined by Chasles, namely the computation of characteristics of elementary systems: he has obtained a vast number of such characteristics in a very simple manner, and has shown that the occasional variations between Chasles’ formulae and his own did not arise in these cases. In fact, in the case of curves and surfaces of the second order, De Jonquières’ numbers are enough to determine all characteristics of elementary systems by duality (or polarity⁸⁷).

However, the presentation of this work by De Jonquières was not entirely uncritical of Chasles’ own program. We already mentioned De Jonquières’ attack on Chasles’ repeated claim that the main task ahead of geometers was the determination of *all* of the characteristics of systems of curves and surfaces. For De Jonquières, several of his former enumerative formulas, and in particular those pertaining to contact conditions, were in fact exactly true as long as the number of points amongst the given conditions (beyond elementary systems) was sufficiently high⁸⁸.

Chasles was far from convinced by these arguments. At first, he simply reiterated his claim that $2(m-1)\mu$ was merely a maximum for ν , that the formula was absolutely false, and so were all the theorems that followed from it. In his second communication to the Académie des Sciences, De Jonquières reformulated his argument: he convened that the formula was not “always exact”, but maintained that it was not “absolutely

⁸⁶ [de Jonquières, 1865c], p.49; [de Jonquières, 1866d], p.794.

⁸⁷Indeed, for conics, De Jonquières’ formula, when restricted to cases where T is sufficiently large, gives the characteristics of the systems $(4p.,)$ and $(3p., 1d.)$, that is to say $(1, 2)$ and $(2, 4)$. Per duality, it follows that $(4d.,) \equiv (2, 1)$, and that $(3d., 1d.) \equiv (4, 2)$. The characteristics of $(2p., 2d.)$ are respectively the second characteristic of $(3p., 1d.)$ (that is to say 4), and the first characteristic of $(1p., 3d.)$ (which is 4 as well).

⁸⁸ [de Jonquières, 1866d], p.796.

false⁸⁹”. Chasles first understood this contrast as expressing the fact that the formula was sometimes true, that is to say that the maximal value was indeed attained in several cases, to which he agreed whilst rejecting it as a mathematical theorem⁹⁰. However, as part of his attack on De Jonquières’ legitimacy as a mathematician, and on his alleged ignorance of the common rules of scientific life, Chasles made his criticism sharper with time. In fact, he accused De Jonquières of misunderstanding what generality meant for mathematicians, and what a theorem was. In his response to De Jonquières’ last 1866 communication, he wrote⁹¹:

On remarque dans l’énoncé textuel de la démonstration que je viens de rapporter le mot *généralement*. Que signifie-t-il? Veut-il dire *le plus souvent*, comme dans une autre proposition, où on lit : “Pour que la formule (b) ne contienne aucune solution singulière, il faut et il suffit *le plus souvent* que la condition, etc.” ? Ce *plus souvent* est-il un résultat de statistique ou du calcul des probabilités? (..)

Ce sont ces écarts des règles observées par les géomètres qui, à mon sens, ont conduit M. de Jonquières aux erreurs qu’il m’a mis, bien volontairement, dans la nécessité de signaler.

Shortly thereafter, in his first published booklet on the controversy, Chasles went even further in his scathing critic of De Jonquières’ scholarly behavior and understanding of the rules of mathematical practice⁹²:

Les géomètres n’appellent point *théorème* une proposition tantôt vraie et tantôt fausse, bien qu’elle pût être vraie *le plus souvent*, ou dans un nombre notable de cas.

Chasles rejected both De Jonquières’ legitimacy as mathematician, and his conception of generality. While Chasles had insisted on the uniformity and simplicity of the theory of characteristics, De Jonquières is trying to argue for a generality of a theorem that is characterized by both the large number of cases to which it applies, and to the fact that its domain of validity can be known *a priori*. As Chasles refused to comply to De Jonquières’ priority claims, and sharpened his own rhetoric, the student had to make his own position more explicitly opposed to that of his master. In so doing, De Jonquières

⁸⁹A theorem that is absolutely false, for De Jonquières, would be something like “the cube of the hypotenuse is equal to the sum of the squares of the sides of the right angle”. Whether this means a theorem which is always false, or something subtler, is not made explicit by De Jonquières. See [de Jonquières, 1866c], pp.18-19.

⁹⁰ [de Jonquières, 1866b], pp.872-873; 875-876.

⁹¹ [de Jonquières, 1866b], p.876.

⁹² [Chasles, 1867a].

widened the gap between these two generalities. During the Monday meetings of the Académie, De Jonquières had straddled a line between generality and exactness: he was willing to recognize cases where his formulas were not applicable, whilst maintaining the claim of their generality. This variation in epistemic values, however, betrayed a larger rift opening between them: De Jonquières was departing from Chasles on the very question of the relation between the language of algebra and that of geometry.

5.2.3 Algebra versus *pensée de vérification*

In the memoir he wrote in December 1866, that is to say right after the initial clash with Chasles at the Paris Académie, De Jonquières feigned surprise at Chasles’ criticism that mere verifications would have shown the falsity of his 1861 enumerative formulae⁹³:

Les calculs numériques ou algébriques, les opérations seules [sont] soumises au contrôle de la vérification. [...] Un théorème est bien établi, si le point de départ est juste et si les règles de la logique sont observées dans le cours de la démonstration. Dans ce cas, une vérification qui ne le vérifierait pas devrait elle-même être déclarée fausse, avant tout examen. [...]

Ainsi, au lieu d’être *absolument faux*, comme l’avance M. Chasles, le théorème dont il s’agit [the $2(m-1)\mu$ formula] est ABSOLUMENT EXACT. Telle est ma conclusion formelle sur ce point capital du débat. Quelle cause, dans cette circonstance, a donc pu égarer l’appréciation d’un si éminent géomètre? C’est, je crois, dans cette pensée de *vérification*, mise en avant par lui, qu’on peut la découvrir. En effet, si l’on cherche à vérifier la formule sur certains cas particuliers des sections coniques pour lesquels le résultat est connu *à priori*, on reconnaît immédiatement qu’elle présente des anomalies dont M. Chasles a le premier donné la clef, mais dont il n’eût pas dû conclure, ce me semble, que le théorème général est faux. Car, avec un peu d’attention, on ne tarde pas à voir que ces contradictions *apparentes* proviennent uniquement de ce que l’esprit introduit, après coup et au moment où il considère le résultat, des restrictions dont il n’était nullement question dans l’énoncé.

De Jonquières contrasted the logical perfection of his *a priori* proofs, and the introduction of *a posteriori* restrictions of the outcome of these proofs. The geometer, expecting the solutions to be pure curves, finds the facts to contradict the theory; but this is only because the initial wording of the question was insufficiently precise. This is why the formula can be both general and present anomalies: its generality derives from it being a valid algebraic proof, and the anomalies derive from that geometers introduce new

⁹³ [de Jonquières, 1866c], pp.19-20.

requirements on the nature of the curves they wish to count. Thus, for De Jonquières, the generality of geometrical and algebraic results ought to be evaluated differently.

By pursuing this line of defense, De Jonquières was framing the controversy in epistemological terms reminiscent of earlier debates within French mathematics. In particular, in a footnote to his response to Chasles, De Jonquières referred to the conception of Algebra which Poinsothad expounded in his work on number theory⁹⁴. In statements which echo Condillac’s description of algebra as “une langue bien faite, et la seule⁹⁵”, Poinsothad delineated the epistemic properties of the language of algebra for the study of number theory⁹⁶:

L’algèbre ne donne donc rien au delà de ce qu’on lui demande ; elle n’est pas plus générale que la logique considérée dans sa perfection, et le degré où l’équation s’élève est le degré même de la question, si elle est parfaitement posée.

Mais le plus souvent nos énoncés sont imparfaits ; je veux dire, qu’indépendamment de cette relation qui lie aux données l’inconnue et qui la *détermine*, notre esprit y mêle encore certaines conditions inutiles et souvent contradictoires ; et voici alors ce qui nous arrive. Comme ces sortes de restrictions ne donnent point d’équation, et ne sont pas de nature à s’écrire en algèbre, l’équation qu’on tire de l’énoncé se trouve exactement la même que si ces suppositions n’avaient point lieu.

Poinsothad developed mechanical theories whose physicalist and quasi-empiricist traits had largely influenced Chasles, and he was responsible for Chasles’ position at the Faculté de Paris. As such, it was a perfect ally to summon in a controversy against Chasles. The rhetorical strategy here employed is clear: De Jonquières aims to show that even an ally of Chasles’ understood that the relation between the formulae of algebra and of other branches of mathematics was such that one could accommodate the generality of a formula and the occasional presence of anomalies.

In fact, Poinsoth’s argument echoed and responded to a much older debate, which had begun in the second half of the 18th century. This debate was that of the introduction and signification of negative numbers in geometry. In the entry on “Equations in algebra” of the *Encyclopédie*, which he partially edited, the mathematician and philosopher Jean D’Alembert had cast doubts over the adequation between a geometrical problem and its algebraic translation⁹⁷. Due to the “richness of Algebra”, D’Alembert contended,

⁹⁴ [Boucard, 2011], pp.265-279.

⁹⁵ [Condillac, 1877], p.4.

⁹⁶ [Poinsoth, 1845], p.9.

⁹⁷“La traduction algébrique du premier énoncé est par sa nature plus générale que ce premier énoncé

equations yielded more solutions than a problem really admitted, for instance by means of negative numbers. These numbers, in turn, allowed for no simple “metaphysics”. While some had sought to explain negative numbers as “numbers below zero” by means of analogies for instance with bookkeeping-practices, such cultural artifacts had no place in the philosophical discussion of mathematical truths, whose own necessity ought to be the sole justification for D’Alembert (and many other Lockean philosophers in his circles, such as Condillac⁹⁸). Other modes of introduction of negative numbers, for instance as quantities viewed in opposite direction to positive numbers, had also been rejected as unsatisfactory⁹⁹.

Carnot, in his 1803 *Géométrie de position*, had made his own D’Alembert’s problem; which he attempted to solve by means of his “tableaux de corrélation”. In his method, one first sets up a primitive system of figures, lists the quantities which can be formed on the basis of these figures (lengths, angles, ratios..), and writes the possible formulae pertaining to them (which, at this point, only involve symbols standing for positive numbers). Considering a continuous motion of the system, one then introduces negative signs to denote the transformations which these primitive formulae ought to undergo when certain quantities become null (or go at infinity)¹⁰⁰.

Carnot’s book was well-known to these French geometers in the first half of the nineteenth-century, for which the generality of algebra was both a source of interrogation, and something to capture through purely geometrical means. As we have seen, Chasles discussed this book in his lectures at the Faculté de Paris (see 2.1.1), mostly as a precursor of his own principle of signs. Chasles, however, had done away with these concerns of the metaphysics of negative quantities. This was, however, not the case of Poncelet. In a text first published in 1815, then republished (with some modifications) in 1864 in his *Applications d’analyse et de géométrie* Poncelet had analyzed, criticized, and refuted some of Carnot’s views on the status of negative numbers, and, more generally, on the relation between algebraic symbols and geometrical objects.

; c’est donc le second qu’il faut y substituer pour répondre à toute l’étendue de la traduction. Plusieurs algébristes regardent cette généralité comme une richesse de l’Algèbre, qui, disent-ils, répond non seulement à ce qu’on lui demande, mais encore à ce qu’on ne lui demandoit pas, & qu’on ne songeoit pas à lui demander. Pour moi, je ne puis m’empêcher d’avouer que cette richesse prétendue me paroît un inconvénient. Souvent il en résulte qu’une équation monte à un degré beaucoup plus haut qu’elle ne monteroit, si elle ne renfermoit que les seules racines propres à la vraie solution de la question, telle qu’elle est proposée”, [D’Alembert, 1755], p.850. Similar views are expressed in various other entries written by D’Alembert.

⁹⁸On the influence of Locke’s philosophy on the history of algebra in England, see [Durand-Richard, 1990].

⁹⁹This historical episode is studied in [Schubring, 2005].

¹⁰⁰Carnot’s geometry has been described as “an engineering justification of algebra”, in [Gillispie & Pisano, 2014], p.107. See also [Chemla, 1998] for an analysis of the status of generality in Carnot’s geometry.

For Carnot, these symbols, and especially negative quantities, did not represent anything real (and especially not quantities below zero, as Euler thought), but only the difference between two absolute quantities. As a result, Carnot thought, geometrical research based on the algebraic translation of verbal problems, and in particular the use of negative quantities therein, could lead to meaningless solutions being obtained. For Poncelet, however, the transformation of a “énoncé verbal” into a “problème mis en équation” was a purely geometrical operation¹⁰¹; while the obtention of solutions from this problem was purely the result of algebra. Thus, these solutions were inadequate if and only if the translation of the problem had been ill-conducted¹⁰²:

Les solutions soi-disant surabondantes ou étrangères qu'on rencontre en traitant les problèmes géométriques par le calcul, ne viennent donc pas de ce qu'elles ont été amalgamées avec les autres par les transformations algébriques, puisqu'elles appartiennent, en toute rigueur, aux divers systèmes que représentent les équations primitives, mais bien de la manière inexacte, vague ou trop générale dont on a mis le problème en équation.

The same views passed down by D'Alembert to Carnot were also quoted and largely criticized by Poinot in his memoir on the theory of numbers. Poncelet and Poinot, for different reasons, both felt the need to refute the former generation's doubts about the adequacy of algebra as an instrument for geometrical or number-theoretic investigations. Incidentally, this was the context in which the concept of “solution étrangère” had been created: by this term, Poncelet and others denoted these solutions which algebra introduces in a geometrical problem that was not sufficiently well formulated. For De Jonquières, it served to distinguish between two modes of generality, of two equally valid results. For Chasles, however, these solutions were eliminated in the course of the proof (and had nothing to do with algebra, as he detected and removed them in proofs relying solely on the principle of correspondence).

Algebra, for Poncelet as for Poinot and De Jonquières, gives exactly what it is asked, neither less nor more. This was not, for Poncelet, an invitation to drop pure geometry. On the contrary, it meant that algebra was not a science, but merely a “mechanism”, an instrument for the geometers to use but also to control, to subordinate to geometrical understanding and precision. In Poncelet's terms, it was merely a “bâton des aveugles¹⁰³”. Similarly, for Poinot and De Jonquières, this division of labor between algebra on the one hand, and geometry or number-theory on the other hand, was not meant to place all faith in the former's power. Instead, for De Jonquières, it was crucial to have means

¹⁰¹ [Poncelet, 1864], pp.284-285.

¹⁰² [Poncelet, 1864], p.291. Note that the term “solution étrangère” appeared already in 1815.

¹⁰³ [Poncelet, 1864], p.295.

of determining *a priori* whether and when an algebraic formula or result adequately translates the geometrical problem at hand.

Chasles did not react directly to this provocative import of Poncelet and Poinso's separation between the perfect language of algebra and the sometimes vague propositions of geometry, and elected to focus his remarks on the dispute around the invention of the principle of correspondence. It is unclear whether, as De Jonquières would suggest, Chasles felt an unease in the epistemic foundations of his geometry, or whether he simply did not care for this line of defense. And yet, the distinction between algebraic generality and geometrical exactness is a far cry from Chasles' attempt at modeling geometrical discourse on the sort of generality that had made the success of analytical methods. Chasles never doubted the strong analogy between the equations of algebra and the propositions of geometry; he even commented upon it at length, in order to justify the merits of his own methods (see 4.2.4). Through this unsuccessful dialogue, Chasles and De Jonquières were triggering a reconfiguration of the interplay between algebra and geometry. By the end of their dispute, they shared neither an understanding of their respective works, nor a common epistemological framework in which to assess each other's argument, and the exchange degenerated into a public contest, to which few seem to have paid close attention.

Conclusion

Throughout the course of this controversy, De Jonquières transformed his position with respect to Chasles' theory. By the year 1867, not only had he completely changed his mind regarding the exactness and generality of his enumerative formulas: he had modified the very meaning of these epistemic categories. By redefining what it meant for algebraic and geometrical formulae to be exact, De Jonquières had put a new strain on the identity between his index N and Chasles' characteristic μ : the latter, he would claim, carries the intention of counting pure curves, while the index, imbued with the special sort of generality which characterizes algebraic reasoning, is applicable to all curves indistinctively - that is to say, curves understood as equations.

Chasles absolutely rejected this solution to the dispute, and never acknowledged the validity of his students' formula. Intertwined with the social dimension of this dispute, and the sterile clash of Chasles' authority as *académicien* with De Jonquières' self-styling as an innovative *amateur* is the confrontation of two incompatible conceptions of the relation between algebraic forms and geometrical content. This tension, which has roots far back into late 18th-century reflexions on the nature of algebraic symbolism, would explode in the following years, as enumerative problems circulated in the hands of mathematicians equipped with newer conceptual tools, but also epistemic norms and values. To this

circulation shall be devoted the next chapter of this dissertation.

Reorganizations, rewritings, and refutations: the early reception of the theory of characteristics

Introduction

The succession of short papers which Chasles published between 1864 and 1867 in the *Comptes-Rendus de l'Académie des Sciences* ultimately amounted to an unfinished work. Neither the much-expected second volume of Chasles' *Traité des Coniques*, nor his book in preparation on the theory of characteristics for second-order surfaces ever saw print – and in the wake of the Vrain-Lucas affair, Chasles did not return to these projects. While a few synthetic and faithful presentations of Chasles' theory were published, either in Italy by Cremona or in France by Prouhet, their circulation remained limited¹. Even these texts, in fact, altered some important tenets of Chasles' theory; or rather, they displayed new choices made by their authors with regard to what in Chasles' theory ought to be considered most important or interesting. For instance, Chasles' emphasis on the constructive character of his enumeration procedure would be completely lost in these summaries. Thus, while Chasles received praise across Europe for his work, the disparate collections of communications it was gathered in did not invite to the same sort of reading practices as his previous books or extended memoirs.

While readers from various mathematical communities immediately took on Chasles' theory of characteristics, very few displayed an interest in the content of every single one of these communications. Some of them only garnered a few key definitions from the second communication, that is to say the paper wherein systems, conditions, character-

¹ [Cremona, 1864], [Prouhet, 1866].

istics, and modules are defined, and where the general procedure for the enumeration of conics satisfying five conditions is given. Others yet were more interested in the principle of correspondence and the systematic method it offered for proving properties of planar curves. And then there were those who identified Chasles' $\alpha\mu + \beta\nu$ formula as a conjecture of great theoretical importance for algebraic geometry, which remained to be proven. Because the theory of characteristics was scattered across papers, it was easy to isolate elements from it for readers not interested in Chasles' broader argument for the defense of purely geometrical methods. As such, several textual features of Chasles' papers, such as his use of long lists of propositions, would be lost on almost all readers; precisely because such lists seem to make little sense if not viewed against the backdrop of Chasles' understanding of the value of generality. Thus, the description of the theory of characteristics which we gave in chapter 4 must be carefully differentiated from what this theory became in the hands of its readers.

The variety of readings to which a mathematical text may give rise has been the subject of much attention by historians of mathematics in recent years². The unanimous conclusion of this sustained attention has been that even in mathematics, the meaning of a text is not an intrinsic property, but rather a function of its reader - or collectives of readers. The case of Chasles' theory of characteristics is no exception to this rule. In this chapter, however, we set out to study not just readings of Chasles' communications, but active rewritings thereof – sometimes even produced in proximity of Chasles himself, or of his sphere of influence. A remarkable, if not exactly unique, feature of these rewritings is the plasticity with which the notions of the heart of Chasles' theory were 'translated' into other 'mathematical languages'. For instance, the notion of a condition, left somewhat vague in Chasles' texts – if only because it was tacitly borrowed from a historical tradition with which Chasles himself was very familiar, namely the tradition of the construction of curves satisfying certain conditions – would be made algebraic by several authors, and in several different ways. This was either to make its definition more precise, its use more rigorous, or simply to be able to apply the methods of analytic geometry to it.

These rewritings were the work of mathematicians belonging to a variety of professional contexts, ranging from students working toward the local equivalent for a doctoral degree to mathematicians at a more advanced stage of their careers, looking either to incorporate this theory into their well-established mathematical practice or simply to produce a synthetic presentation for their local colleagues. Furthermore, these rewritings were created in epistemic cultures far removed from Chasles'; that is to say, fabricated with very different epistemic values in mind, or even attaching different understandings and practices to the same values. Lastly, but not unrelatedly, these rewritings were produced by actors

²For instance, see [Goldstein, 1995], [Guicciardini, 1999], [Ehrhardt, 2012].

with profoundly different ideas about what was most important to mathematical activity. Whereas proving the generality of the $\alpha\mu + \beta\nu$ formula had been less of a concern for Chasles than the production of lists of propositions exemplifying this very formula, the opposite would be true for others.

It comes therefore as no surprise that such translations, as the adage goes, betrayed, or at least severely altered many features of Chasles' texts. Not only would several of Chasles' textual practices disappear in the process; but several concepts crucial to Chasles' methods would prove difficult to unambiguously carry over to a different mathematical language. While it may seem somewhat innocuous to identify a condition with an algebraic equation over the coefficients of the general equation of the conic, there were much more serious issues besetting those who attempted to find an analytical equivalent to the theory of characteristics. Especially difficult would be the question of reformulating what constitutes a proper solution to an enumerative problem, in particular in the case of degenerate conics. These difficulties culminated in 1876 when Georges Halphen, who had previously taken an active part in the search for algebraic proofs of Chasles' $\alpha\mu + \beta\nu$ formula, suddenly declared it outright false. Degenerate conics, he would claim, had been improperly classified in the context of enumerative geometry; and with his new description of the ways in which a conic could degenerate, he had been able to construct counter-examples to Chasles' formula – that is to say to the very basis of the theory of characteristics. Even the truth-value of a crucial theorem had been altered throughout the translating process.

In order to understand how actors came about adapting Chasles' sparse papers to their own scientific practices, this chapter (and, to an extent, the next one) focuses on the early circulation of the theory of characteristics across Europe. More precisely, in the first three sections of this chapter, we survey the reception of Chasles' theory between 1865 and 1875, discussing its institutional reception and the rewritings produced during this period. The motivation behind this periodization is that, by 1875, two independent proofs for Chasles' $\alpha\mu + \beta\nu$ formula had been given; and several distinct research programmes for the generalization of the theory of characteristics had been fleshed out. In the following year, however, things would take a drastic turn, as Halphen published his analytical rewriting (and, ultimately, refutation) of Chasles' theory. Consequently, the fourth and last section of this chapter focuses on Halphen's memoirs against the $\alpha\mu + \beta\nu$ formula, written between 1876 and 1878.

6.1 The institutional reception of the theory of characteristics

6.1.1 The attribution of the 1865 Copley Medal

Nowhere was the institutional recognition for Chasles' achievements more immediate than at the London Mathematical Society. This recognition, however, was less the unanimous assent of a collective of mathematicians than the outcome of intense lobbying by Thomas Archer Hirst (1830-1892). Hirst was an English geometer who had trained in Marburg between 1850 and 1852, then in Göttingen and Berlin for one more year. During his stay in Germany, he had come to admire above all the synthetic geometry of Steiner, who he had personally befriended. Once his studies complete in April 1853, Hirst would also visit Paris and meet Chasles, the other prominent representative of pure geometry in continental Europe. Upon returning to England, Hirst first began a somewhat unsuccessful and difficult career as a professor of mathematics and physics. His first years as a mathematician was marred by various professional setbacks, but also by the tragic passing of his wife at a young age³. In the early 1860s, however, Hirst was able to secure a more stable form of institutional support: in the same week in November 1864, he was elected to the Council of the Royal Society and as Vice-President of the London Mathematical Society; and in August 1865, he was appointed as Professor of Mathematical Physics at the University College London. From these positions of power, Hirst quickly became an influential member of several of the institutional bodies ruling over the organization of British science⁴. Hirst's push for the recognition of Chasles' recently-published theory came as he was beginning to solidify his position within London-based scientific institutions. In September 1865, Hirst made a communication in front of the Section A (Mathematical and Physical Science) of the Royal Society, of which he would later write in his personal diaries that it "was listened to attentively⁵". In the audience on this day were Plücker himself, but also James Joseph Sylvester, who was equally enthused by Chasles' theory. While Sylvester never contributed to the theory of characteristics, or to enumerative geometry as a whole, he was very much interested by the subject, even

³Anna Hirst, née Martin, died from tuberculosis. When her disease was first diagnosed, the Hirst couple moved to the south of France. Anna Hirst was later buried in Paris, a city which Thomas Hirst then regularly visited over the next few years.

⁴These biographical elements can be found in [Wilson & Gardner, 1993]. Hirst famously kept diaries from the age of fifteen until his death, which are now preserved at the Royal Institution in London. Hirst wrote entries almost every day about his scientific and social life, thus giving us a rare window into the daily activities of a Victorian mathematician. These diaries, totalling about 2850 typewritten folio pages, are available as microfiches (alongside an informative biography) in [Brock & MacLeod, 1980]. In what follows, we will refer to diaries and pages as they can be found in this typewritten edition.

⁵Hirst Diaries, preserved at the Royal Institution in London, Journal XI, entry dated September 17th 1865, p.248.



Thomas Archer Hirst (1830-1892)

submitting questions about it to international journals⁶. Hirst' communication was quick to pay off: Spottiswoode, in the same session, praised Chasles' theory of characteristics in his President's address to said section A, asserting that "we may almost say that [Chasles] has condensed into [one] formula the whole theory of conics⁷".

Bolstered by this initial success, and the rising interest in Chasles' theory amongst British mathematicians, Hirst continued his promotional enterprise. A Council member of the Royal Society, Hirst had a say in the attribution of the Society's annual medals, and in particular the Copley medal. Created in 1731, the Copley medal is one of the oldest honorary rewards in European science⁸. At first meant to celebrate "the best experiment of the year", it was awarded mostly to British chemists and natural philosophers, such as Henry Cavendish (1766), Joseph Priestley (1772), and William Herschel (1781). After a few decades, the medal became a much coveted prize, if only because its awarding took place during a solemn meeting of the Royal Society, whose president then delivered a speech outlining the reasons for the selection of the annual recipient, and led to valuable publicity in the Society's *Philosophical Transactions*. By the 1820s, the medal began to be attributed to non-British scientists, including François Arago (1825), Siméon-Denis Poisson (1832), and Carl Friedrich Gauss (1838), and instead of celebrating experimental results, it rewarded contributions to "natural knowledge⁹". By 1864, the medal had rarely been attributed for research in pure mathematics, with only Edward Waring in 1784 and

⁶For instance, see the question printed immediately after De Jonquières' note for Battaglini's *Giornale*, [de Jonquières, 1865c], p.53.

⁷Hirst's diaries, Journal XI, p.249.

⁸On the early history of the Copley medal, see [Bektas & Crosland, 1992]. For studies in the attribution of this medal in the 1860s, see [Bartholomew, 1976], [Burkhardt, 2001], [Cahan, 2012].

⁹[Bektas & Crosland, 1992], p.52.

James Ivory in 1814 becoming medallists for mathematical contributions¹⁰. For instance, the justification for the attribution of the medal to Poisson mentioned his *Théorie de l'action capillaire*, while Gauss was praised for his mathematical research on magnetism¹¹.

And yet, on November 2nd 1865, when Hirst attended a meeting of the Council of the Royal Society for the adjudication of the Copley medal, mathematicians were very much represented amongst potential nominees. In a previous meeting of the Council, on October 26th, three names had been brought forth, including two mathematicians: French chemist and physicist Henri Victor Regnault by Miller the Treasurer, German mathematician Julius Plücker by Stokes the Secretary, and French mathematician Jean-Victor Poncelet, by Price of Oxford¹². This selection of names had been somewhat disquieting for Hirst, who favored the nomination of Chasles “on the ground not only of a long established reputation but also of a splendid discovery recently made¹³”. This discovery, of course, is the theory of characteristics. With the help of Scottish geologist Sir Roderick Murchison, Hirst was able to add Chasles to the list of potential nominees, and Price eventually withdrew Poncelet’s name from the ballot. To ensure the victory of his candidate, Hirst wrote between October 26th and November 2nd to Cayley, Sylvester, and Henri Smith, asking for letters of recommendation for Chasles’ nomination. On the day of the vote, Hirst used these letters to make a final push for his own nominee¹⁴.

So the Council expected to proceed to the vote without further discussion, I pleaded however for permission to read three letters received since the previous meeting. The permission was granted, and the perusal took the Council by storm. There were 15 members present many had already written on a slip of paper the name of the man for whom they were about to vote, one or two of these papers I saw torn up when my letters were read. A significant silence ensued, broken at last by Miller who remarked that the perusal of these eloquent letters at the last moment put the other candidates at a disadvantage. It became a serious question whether the whole discussion ought not now to be re-opened. It was decided that this should be done.

On the day of the election proper, each Council member who had put forth a candidate proceeded to read supporting statements. Miller began by reading a letter by Sir John

¹⁰Ivory’s work was largely about mechanics and the theory of attraction. Several other medallists worked on mathematical theories with applications to mechanics; however, they were not described as such in the speech of the Royal Society’s president. This is the case for instance of Charles Hutton, who studied the velocity of cannon balls; see [Wardaugh, 2019].

¹¹Bektas and Crosland attribute this to “a general British empirical approach, reinforced by a firm Newtonian tradition, with its emphasis upon experimental and quantitative methods”, [Bektas & Crosland, 1992], p.60.

¹²Plücker and Regnault would both be awarded the medal, in 1866 and 1869 respectively.

¹³Hirst’s diaries, Journal XII, p.2.

¹⁴Hirst’s diaries, Journal XII, p.3.

Herschel in favor of Regnault. But Hirst, according to his retelling of the events, noticed publicly that Miller had intentionally omitted a postscript to the letter. In this postscript, which Miller was then forced to read out by his peers, Herschel allegedly described Regnault's researches as more suitable for the Rumford medal than the Copley medal¹⁵. For Hirst, this forced admission was a crucial victory. Next came Stokes' supporting statement in favor of Plücker, which rested on the argument that the German mathematician had in fact two claims to this award: one for his mathematical research in analytical geometry, and the other for his physical research. This was a strong argument, as knowledge of the natural world was still very much valued above pure mathematics by the Council members of the Royal Society, few of which were mathematicians. Hirst, privately, acknowledged that Stokes' argument "weakened [his] cause"¹⁶. Lastly, Hirst read the letters of Cayley, Sylvester, and Smith, which he would later describe as having been surprisingly effective, even on non-mathematicians.

Surprisingly, the initial vote ended up in a three-way tie, with Chasles, Plücker, and Regnault each collecting five votes. To win over his colleagues, Hirst once more enlisted the help of Sir Roderick Murchison. Murchison had agreed to vote for Chasles after hearing all three recommendation letters, despite not being able to understand the mathematics at play. Hirst then discretely suggested to Murchison, whom he described as his "knightly neighbour", that he express the opinion that seniority be a deciding factor¹⁷. Roderick's timely intervention, Hirst would later write, helped tip the scales in favor of Chasles, especially as it "luckily drew forth a still more influential remark from Prof. Adams of Cambridge to the effect that, independently of seniority, he thought Chasles' claims were highest"¹⁸. A couple of additional members of the Royal Society were brought in to break the tie, and when voting started over, Chasles was elected with 7 votes, against 5 for Regnault and 4 for Plücker.

This victory meant that Chasles' theory of characteristics was even further publicized across British scientific circles, as the Presidential address for the the Anniversary Meeting of the Royal Society, delivered by Sir Edward Sabine on November 30th 1865, and later published in the *Proceedings of the Royal Society*, featured a short and laudatory summary thereof¹⁹. In fact, this summary itself had been largely written by Hirst himself on Sabine's request, as the latter was no mathematician.

While this publicity attests to the existence of widespread interest for Chasles' theory among British mathematicians, few of them actively attempted to investigate this subject-

¹⁵In fact, Regnault had already won the Rumford medal in 1848.

¹⁶Hirst's diaries, Journal XII, p.4.

¹⁷In 1865, Chasles was 72, while Regnault was 55 and Plücker 64.

¹⁸Hirst's diaries, Journal XII, p.6.

¹⁹ [Sabine, 1865].

matter further. Hirst himself did not publish anything on this topic in the years following his electoral triumph, save for a paper on the degenerate forms of conics²⁰. A few years later, Hirst proposed a theory of the correlation between planes which borrows largely from the notations and methods of the theory of characteristics²¹, which does not seem to have circulated a great deal (and as such will not be discussed here). A few syntheses of Chasles' theory were written in English. The Irish mathematician George Salmon would include sections about it in the second and later editions of his *Treatise on the Higher Plane Curves*²², as well as a paper on the characteristics of elementary systems of second-order surfaces²³. Cayley also wrote a long presentation (split across two papers) of Chasles' theory, proposing new notations for it, a “quasi-geometrical” interpretation of its key concepts, and criticizing De Jonquières' earlier enumerative theory²⁴. We shall come back to the content of these papers below (see 6.3.1). It suffices to say for now that Cayley's interpretation, however influential it would prove to be (in particular for Italian algebraic geometers in the 1880s), was not aimed at pursuing the theory of characteristics for its own sake, but rather at showing that it could be incorporated into the framework of the so-called “abstract m -dimensional geometry” Cayley was developing at the time²⁵. Later on, Cayley would never go back to enumerative methods, only following from a distance the disputes triggered in the wake of Halphen's refutation of Chasles' formula²⁶. Beyond these few authors, there seems to have been no significant engagement with Chasles' theory amongst British mathematicians despite the Copley medal celebrating it in 1865.

²⁰ [Hirst, 1866]. In this paper, Hirst discusses the third mode of degeneration for conics, whereby a conic collapses into one straight line and one point. We shall come back to this kind of conic in sections 6.4.3, 7.3.3.

²¹ [Hirst, 1873].

²² [Salmon, 1873], pp.360-373. Note that Salmon would only include a much shorter account of the theory of characteristics in editions of his *Treatise on Conics* posterior to 1864, and this account is only part of the end notes (and not of the body text), see for instance [Salmon, 1869], pp.368-370. This might have to do with the fact that the theory of characteristics was considered as too advanced to be included in this elementary exposition of the geometry of conics. However, Wilhelm Fiedler, in his *Analytische Geometrie der Kegelschnitte nach Salmon* did devote the entirety of the 25th and last chapter to it, see [Fiedler, 1866], pp.571-587.

²³ [Salmon, 1867b].

²⁴ [Cayley, 1868a], [Cayley, 1868b].

²⁵ [Cayley, 1870].

²⁶ In fact, Cayley had been interested in Chasles' publications at an earlier stage: in 1866, he had begun working on the principle of correspondence, which he sought to generalize by considering correspondences not on a straight line L , but on a curve of “deficiency” (*genus*) D . A correspondence (α, β) on such a curve, he had shown, had $\alpha + \beta + 2kD$ coinciding points, where k is a number to be found on a case by case basis. On this topic, Cayley sent short notes to Chasles to be read before the Paris Académie, and wrote short papers for the London Mathematical Society; see [Cayley, 1866a], [Cayley, 1866c]. This extension of the principle of correspondence would later be taken up by Brill, among others; see [Brill, 1873].

6.1.2 Doctoral dissertations on the theory of characteristics

Another facet of the international reception of the theory of characteristics, which had a more profound and lasting impact on the development of this theory, was the fact that it became the subject of a few doctoral dissertations, and in different countries²⁷. Three such dissertations will be of particular interest to us.

The first one is that of the Danish mathematician (and historian of mathematics) Hieronymus Zeuthen. The son of a protestant minister, Zeuthen entered the University of Copenhagen in 1857, where he studied mathematics²⁸. After graduating in 1862 *cum laude*, he became particularly interested in geometry, writing a short essay on “geometrical methods of transformation”, and obtaining a grant the following year to travel to Paris and study under Chasles’ supervision. A privileged observer of the initial publication of the theory of characteristics, Zeuthen was forced to an early return to Denmark in April 1864 due to the outbreak of the Second Schleswig War with Prussia and Austria. In 1865, back in Copenhagen, Zeuthen wrote a dissertation in Danish on the theory of characteristics, entitled “Nyt Bidrag til Læren om Systemer af Keglesnit, der ere underkastede 4 Betingelser²⁹” (*New contribution to the theory of systems of conics satisfying 4 conditions*), and submitted it on October 22nd. He then translated into French large parts of this dissertation, and had them published in the *Nouvelles Annales*³⁰. With these publications, Zeuthen became one of the very first mathematicians to produce extensive research on the theory of characteristics, besides Chasles, De Jonquières, and Cremona. Throughout his long career, Zeuthen would maintain a constant engagement with this topic, and with enumerative geometry at large. He would eventually be recognized as a leading expert in this field, with for instance Klein and Meyer tasking him with writing an entry on “enumerative methods” for their *Encyklopädie*³¹. Another dissertation on the theory of characteristics written by someone who would devote his entire scientific career to enumerative geometry is that of the German mathematician Hermann Schubert (1848-1911). In fact, this dissertation, written in 1870, would be the first contribution to the theory of characteristics written in German (as far as we can tell). Indeed, due to Chasles’ personal and scientific proximities with mathematicians based in both Italy and England, the reception of the theory of characteristics was relatively fast in these

²⁷Of course, there was no unified higher-education system across European countries at this time, and to speak of ‘doctoral dissertation’ is an abuse of language. However, for the sake of brevity, we almagate through this term the *Habilitationsschrift*, the *Doctorat*, and other similar institutions. On these questions, see [Rüegg, 2004], especially pp.37-72.

²⁸For biographical information on Zeuthen’s early years, see his autobiography, [Zeuthen, 1866b], as well as [Kleiman, 1991].

²⁹[Zeuthen, 1865].

³⁰[Zeuthen, 1866a].

³¹[Zeuthen, 1905].



Hieronymus Zeuthen (1839-1920)

countries, with faithful translations and summaries being produced as early as 1865. In Germany, however, the uptake of Chasles' new discoveries was much slower.

The circumstances which led Schubert to this particular topic for his dissertation are rather unclear. Schubert had studied physics and mathematics at the University of Berlin between 1867 and 1870, attending the seminars of Kummer and Kronecker. In parallel, he was actively studying with the German physicist Gustav Magnus, under whose supervision he intended to write a doctoral dissertation³². When Magnus died in April 1870, Schubert had to transfer to the University of Halle, where he obtained his doctoral habilitation under Eduard Heine. Heine knew nothing of Chasles' theory until reading Schubert's dissertation, whose worth he did not feel capable of fully assessing³³. He nonetheless wrote a positive review, and the dissertation, once defended, was immediately published in Crelle's *Journal*³⁴. After a three-year hiatus between 1871 and 1874, Schubert went back to the theory of characteristics, and became one of the most active authors on this topic over the course of his long career³⁵. Lastly, at least one dissertation was defended in France in the years immediately following Chasles' communications. Its author was a young *normalien* named Sylèvre Nicolas Maillard (1845-19??). After taking the Agrégation in 1867, he defended in December 1871 a dissertation at the Faculté des Sciences de Paris on the theory of characteristics for cubics. Despite the fact that Chasles was still a professor at the Faculté (and was very much alive and at work, his old age notwithstanding), Serret is listed as having presided over the commission which examined this dissertation³⁶. The

³² [Bureau & Renschuch, 1966].

³³ [Bureau, 1993], p.64.

³⁴ [Schubert, 1870]. The following year, Schubert would publish a brief note in Crelle's *Journal* to acknowledge Zeuthen's priority regarding some of the results presented in his dissertation, see [Schubert, 1871].

³⁵ Schubert's enumerative geometry is the subject of the next chapter of this dissertation.

³⁶ The external reviewers, or *examineurs*, were Briot and Ossian-Bonnet; [Maillard, 1871].



Hermann Schubert (1848-1911)

next year, Maillard was hired as a *chargé de cours* at the Université de Poitiers, where he spent the rest of his career, seemingly not continuing on with his research on the theory of characteristics³⁷.

We shall come back to the content of these three dissertations shortly, and discuss how they all propose to reorganize the theory of characteristics whilst still accepting the main tenets of the research programme laid out by Chasles (see 6.2).

6.1.3 The Société Mathématique de France: a center for the development of the theory of characteristics

Beyond Maillard, there were several other French mathematicians who, in the early 1870s, published on the theory of characteristics. One of the first to contribute to this theory was George-Henri Halphen (1844-1889), who graduated from the Ecole Polytechnique in 1862 and pursued a military career (as an artillery officer) alongside his mathematical research. Between 1869 and 1872, he published in the *Comptes-Rendus de l'Académie des Sciences*, with the help of his professor Joseph Bertrand and of Chasles himself, several papers which extended the notion of characteristics to straight lines in space³⁸. Halphen's theory of straight lines in space is not exactly analogous to the theory of characteristics for conics, but its central questions and the notations are similar to what Chasles had been doing. For instance, in the first paper of this series, Halphen sets out to count how many straight lines in space satisfy simultaneously two pairs of conditions³⁹. To that

³⁷What little is known of Maillard's life can be found in [Huguet & Noguès, 2011].

³⁸ [Halphen, 1869], [Halphen, 1871], [Halphen, 1872], [Halphen, 1873a].

³⁹The parametric equation of a straight line in space involves four coefficients; therefore, four (simple) conditions determine a finite number of straight lines.

end, he defines the characteristics μ, ν of a pair of conditions as, respectively, the number of straight lines in a given plane satisfying the pair of conditions, and the number of straight lines passing through a given point and satisfying the pair of conditions. Given two pairs of conditions, whose characteristics are respectively μ, ν and μ_1, ν_1 , Halphen's central result states that the number of straight lines satisfying both pairs of conditions is

$$\mu\mu_1 + \nu\nu_1$$

This is obviously reminiscent of Chasles' $\alpha\mu + \beta\nu$ theorem. However, no equivalent to the notion of systems of curves is provided by Halphen, nor are these characteristics derived from 1-parameter families of figures.



Georges Henri Halphen (1844-1889)

Other mathematicians who contributed to Chasles' theory include Louis Saltel and Georges Fouret⁴⁰. However, the papers published by Fouret and Saltel quickly diverged from the research programme initiated by Chasles, with for instance Fouret introducing transcendental curves and differential analysis into the picture, by showing that all systems of curves (algebraic or transcendent) having the same characteristics satisfy one differential equation⁴¹.

The case of Saltel is particularly interesting in this regard. Saltel, born on May 7th 1847 in Espalion, obtained a doctorate in mathematics from the university of Nancy in 1877. His thesis, and much of his subsequent work, bears on what he called 'the analytical principle of correspondence' – which he thought to be a more general version of Chasles'

⁴⁰Little is known regarding the life of Fouret, who replaced Poincaré as president of the Société Mathématique de France in 1887, having by then moved on to research interests unrelated to the theory of characteristics.

⁴¹ [Fouret, 1873a], [Fouret, 1873b].

principle. Earlier, he had proposed a method for obtention of the characteristics of elementary systems of curves of order m , relying on what he called the “principe arguésien unicursal⁴²”. In 1876, Saltel began to raise doubts over the validity of the theory of characteristics. In particular, he attacked the way *solutions étrangères* were being counted and published various ‘proofs’ of the falsity of Chasles’ theory, mostly in the *Bulletin de l’Académie Royale des Sciences de Belgique*, with the help of Catalan⁴³. Despite these attacks seemingly coinciding in timing and direction with those of Halphen, the latter did not take Saltel’s work seriously. Indeed, at various points of their private correspondence, Halphen and Zeuthen make clear their complete disregard for these publications, which they never mention in their respective publications. From 1882 onwards, Saltel suffered from mental health issues, and would have several stays in a sanatorium in Armentières, after which his scientific output was all but halted⁴⁴.

These young French geometers all met and interacted at the Société Mathématique de France (SMF) in Paris, which had been created in 1872. Chasles himself presided over this society for the first year of its existence; after which he remained honorary president until his death in 1880. The Société in fact had been created partly after his own lament, expressed in his 1870 *Rapport*, that French mathematics was doomed to lag behind their German, English, or Italian counterparts lest such a society be created immediately⁴⁵. A mathematical journal, namely the *Bulletin de la Société Mathématique de France*, was immediately associated to this Society. In the first volumes of the *Bulletin*, the theory of characteristics is much more represented than in any other European mathematics journal⁴⁶, largely due to the contributions of Halphen, Saltel, and Fouret. In fact, reports on the scientific life of the Société and on the matters discussed during its bi-weekly meetings, also published in the *Bulletin*, attest to the constant presence of discussions pertaining to Chasles’ theory within the walls of this institution throughout the 1870s. Ferdinand von Lindemann, a German mathematician who had been editing and publishing the *Vorlesungen* of Alfred Clebsch after the latter’s death, would also attend and participate to some of these meetings⁴⁷. Both he and Clebsch had worked on this topic in previous years, as

⁴² [Saltel, 1873]. This principle, in a nutshell, states that “if a plane curve has three multiple points whose orders add up to a number higher than the curve’s order, then the properties of this curve can be reduced to the properties of a curve of lower order”.

⁴³For instance, [Saltel, 1876].

⁴⁴ [Huguet & Noguès, 2011].

⁴⁵ [Chasles, 1870], pp.378-379. On the early history of this society, see [Gispert, 1991].

⁴⁶Of course, at the same time, the *Comptes-Rendus de l’Académie de Paris*, while not properly speaking a mathematics journal, would also publish a large number of papers on the theory of characteristics, in large part due to Chasles’ impressive output between 1870 and 1876.

⁴⁷See for instance [SMF, 1877], wherein the importance of the theory of characteristics as a subject of discussion for the mathematicians of the Société Mathématique de France appears clearly. Note that, beyond the aforementioned authors, Jordan and Darboux also seemingly took part in such discussions, despite not having published on the theory of characteristics.

we shall see below (see 6.3.2). Furthermore, Schubert and Zeuthen, two of the most active authors on the theory of characteristics outside of France, would engage with the Société early on, with Schubert becoming a foreign member in 1876 with Halphen’s help⁴⁸, and Zeuthen in 1881. When, toward the end of the year 1879, Halphen and Schubert feuded over the validity of some of the latter’s enumerative results, they ultimately decided to resolve their dispute via public notes published in the *Bulletin*⁴⁹. Thus, throughout the 1870s, the SMF would serve as a nodal point for (sometimes polemical) reception, diffusion, and circulation of the theory of characteristics.

6.2 The reorganization(s) of a theory

Having discussed the institutional reception of the theory of characteristics, we now return to the three aforementioned dissertations written about it between 1865 and 1871. These three dissertations, much more than the surveys previously evoked, show a great deal of familiarity with Chasles’ methods and research programme for the theory of characteristics. However, while all three accept this programme to a certain extent, they also propose to reorganize the theory of characteristics, that is to say to alter its presentation, its notations, or some of its computational techniques. We present these dissertations in chronological order; however, they do not cross-reference, and the reorganizations they put forth are unrelated for the most part.

6.2.1 Zeuthen’s 1865 dissertation

Zeuthen’s dissertation is divided into three sections, the first of which introduces the general problem of enumerating conics satisfying five conditions, and surveys efforts in that direction prior to Chasles’ theory of characteristics⁵⁰. In particular, Zeuthen briefly discusses Bischoff’s enumerative formulae, and De Jonquières’ theory of plane curves. Written in the first half of 1865, this dissertation predates De Jonquières’ three notes from Saïgon, and his public dispute with Chasles⁵¹. As such, it features criticisms of Bischoff’s and De Jonquières’ formulas and theories similar to those expressed Chasles in his 1862 report for the Grand Prix de Mathématiques⁵². In particular, the theory of characteristics is thus presented as solving the “difficulties” (*Vanskeligher*) which the occasional presence of exceptional conics brought into De Jonquières’ formulas, and is said to reestablish the duality which sorely lacked in them. For Zeuthen, “the main point

⁴⁸We come back to the circumstances of Schubert’s election, see 8.2.1.

⁴⁹On this dispute, see 8.2.2.

⁵⁰[Zeuthen, 1865], pp.1-16.

⁵¹See 4.1.

⁵²[Zeuthen, 1865], pp.9-10.

of Chasles' method, to which he also attaches the utmost importance, is the successive introduction of the individual conditions by which a conic must be determined, and he solves his task by starting from the elementary systems and then introducing the given conditions one by one⁵³. The rest of Zeuthen's dissertation consists in "a new method for the determination of the characteristics of a system of conics⁵⁴", which does not proceed by Chasles' method of substitution⁵⁵. This method is exposed in the second section of the dissertation, while applications to curves of order m and class n are discussed in a shorter, third section⁵⁶. The second section would be translated into French by Zeuthen himself, and published in 1866 across five papers for the *Nouvelles Annales*⁵⁷. We shall now summarize it, relying on this later publication in French.

The starting point of Zeuthen's method is to rewrite Chasles' characteristics as functions of the two numbers of degenerate conics in a system of conics. Chasles had shown that, in a system of characteristics (μ, ν) , there were always $\lambda = 2\mu - \nu$ point-pairs (or *coniques infiniment aplaties*) and $\tilde{\omega} = 2\nu - \mu$ line-pairs⁵⁸ (or *coniques représentées par deux droites*). Therefore, as Zeuthen noticed, the characteristics could be defined through these two other numbers⁵⁹:

$$\begin{aligned}\mu &= \frac{1}{3}(2\lambda + \tilde{\omega}) \\ \nu &= \frac{1}{3}(2\tilde{\omega} + \lambda)\end{aligned}$$

⁵³"Hovedpunktet i Chasles's Methode, som han ogsaa selv tillægger størst Betydning, er den successive Indførelse af de enkelte Betingelser, hvorved et Keglesnit skal bestemmes, og han løser sin Opgave ved at gaae ud fra de elementaire Systemer og derpaa indføre de givne Betingelser en for en", [Zeuthen, 1865], p.15.

⁵⁴ [Zeuthen, 1865], p.16.

⁵⁵On Chasles' method, see 4.2.1.

⁵⁶ [Zeuthen, 1865], pp.17-87 and 88-97 respectively.

⁵⁷ [Zeuthen, 1866a].

⁵⁸Remember that point-pairs are defined by Chasles (and, subsequently, by Zeuthen), as conics composed of one straight line and two points on it. Conversely, line-pairs are composed of two straight lines and one point on them. Conics are curves of order and class 2, therefore they intersect every straight line in two points, and from every point, two tangents to them can be drawn. Since usual definitions of intersections and tangencies become problematic when applied to such degenerate curves, new definitions of intersections and tangencies must be stipulated for these properties to remain valid. The straight line composing a point-pair is in fact two coinciding straight lines, therefore the intersection of a straight line and a point-pair is a double point (potentially at infinity). From a given point, one draws two tangents to the point-pair by joining it to the two points. These are tangents because they only intersect the degenerate conic at a single point. Similarly, any straight line intersects a line-pair at two points because it intersects each one of the two straight lines composing it at one point (including if it passes through the point composing the line-pair, which is at the intersection of the two straight lines); and from a fixed point, one draws two coinciding tangents to the line-pair by joining it to the point composing the pair. That is to say, because the point of a line-pair is a double point, one can draw the straight line to it twice, thus drawing two tangent lines. See 4.2.5 for more details.

⁵⁹The realization that one can study the properties of systems of conics by looking at its degenerate elements rather than its characteristics would be extremely influential on Schubert, see 7.2.3, 7.2.4.

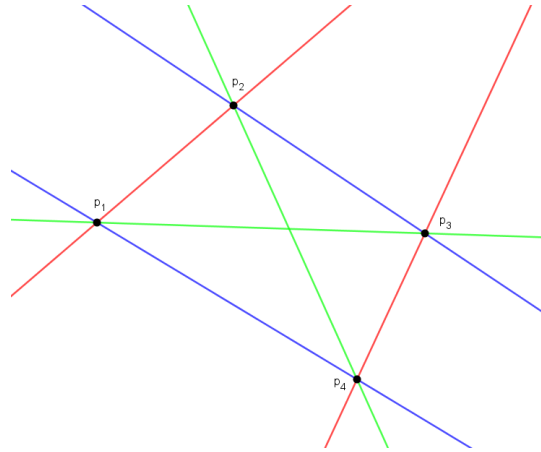
To determine the values of λ and $\tilde{\omega}$ in a given system is, therefore, enough to determine its characteristics. This task, Zeuthen explained, could be decomposed into three sub-tasks⁶⁰:

Il s'agit de trouver: 1° sur quelles droites sont situées les coniques infiniment aplaties, et quels points sont doubles dans les autres coniques exceptionnelles ; 2° quelles coniques singulières de la première espèce sont situées sur chacune des droites trouvées, et quelles coniques singulières de la seconde espèce ont à chacun des points trouvés un point double ; 3° *combien de fois chacune des coniques singulières qu'on a trouvées est comptée dans les nombres λ et $\tilde{\omega}$.*

This third task, according to Zeuthen, is where the real difficulties lie. To show how these numbers could be determined, Zeuthen first treated the case of elementary systems, whose characteristics were of course already well-known. However, in so doing, Zeuthen did more than prove once again the values for the numbers of conics satisfying five elementary conditions: he also obtained a description of the degenerate conics which satisfy them, and of their respective multiplicities.

In the case of the sheaf of conics, that is to say the system of conics defined by four points p_1, \dots, p_4 , Zeuthen first notes that there cannot be any point-pair in such a system, and therefore that $\lambda' = 0$ (where the dash indicates that we are referring to numbers pertaining to this first elementary system). Indeed, a point-pair is formed of only one (double) straight line, which cannot go through all four points in general. To enumerate line-pairs, Zeuthen notes that one can only draw three pairs of lines going through all four points (see fig. below). From this, Zeuthen concludes that $\tilde{\omega}' = 3x$, where x is a positive, non-zero integer: x represents how many times each of these exceptional conics ought to be counted. From the system of equations above, Zeuthen obtains $\mu' = x$, and $\nu' = 2x$. Since $\mu' = 1$ (as five points determine uniquely a conic), $x = 1$ and therefore $\nu' = 2$. Note that two different conclusions are drawn by Zeuthen, in keeping with the sub-tasks identified above: from the equation $x = 1$, Zeuthen knows that each line-pair in this system is to be counted once, and from the equation $\nu' = 2x$, he knows that there are two such line-pairs, irrespectively of their multiplicity.

⁶⁰ [Zeuthen, 1866a], p.242.



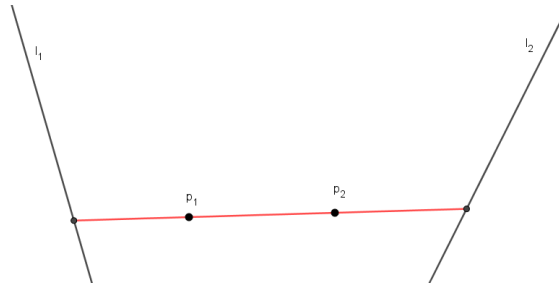
In the case of a system of conics defined by three points p_1, p_2, p_3 and a straight line l , the same reasoning as above allows Zeuthen to write $\lambda'' = 0$. A line-pair in this system must pass through all three points and touch the line l . If one of the lines in the pair goes through p_1 and p_2 , the other line must go through p_3 and the intersection point o of l and the line p_1p_2 . There are three such line-pairs, Zeuthen notes⁶¹; as a result, he writes $\tilde{\omega}'' = 3y$, and obtains the equations $\mu'' = y$, $\nu'' = 2y$. Furthermore, $\mu'' = \nu' = 2$, as μ'' and ν' both stand for the numbers of conics passing through four points and touching one straight line. Substituting these numbers in the equations given above, Zeuthen concludes that $y = 2$ and that $\nu'' = 4$. This means that every line-pair passing through three given points and determined by one given straight line ought to be counted twice, resulting in a total of four degenerate conics of this kind being present in the system.

In the case of a system of conics defined by two points p_1, p_2 and two straight lines l_1, l_2 , Zeuthen observes that there is only one way to form a line-pair, namely by joining the intersection point o of l_1 and l_2 to p_1 and p_2 respectively. Furthermore, there is only one way to form a point-pair, by forming the straight line joining the two points p_1 and p_2 , and comprised between the points where the given lines touch⁶² (see fig. below). As a result, $\lambda''' = 1 \cdot z$, and $\tilde{\omega}''' = 1 \cdot u$. Zeuthen then notes that “by transforming this system via the principle of duality, one obtains a system of a similar kind⁶³”, and concludes that $\lambda''' = \tilde{\omega}'''$, therefore $z = u$, and $\mu''' = \nu''' = z = u$. Since $\mu''' = \nu'''$, this system is of characteristics $(4, 4)$, and the point-pairs and line-pairs which pass through two points and are limited by two straight lines must each be counted four times.

⁶¹Given three points p_1, p_2, p_3 , there are exactly three pairs of straight lines joining two of these points, such that by every point passes at least one straight line: (p_1p_2, p_1p_3) , (p_1p_2, p_2p_3) , (p_1p_3, p_2p_3) .

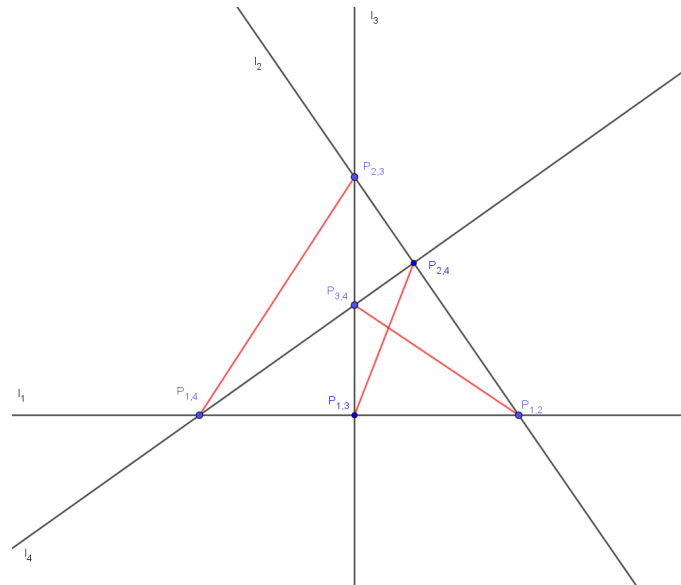
⁶²A point-pair can either be thought of as a straight line and two points on this line, or, as Zeuthen tends to prefer, a straight line limited by two points. One can visualize this as an ellipse collapsing onto the straight line joining its foci.

⁶³ [Zeuthen, 1866a], p.244.



In the system defined by one point p_1 and three straight lines l_1, l_2, l_3 , there are no line-pairs. Indeed, the point defining such a degenerate conic could not possibly be on all three given lines⁶⁴. There are three point-pairs, which are all composed of the straight line joining p and one of the l_i 's, and of its intersections with the other two l_i 's. Therefore, $\lambda^{iv} = 3v, \tilde{\omega}^{iv} = 0$, and, using the relations between characteristics and degenerate conics, $\mu^{iv} = 2v, \nu^{iv} = v$. Since $\mu^{iv} = 4$, Zeuthen finds that $v = 2$: point-pairs defined by three points and one straight line must be counted twice, which means that there are six in this system (whose characteristics can now be computed, and are $(4, 2)$).

Lastly, in the system defined by four straight lines l_1, l_2, l_3, l_4 , there cannot be point-pairs for the same reason as in the previous case. There are three such point-pairs (see fig. below), obtained by joining two-by-two the intersections of two of the given straight lines. In the same manner as before, Zeuthen obtains $\lambda^v = 3s, \tilde{\omega}^v = 0$, and therefore $\mu^v = 2s, \nu^v = s$. Since $\mu^v = 2$, this means that $s = 1$, and point-pairs defined by four straight lines must be counted once (and the characteristics of this system are $(2, 1)$).



At this point, not only has Zeuthen obtained the characteristics of all elementary systems in a new manner, but he also obtained a list of prescriptions regarding the counting of

⁶⁴This is the dual statement to the proposition that there cannot be a point-pair in a system defined by three points, because the straight line defining such a point-pair could not possibly pass through all three points.

degenerate conics defined by elementary conditions, which will prove useful beyond the case of elementary systems. Indeed, these elementary systems being dealt with, Zeuthen moves on to the *locus classicus* of enumerative problems at that time, namely contact problems⁶⁵. In order to find the characteristics of a system of conics defined by four contact conditions, Zeuthen suggests “regarding the motion of a variable conic which continuously touches a given curve at will as composed of a sequence of rotations about successive points of contact, or as a sequence of slides (glissements) on the tangents at these points⁶⁶”. A system of conics defined by four contact conditions is viewed as a mobile conic which rotates about the points where tangencies are demanded, and whose tangents slide along the stipulated lines. This is particularly useful, because the degenerate conics in such a system are then the degenerate conics defined by i points and $4 - i$ straight lines, whose multiplicities as elements of the system (or, alternatively, as ‘elements’ of the variable conic) have been computed at the same time as the characteristics of elementary systems.

This, Zeuthen claims, showed that the numbers of exceptional conics in the system determined by four given curves depend only on the instantaneous variation of a conic which, at any given time, satisfies elementary conditions. In particular, the multiplicity of the special conics present in the system determined by four curves could be known *a priori* thanks to the results outlined above. For instance, explains Zeuthen⁶⁷,

On doit compter:

Dans le nombre λ relatif à un système de coniques qui touchent quatre courbes données : *une seule fois*, toute conique infiniment aplatie joignant deux points où les quatre courbes se rencontrent deux à deux et limitées à ces points ; *deux fois*, toute conique infiniment aplatie touchant une courbe donnée, passant par un point de rencontre de deux autres courbes données et limitées à ce point et à la quatrième courbe [...]

Et dans le nombre $\tilde{\omega}$ relatif au même système : *une fois*, toute conique sin-

⁶⁵By contact problem, we mean the enumeration of conics satisfying four contact conditions. A contact condition is a condition of the form ‘to have a contact of order r with a given curve of order m and of class n ’.

⁶⁶“On peut regarder le mouvement d’une conique variable qui touche continuellement une courbe donnée à volonté, comme composé d’une suite de rotations autour de points successifs de contact, ou comme une suite de glissements, sur des tangentes en ces points”, [Zeuthen, 1866a], pp.246-247.

⁶⁷“We must count: in the number λ , relative to a system of conics touching four given curves : *only one time*; every point-pair conic joining two points where the four curves intersect two by two and limited by these points ; *two times* every point-pair touching a given curve, passing through an intersection point of two other given curves, and limited at this point and the fourth curve [...] and, in the number $\tilde{\omega}$ relative to the same system : *once*, every singular conic composed of a pair of straight lines, of which one touches two given curves, the other one the other two curves ; *twice*, every conic with a double point at one of the points where a given curve meets a common tangent to two other given curves, and composed of this line and a tangent going through the double point of the fourth curve”, [Zeuthen, 1866a], p.247.

gulière composée d'un couple de droites dont l'une touche les deux courbes données, l'autre les deux autres ; *deux fois*, toute conique ayant un point double à l'un des points où une courbe donnée rencontre une tangente commune à deux autres, et composée de cette droite et d'une tangente par le point double à la quatrième courbe [...].

Considering a general system defined by four conditions of the form ‘to touch a given curve’, Zeuthen examines the various ways for degenerate conics to appear. Four curves C_1, C_2, C_3, C_4 being given, consider one point p_1 at the intersection of C_1 and C_2 , and one point p_2 at the intersection of C_3 and C_4 . Drawing a straight line l joining these two points, it appears that the point-pair composed of l and (p_1, p_2) satisfies all four contact conditions. From the preliminary groundwork described earlier, Zeuthen knows already that this degenerate conic will count once, because it is a point-pair defined by four *glissements* (or four straight-lines), and the study of the fifth elementary system had yielded for such conics the multiplicity $s = 1$. Similarly, a point-pair can be formed by taking one point at a point p_1 at the intersection of C_1 and C_2 , drawing a tangent l from p_1 to C_3 , and taking one point p_2 at the intersection of l and C_4 . There again, one can check that, with the extended notions of incidences and tangencies for degenerate conics, the point-pair (p_1, p_2) satisfies the four contact conditions. This time, however, it must be counted twice, because it is defined by three *glissements* and one *contact*, and the study of the fourth elementary system had yielded the multiplicity $v = 2$.

Of course, given four curves in the plane, there are several degenerate conics which can be thusly generated, irrespective of their multiplicity. But these numbers can be computed simply using Bézout’s theorem and its analog for tangencies. To express this result more generally, Zeuthen then lets $C_{m,n}$ denote the condition of touching a curve⁶⁸ of order m and class n . Zeuthen then proceeds to enumerate the exceptional conics of each kind in the system $(C_{m_1, n_1}, C_{m_2, n_2}, C_{m_3, n_3}, C_{m_4, n_4})$. To that end, Zeuthen first recalls that two curves of order m_1 and m_2 , and of class n_1 and n_2 , have $m_1 m_2$ intersection points and $n_1 n_2$ common tangents. As a result, Zeuthen claims that, for instance, there are

$$m_1 m_2 \cdot m_3 m_4 + m_1 m_3 \cdot m_2 m_4 + m_1 m_4 \cdot m_2 m_3 = 3 m_1 m_2 m_3 m_4$$

point-pairs of the first kind, that is to say which join two points where the four curves

⁶⁸In fact, Zeuthen also introduces the numbers d of double points, d' of cusps, t of bitangents, and t' of stationary tangents. All these numbers are linked by Plücker’s formulae, with which Zeuthen was very familiar. Zeuthen refers to Plücker in the original 1835 German edition of the *System der analytischen Geometrie*, but he also had studied extensively Salmon’s textbooks, see [Kleiman, 1991], p.4. These numbers play a role in later parts of Zeuthen’s memoir, which we shall not discuss here. It is easy to imagine how they might play a role in the fine-tuned determination of the conics satisfying more specific contact conditions.

meet two by two, and comprised between these points. Indeed, while Zeuthen gives no further explanation of this formula, to form such a point-pair, one must select two points, which will be on all four curves. As a result, one point will be at the intersection of two of the four curves, and the other point at the intersection of the two other curves. Each factor in the left-hand term represents a possible choice for the two first curves. There are, in the first case (one point on C_1 and C_2 , the other on C_3 and C_4), m_1m_2 possible choices for the first point, and m_3m_4 for the second point; hence the result.

Similarly, Zeuthen produces the number of special conics of each kind, and adds them to obtain expressions for λ and $\tilde{\omega}$ (taking into account multiplicities), from which he obtains the characteristics of the entire system defined by the four conditions C_{m_i, n_i} . Zeuthen goes on to obtain the characteristics of systems defined by more complicated contact conditions, for instance imposing on conics to have contacts of a certain order with a given curve, or that the contact be at a given point. While the method is the same, Plücker's formulae are required in these more delicate cases.

Furthermore, Zeuthen introduces operations on conditions. For instance, denoting the condition 'touching the curve $C_{m,n}$ at a given point' as the juxtaposition $C_{m,n}\theta$, and $C_{m,n} - C_{m',n'}$ the condition 'touching the curves $C_{m,n}$ and $C_{m',n'}$ at different points', Zeuthen showed that for any three conditions Z_1, Z_2, Z_3 , there held an equation between the numbers of conics satisfying the following sets of conditions⁶⁹:

$$N(C_{m,n}, C_{m',n'}, Z_1, Z_2, Z_3) = 2N(C_{m,n}\theta, Z_1, Z_2, Z_3) + N(C_{m,n} - C_{m',n'}, Z_1, Z_2, Z_3)$$

This was not turned by Zeuthen into a more general computational system on conditions, nor where such equations combined into an algebra of conditions. Rather, these formal equations were useful insofar as Zeuthen was able to find relations between the special conics of each of these systems. For instance, the special conics of $(C_{m,n}\theta, Z_1, Z_2)$, Zeuthen claimed, are the limits to which tend some of the special conics of $(C_{m,n}, C_{m',n'}, Z_1, Z_2)$. As such, Zeuthen was able to provide a method to count the special conics of systems involving complex conditions (such as $C_{m,n}\theta$) via the special conics of the general system involving basic contact conditions, in the same way as the special conics of elementary systems had served to enumerate the special conics of systems of conics satisfying contact conditions.

Zeuthen's dissertation, and the series of papers derived from it for the *Nouvelles Annales*, were written as Chasles was publishing his own theory: as a result, it does not follow precisely the program Chasles would lay out for further research, namely the determination of characteristics of elementary systems for curves of higher degree (or for

⁶⁹ [Zeuthen, 1866a], p.291.

surfaces). Instead, Zeuthen focuses his effort on the creation of an alternative method for the obtention of characteristics of systems of conics, and set out to fully solve a problem which had occupied earlier authors, especially Bischoff and De Jonquières: that of enumerating conics satisfying five contact conditions. Zeuthen, however, fully accepted Chasles' criticism of these predecessors' formulae, and his method explicitly purports to import the merits of the theory of characteristics into this general problem. In order to do so, Zeuthen had made degenerate conics the cornerstone of his enumerative method, instead of the characteristics of a system, thus simply but profoundly reorganizing Chasles' theory.

6.2.2 Schubert's 1870 dissertation

Schubert's dissertation aimed mostly to summarize and expand on Chasles' theory of characteristics for systems of surfaces, which, as we saw earlier, are characterized not by two but by three characteristic numbers⁷⁰. In a system of surfaces, μ denotes the number of surfaces passing through any given point, ν that of surfaces touching any given line, and ρ that of surfaces touching any given plane. Schubert asserts that it has been “observed” (*beobachtet*) that for any condition Z , in any system of surfaces (μ, ν, ρ) , the number of surfaces satisfying Z is $\alpha\mu + \beta\nu + \gamma\rho$, where α, β, γ depend only on Z . These three numbers are called the “parameter” (*Parameter*) of a condition by Schubert, who thus replaces Chasles' *modules*. Schubert makes no attempt to prove this theorem in this paper, nor does he suggest that it should or could be done. However, he immediately proves a few results that were merely stated in Chasles' and De Jonquières' papers: in particular, the parameter of the condition ‘touching a given surface of order m ’. In order to prove this latter result on surfaces, Schubert first proves another related result only stated by Chasles⁷¹, namely the fact that in a system (μ, ν) of curves of order n , there are $m[(m-1)\mu + \nu]$ curves which touch another given curve of order m . The way in which Schubert conducts his proof is interesting, if only because it departs from the methods employed by Chasles and De Jonquières. In lieu of correspondences, Schubert elects to prove these results directly from geometrical considerations, showing familiarity with synthetic geometry (even though no reference is given to indicate where this knowledge was acquired).

To compute the module (or parameters) of the contact condition stated above, Schubert considers a curve f of order m in the plane of a system (μ, ν) of curves of order n . He then proposes to determine the order of the locus generated by the point p such that

⁷⁰See 4.2.6.

⁷¹See [Schubert, 1870], p.367. We have already discussed this result, see 4.2.2.

a straight line which, at p , is tangent to a curve of the system, intersects the polar line⁷² of p with respect to f on a given straight line e . The order of this locus is the number of intersections it has with any straight line; therefore, explains Schubert, it suffices to count how many points p on e satisfy the condition above, namely ‘being at the intersection of a straight line tangent to a curve of the system and of the polar line of p with respect to f ’.

There are ν points on e at which a curve of the system touches e , per definition of the characteristics. These points all fulfil the condition. Indeed, the tangent line to a curve in the system is e itself, and whatever the polar line is, it will necessarily cut this tangent line on e . Furthermore, any point r on e which lies on its own polar line with respect to f fulfils the condition as well, for a similar reason. These points are none other than the m points where f and e intersect. Each of these points fulfils the condition μ times, as through each of these points p pass μ curves of the system from which the tangent can be drawn to satisfy the condition. Schubert then shows that no other point can satisfy the condition.

Therefore, this locus is of order $\nu + m\mu$; and, it intersects the curve f at $m(\nu + m\mu)$ points. Each of these intersections is a point p such that the polar line of p with respect to f (which is the line tangent to f at p , since p is on f) intersects on e the lines which touch a curve of the system at p . Such a point is both on a tangent of f and on a tangent of a curve of the system; therefore, Schubert notes, when the two tangents do not coincide, it must be their intersection point, and thus be on e . There are only $m\mu$ points in this situation, per hypothesis. Consequently, the number of points p that are real points of contact of coinciding tangents is

$$m(\nu + m\mu) - m\mu = m[(m - 1)\mu + \nu]$$

This proof is immediately followed by and used in a similar proof of an analogous result for surfaces, namely that in a system of surfaces (of order N) of characteristics (μ, ν, ρ) , the number of surfaces touching a given surface of order m is⁷³:

$$m[(m - 1)^2\mu + (m - 1)\nu + \rho]$$

Here again, Schubert resorts to methods foreign to Chasles’ theory of characteristics. As a reader of the papers published in the *Comptes-Rendus*, which he precisely refers to at several occasions, he nonetheless displays no interest or familiarity with Chasles’ principle

⁷²The polar line of a point p with respect to a curve f of order m is a curve of order $m - 1$ which contains every point of f at which the tangent line to f passes through p .

⁷³See [Schubert, 1870], p.368.

of correspondence, which he would put to important uses in later stages of his career⁷⁴.

Once the parameters of certain conditions are determined, Schubert explains, one can finally determine the numbers of curves or surfaces satisfying a maximal number of conditions. Going back to the case of second-order surfaces, Schubert notes that for any nine conditions Z_1, \dots, Z_9 , of parameters $(\alpha_1, \beta_1, \gamma_1), \dots, (\alpha_9, \beta_9, \gamma_9)$,

$$(Z_1, \dots, Z_9) = \alpha_1(\mathbf{p}^1, Z_2, \dots, Z_9) + \beta_1(\mathbf{g}^1, Z_2, \dots, Z_9) + \gamma_1(\mathbf{e}^1, Z_2, \dots, Z_9)$$

where $\mathbf{p}^1, \mathbf{g}^1, \mathbf{e}^1$ represent the elementary conditions ‘passing through a given point’, ‘touching a given line’, and ‘touching a given plane’.

At first glance, this is nothing more than the definition of the parameter of the condition Z_1 , with $(\mathbf{p}^1, Z_2, \dots, Z_9), (\mathbf{g}^1, Z_2, \dots, Z_9), (\mathbf{e}^1, Z_2, \dots, Z_9)$ standing for the characteristics (μ, ν, ρ) of the system defined by the eight conditions Z_2, \dots, Z_9 . However, through this rewriting, Schubert makes several notational innovations. One such innovation is to let parentheses denote both numbers and systems of surfaces: more precisely, 8 conditions surrounded by parentheses represent the system of surfaces satisfying them, while 9 conditions in parentheses represent the number of surfaces satisfying them⁷⁵. This was not the case in Chasles’ (or in Zeuthen’s) notations. Chasles distinguished between the notation for numbers of surfaces, denoted for instance $N(Z_1, \dots, Z_9)$, and systems, which indeed used parentheses: (Z_1, \dots, Z_8) . As a result, Chasles did not equate numbers and (combinations of) systems of surfaces: for instance, he would write $(Z_1, \dots, Z_8) \equiv (\mu, \nu, \rho)$, denoting that the three numbers in the right-hand side characterized the system on the left-hand side.

Furthermore, Schubert introduced exponents m, n, r over the symbols for elementary conditions $\mathbf{p}, \mathbf{g}, \mathbf{e}$ to denote the requirement that these conditions be satisfied m, n, r times (that is to say, \mathbf{p}^m denotes the condition ‘passing through m given points’, and so on). This allows Schubert to further decompose the terms on the right-hand side; for instance substituting $\alpha_2(\mathbf{p}^2, Z_3, \dots, Z_9) + \beta_2(\mathbf{p}^1, \mathbf{g}^1, Z_3, \dots, Z_9) + \gamma_2(\mathbf{p}^1, \mathbf{e}^1, Z_3, \dots, Z_9)$ to $(\mathbf{p}^1, Z_2, \dots, Z_9)$. Through some simple combinatorics, Schubert finds that this process would result in expressing the number of surfaces satisfying 9 conditions as a function of the $9 \times 3 = 27$ parameters, and of the $\frac{10 \cdot 11}{1 \cdot 2} = 55$ quantities $\mathbf{p}^m, \mathbf{g}^n, \mathbf{e}^r$. This expression would therefore be very lengthy, and Schubert only writes its initial terms to show how it can be obtained.

⁷⁴See 7.3.2.

⁷⁵“Ferner möge immer eine Klammer, welche 9 Bedingungen einschliesst, die Anzahl der ihnen genügenden Flächen zweiter Ordnung bedeuten, eine Klammer aber, welche 8 Bedingungen einschliesst, das System der Flächen darstellen welche diesen Genüge leisten”, [Schubert, 1870], pp.369-370.

However, he also suggests representing it with the more synthetic expression⁷⁶:

$$\Sigma[(\mathfrak{p}^m, \mathfrak{g}^n, \mathfrak{e}^r) \Sigma(\alpha^{(m)}, \beta^{(n)}, \gamma^{(r)})]$$

where the first Σ represents a sum over all possible triplets (m, n, r) of positive integers such that $m + n + r = 9$, and where the second Σ represents the sum of all possible products of m characteristics α_i , n characteristics β_i , and r characteristics γ_i ; over the same range of values for m, n and r . Schubert then uses this formula to give concrete numbers of surfaces satisfying certain sets of conditions. Schubert also notes that each term of the first sum is composed of $\frac{9!}{m!n!r!}$ products, but does not go further into this combinatorial approach.

Note the crucial role that the exponents on top of elementary conditions play in the factorization of this expression. Like Zeuthen, Schubert introduced new ways to manipulate symbols for conditions and systems. However, here as well, these uses were limited to the expression of a synthetic formula. Schubert's formula arguably allows for more direct computations than Chasles' enumerative procedure, and it makes the recursive character of this procedure evident; but this exponential notation for conditions, and this common use of parentheses to denote systems and numbers alike, would not be developed further in this text⁷⁷.

In the rest of his dissertation, Schubert identified after Chasles the “fundamental problems” (*Fundementalaufgaben*) of the theory of characteristics for second-order surfaces to be the determination of the factors $(\mathfrak{p}^m, \mathfrak{g}^n, \mathfrak{e}^r)$, which he also wrote (m, n, r) for the sake of brevity. Once these numbers known, it would be easy to compute the number of surfaces satisfying any nine conditions of known parameters, using the formula given above. In fact, these characteristics had already been given by Chasles⁷⁸, but Schubert set out to “publish a justification⁷⁹” for them. Furthermore, Schubert gave a few new numbers of degenerate surfaces in these elementary systems. At the end of his dissertation, he presented these results in a table, wherein he made clear which numbers were already known and which were new (see fig. below).

Before computing these numbers, Schubert had resorted to “a few considerations borrowed from the geometry of position⁸⁰” in order to frame the ontology of figures of the second order which he was to enumerate. Borrowing from the concept of *Grundgebilde*

⁷⁶ [Schubert, 1870], p.370.

⁷⁷A mere three years later, this exponential notation would be reinterpreted by Schubert and given a crucial role in a full-fledged algebra of conditions; see 7.1.2, 7.3.

⁷⁸ [Chasles, 1866d].

⁷⁹“Da, soviel mir bekannt, nie eine Begründung dieser Zahlen publicirt ist..”, [Schubert, 1870], p.371.

⁸⁰“einige fundamentale Betrachtungen aus der Geometrie der Lage”, [Schubert, 1870], p.371.

§. 23. Tabelle der Elementarsysteme (cf. §. 22).

	System	B e k a n n t							Gefunden (nach §. 15)					Reciprokes System
		Wegen		Wegen		σ	ε	α	μ	ν	ϱ	σ	α	
		μ		ϱ										
a)	(4, 0, 4)					0	42	0	21	42	21			(3, 0, 5)
b)	(5, 0, 3)			21	a)	0	30	8	17	34				(2, 0, 6)
c)	(6, 0, 2)			17	b)	0	10		9	18		16		(1, 0, 7)
d)	(7, 0, 1)			9	c)	0	0		3	6		12		(0, 0, 8)
e)	(8, 0, 0)			3	d)	0	0		1	2		4		(3, 1, 4)
f)	(4, 1, 3)	34	b)	42	a)	0	60	16		68				(2, 1, 5)
g)	(5, 1, 2)	18	c)	34	b)	0	20			36		32		(1, 1, 6)
h)	(6, 1, 1)	6	d)	18	g)	0	0			12		24		(0, 1, 7)
i)	(7, 1, 0)	2	e)	6	d)	0	0			4		8		(2, 2, 4)
k)	(3, 2, 3)	68	f)	68	f)	32	72	32		104				(1, 2, 5)
l)	(4, 2, 2)	36	g)	68	f)	0	40			72		64		(0, 2, 6)
m)	(5, 2, 1)	12	h)	36	g)	0	0			24		48		(2, 3, 3)
n)	(6, 2, 0)	4	i)	12	h)	0	0			8		16		(1, 3, 4)
o)	(3, 3, 2)	72	l)	104	k)	32	48			112		96		(0, 3, 5)
p)	(4, 3, 1)	24	m)	72	l)	0	0			48		96		(1, 4, 3)
q)	(5, 3, 0)	8	n)	24	m)	0	0			16		32		(0, 4, 4)
r)	(2, 4, 2)	112	o)	112	o)	32				128		96		(1, 5, 2)
s)	(3, 4, 1)	48	p)	112	o)	16	0			80		144		(0, 5, 3)
t)	(4, 4, 0)	16	q)	48	p)	0	0			32		64		(1, 6, 1)
u)	(2, 5, 1)	80	s)	128	r)	0				104		56		(0, 6, 2)
v)	(3, 5, 0)	32	t)	80	s)	8	0			56		104		(0, 7, 1)
w)	(1, 6, 1)	104	u)	104	u)	0				104		104		
x)	(2, 6, 0)	56	v)	104	u)	0				80		32		
y)	(1, 7, 0)	80	w)	104	v)	0				92		68		
z)	(0, 8, 0)	92	x)	92	y)	0				92		92		

[Schubert, 1870], p.383.

which had been put forth by geometers such as Von Staudt and Reye⁸¹, Schubert described geometrical figures such as conics or second-order surfaces as being all composed of three elements, namely points, straight lines, and planes. By alternating between points of view on a same object – that is to say, by viewing it alternatively as composed of points, straight lines, or planes –, new distinctions may arise, which for Schubert are crucial in discussing its degenerations. For instance, by viewing second-order surfaces as made of points, one obtains cones; but another figure arises from placing these surfaces within the geometry of the line⁸²:

Wie die Punkte und die Geraden, welche in einer Ebene liegen, Elemente der Ebene, und reciprok die Ebenen und die Geraden, welche durch einen Punkt gehen, Elemente des Punktes heissen, so sollen auch die Geraden, welche eine Gerade schneiden, – *Punktgeraden* –, sowie die Geraden, welche mit ihr in einer Ebene liegen, – *Ebenengeraden* –, *Elemente dieser Geraden* genannt werden. Obwohl zwar diese beiden Begriffe, Punktgeraden und Ebe-

⁸¹See [Nabonmand, 2006], pp.112-117; 197-201.

⁸²“As the points and the straight lines which lie in a plane are called elements of the plane, and conversely the planes and the straight lines which go through a point are called elements of the point, so are the straight lines which intersect a line, – point-lines –, as well as the straight lines which lie with it in a plane, – plane-lines – called elements of these straight lines. Although the two terms point-line and plane-line coincide, the distinction will nevertheless prove to be useful. Both the conic belongs to the geometry of the plane and the cone to the geometry of the point as figures of the second order; similarly, to the geometry of lines belong a (second-order) figure which can be understood either as a section of two planes on which two special points lie, or as a line connecting two points, through which two special planes go - which is equivalent. We will call this figure ‘limited plane section’, the two special points ‘main points’, the two special planes ‘main planes’, and the straight line which appears either as joining the main points or as the intersection of the main planes, the ‘main line’”, [Schubert, 1870], p.371.

nengeraden, sich decken, so wird die Unterscheidung sich dennoch als zweckmässig erweisen. Wie nun als Gebilde zweiter Ordnung der Kegelschnitt der Geometrie der Ebene, der Kegel der Geometrie des Punktes angehört, so gehört der Geometrie der Geraden ein Gebilde an, welches entweder als Schnitt zweier Ebenen aufgefasst werden kann, auf dem zwei ausgezeichnete Punkte liegen, oder als Verbindungsgerade zweier Punkte, durch welche Gerade zwei ausgezeichnete Ebenen gehen, was reciprok und gleichbedeutend ist. Dieses Gebilde wollen wir *begrenzten Ebenenschnitt*, die beiden ausgezeichneten Punkte Hauptpunkte, die beiden ausgezeichneten Ebenen *Hauptebenen*, und die Gerade, welche als Verbindung der Hauptpunkte oder als Schnitt der Hauptebenen erscheint, *Hauptgerade* nennen.

These three analogous objects can be decomposed in “elements”: a conic is made of its points and its tangents, a cone of its planes and its tangents (that is to say the lines that generate it), while a ‘limited plane section’ is made of the point-lines that go through both its main points, and the plane-lines that are on both its main planes. Therefrom Schubert can describe these figures in terms of “fundamental figures of first level” (*Grundgebilden erster Stufe*): a conic is a figure which has two elements in common with each sheaf of points (that is to say, each point-line) in its plane, and with each bundle of rays (*Strahlbüschel*) in its plane. Similarly, a cone is a figure that has two elements in common with each bundle of planes which contains its center point, and with each bundle of rays which has same center point. Lastly, a limited plane section is a figure which has two elements in common with each bundle of rays whose center point is on the main line, and with each bundle of rays whose plane contains the main line. Conversely, Schubert asserts, each bundle of planes and each bundle of rays contains two elements which are, respectively, two tangential planes and two straight lines belonging to some conic. Similar statements are made for cones and limited plane section.

Schubert would go on to construct second-order surfaces as having three elements: the points which lie on them, the lines that are tangent to them, and the planes that are tangent to them. This leads Schubert to describe three degenerate cases of second-order surfaces, but also to show how these surfaces can be described in terms of conics in space, cones, and limited plane sections themselves. We shall not present the technical details of Schubert’s construction here, if only because we will do it in a more general context in the next chapter⁸³. It suffices to note here that Schubert proposes to buttress the theory of characteristics with an analysis of the ways in which the geometrical figure to be enumerated can be generated from the main elements of space. In the context of this dissertation, this provides Schubert with yet another solution to the difficult problem of

⁸³See 7.2.3.

special solutions to enumerative problems. Special second-level figures appear when the first-level elements defining it coincide, explains Schubert. In the case of conics, there are two possible cases. In the first case, every sheaf of points has two elements in common with the conic, so that the points of the conic itself form a point-line g , and its tangents form two bundles of rays whose center points are two points on g . In the second case, the conic has two elements in common with every bundle of rays (in its plane), so that the tangents of the conic form a bundle of rays, and the points of the conic form two point-lines going through p . These descriptions of Schubert's correspond respectively to point-pairs and line-pairs. Here again, similar classifications of the two degenerate forms are given for second-order surfaces, which allows Schubert to redefine incidences and tangencies for these limit cases, and to enumerate them. In so doing, Schubert proposes another approach to the same problem which Zeuthen had answered with his own method for the enumeration of degenerate conics satisfying contact conditions.

6.2.3 Maillard's 1871 dissertation

Maillard's dissertation, entitled "Recherche des caractéristiques des systèmes élémentaires de courbes planes du troisième ordre", is perhaps the most faithful continuation of Chasles' initial effort. Maillard had set out to do for systems of curves of the third order what Chasles had previously done for conics, and focused especially on the determination of the characteristics of elementary systems, which he obtained by using systematically the principle of correspondence (even using Chasles' notations). However, while Maillard did not, strictly speaking, generalize or alter the mathematical content of Chasles' principle of correspondence, he had attached to it a new computational technique to this principle, which we shall now present.

After discussing the various modes of degeneration for cubics, Maillard created symbols for several numbers. Some of them are strictly analogous to Chasles' notations for systems of conics, with μ and ν denoting numbers of cubics in a system respectively passing through a given point and touching a given line. Other symbols, however, are specific to these modes of degeneration (in a manner comparable to what Chasles did for systems of second-order surfaces), with for instance α denoting the number of curves in the system which have a double branch and β the number of curves in the system which have a triple branch. Furthermore, some symbols serve to denote degrees (a term which Maillard favors over that of *order*) or classes of certain loci determined by a system, such as δ , the degree of the locus of the double points of the curves of the system, or ρ the degree of the locus of the cusps of these curves. The first half of Maillard's dissertation consists in a list of

algebraic formulas in these symbols, such as⁸⁴:

$$4\mu = \nu + 2\alpha + 6\beta + 2\delta + 3\rho$$

To establish this formula, Maillard considers a point X on a straight line L , through which necessarily pass μ curves in the system. These curves intersect L at 2μ other points U . Conversely, to a point U correspond 2μ other points X (because the reverse construction is the same). From the principle of correspondence it follows that there are 4μ coinciding points. Maillard goes on to list the possible “various causes” from which such coinciding points arise. Amongst them of course lie the curves in the system tangent to L , of which there are ν . But there also are α curves in the system composed of one double straight line and one simple straight line. The intersection of such curves with L also give rise to coinciding points X . Maillard then considers the “déplacement de la courbe aplatie, dont la limite est l’ensemble de deux droites”: as the curve degenerates, it cuts L at two points U', U'' infinitely close to a point X' where the limit (i.e. the degenerate curves) intersects L . Thus, the coincidence is in fact double, and there are 2α coincidences caused by such curves.

Maillard goes on to list the other possibles causes for coincidences, namely triple lines, double points and cusps. In each case, he discusses the multiplicity of these coincidences, thus obtaining the formula shown above. Maillard never shows that his list of causes is exhaustive. His use of the principle of correspondence is rather different from Chasles’: he does not reproduce the same textual apparatus, nor does he use the concept of “solutions étrangères”. Furthermore, this decomposition of the sum $\alpha + \beta$ into a list of causes for coincidences is totally novel⁸⁵.

After producing some fifteen formulas, Maillard explains that they “are not all distinct, which makes it possible to check several among them by supposing the others proven⁸⁶”. From these simple equations in the symbols he had previously defined, Maillard shows how the knowledge of certain “unknowns” allows for the determination of others. Furthermore, combining these formulae allows for the expression of a certain number to be expressed in terms of others, which may be easier to compute in certain cases. This is particularly important, as no equivalent to the $\alpha\mu + \beta\nu$ formula was known for systems of cubics, and Maillard provides no systematic reduction of this collection of equations to a minimal subset of symbols.

In the rest of his memoir, Maillard focuses on special cases, such as curves which

⁸⁴ [Maillard, 1871], p.9.

⁸⁵In fact, this method would play a crucial role in Schubert’s reappropriation of the principle of correspondence in his own geometrical practice later on, see 7.3.2.

⁸⁶ [Maillard, 1871], p.20.

have a given cusp, a given double point, or a double point on a given straight line. In these cases, the determination of the characteristics of elementary systems is enabled by the equations produced previously and the study of their interdependence. In each case, Maillard produces tables of formulas and numbers, culminating with the general series of numbers for general systems of cubics⁸⁷ (see fig. below). Maillard, in his dissertation,

Systèmes	μ	ν	Δ	α	β	t	i
(8p)	1	4	12	0	0	9	12
(7p.1d)	4	16	48	0	0	36	12
(6p.2d)	16	64	192	0	0	144	192
(5p.3d)	64	256	768	0	0	576	768
(4p.4d)	256	976	2784	24	0	2232	2856
(3p.5d)	976	3424	8832	240	0	8064	9552
(2p.6d)	3424	9766	21828	885	360	23841	25563
(1p.7d)	9766	21004	39072	1470	2320	53244	51042
(8d)	21004	33616	50448	0	8400	88236	75648

[Maillard, 1871], p.51.

rather faithfully fulfilled the research programme laid out earlier by Chasles: taking on curves of order three, that is to say the natural continuation of the theory of conics, he had focused his effort on the computation of the characteristics of a variety of elementary systems, defined by elementary conditions and various modes of degenerations. However, in so doing, Maillard proposed new uses for the principle of correspondence, and new textual practices to structure the results derived from it – in particular, relevant to a context where no simple equivalent to the $\alpha\mu + \beta\nu$ formula can be hoped for.

6.3 Algebraic rewritings and analytical proofs

All three dissertations surveyed here attempted to continue Chasles' theory on its own grounds. While Zeuthen, Schubert, and Maillard all introduced new computational methods, notational innovations, and even modified descriptions of degenerate solutions, they nonetheless agreed on the main goals of this theory. In particular, they all focused on computing the characteristics of systems of curves and surfaces, producing lists of formulae and tables of numbers which could be used toward actual enumerations. More crucially, all three texts resort to geometrical descriptions of the objects (figures, conditions, systems) at hand. In this sense, these three dissertations can be described as 'reorganizations' of the theory of characteristics, but not as full-fledged 'rewritings' of it.

⁸⁷Note that this table has symmetries similar to those found in Chasles' theory of conics; for instance, the 4 corresponding to the first row, second column, is equal to that of the second row, first column, because both numbers denote how many cubics go through 8 points and touch one straight line. The other numbers, however, must be computed individually in most cases.

In other words, these three mathematicians accepted the core of Chasles' discourse, and attempted to accommodate its organization and its contour to new domains of application. However, they did not purport to systematically translate and adapt every single term of Chasles' theory into a different mathematical language.

6.3.1 Cayley's 1868 'quasi-geometrical interpretation'

By contrast, Cayley's 1868 presentation of the theory of characteristics constituted a first step toward an algebraic rewriting thereof. In two successive papers, he introduced what he called a "quasi-geometrical representation of conditions". More precisely, Cayley understood "a condition imposed upon a subject" – that is to say, a given kind of figure – "[to give] rise to a relation between the parameters of the subject"⁸⁸. For instance, a condition imposed on conics gives rise to an algebraic equation in the six coefficients that constitute the general equation of a conic. Beyond conics, a subject being defined by a general equation having ω coefficients, a condition will be understood to be an algebraic equation in these coefficients. But a set of ω parameters, Cayley continued, "may be considered as the coordinates of a point in ω -dimensional space"⁸⁹. A relation between the parameters, that is to say an algebraic equation in them, can then be regarded as a locus in this multi-dimensional space.

To this framework, Cayley introduced composed conditions. A condition can be k -fold for any positive integer k : passing through a given point is a onefold condition, passing through two given points, or touching a given line at given point, is a twofold condition, and so on⁹⁰. Similarly, relations and loci associated to k -fold conditions are also said to be k -fold. If the number of parameters of a subject is k , then the number of "solutions of a k -fold relation", that is to say the number of subjects which satisfy a k -fold condition giving rise to the relation, is determinate, that is to say finite. Conditions can be combined, in which case their orders are added, until a maximum order is reached. Indeed, an ω -fold locus is a point-system, that is to say a finite collection of points; therefore, conditions cannot be composed ad libitum.

Cayley then proceeded to exemplify this framework with the case of conic sections.

⁸⁸ [Cayley, 1868a], p.76. The memoir actually opens on a brief survey of the literature on the enumeration of curves which satisfy given conditions, from De Jonquières' 1861 paper to Zeuthen's 1865 dissertation.

⁸⁹ [Cayley, 1868a], p.77.

⁹⁰ Recall that Chasles had already introduced such 'conditions multiples', albeit without this algebraic language; see 4.2.6.

The general equation for conics, in Cayley's notations⁹¹, is as follows:

$$(a, b, c, f, g, h)(x, y, z)^2 = 0$$

There are six parameters in this equation, which form the (homogeneous) coordinates of a point in a 5-dimensional space. In this space, two special loci can be considered. The first one, which Cayley calls the “discriminant-locus”, is formed of all the points which correspond to a line-pair. It is a onefold, cubic locus, as its equation is given by the vanishing of the determinant of the conic, that is to say

$$abc - af^2 - bg^2 - ch^2 + 2fgh = 0$$

A second special locus is the “Bipoint-locus”, that is to say the locus formed of all the points which correspond to a coincident line-pair⁹². The equations defining this locus are:

$$\begin{aligned} bc - f^2 &= 0, \quad ca - g^2 = 0, \quad ab - h^2 = 0 \\ gh - af &= 0, \quad hf - bg = 0, \quad fg - ch = 0 \end{aligned}$$

This locus is the image of the mapping $(a : b : c : f : g : h) \rightarrow (a^2 : b^2 : c^2 : bc : ca : ab)$, and as such, it is a threefold quadric locus⁹³.

Loci can also be associated to conditions. For instance, the conics satisfying the condition of touching a given curve of order m and class n form a one-fold locus (which Cayley calls a “contact locus”); and its order is $2m + n$. Indeed, Chasles' 1864 formulae show that this is the number of conics passing through four given points and satisfying the condition (or, in other words, the number of conics in the system $(2, 1)$ satisfying the condition). In Cayley's quasi-geometrical interpretation, this value is also the number of intersections between the one-fold locus and a straight line, that is to say the order of the locus.

One important theoretical consequence of this reinterpretation is that composing conditions means forming intersections of surfaces in this abstract 5-dimensional space of conics; however, one cannot merely multiply orders in hope of enumerating conics satisfying conditions, as this would lead to counting unsatisfactory solutions (for the same

⁹¹This is the notation introduced in Cayley's famous memoirs on quantics; see [Cayley, 1854], pp.246-247. The expression given here simply translates to the general (homogeneous) equation of a plane conic, that is to say: $ax^2 + by^2 + cz^2 + 2fxy + 2gxz + 2hyz = 0$. On this memoir, see [Crilly, 1986].

⁹²If the conic is represented as a symmetrical matrix $\begin{pmatrix} a & h & g \\ h & b & f \\ g & f & c \end{pmatrix}$, then the equations for this locus are obtained by imposing that each 2×2 minor of this matrix be equal to zero.

⁹³In fact, this is what is now called the *Veronese surface*, see [Harris, 1992], p.23.

reasons which led to the rejection of De Jonquières' theory of plane curves, as discussed in section 5.1.2). Cayley suggests a few ways in which enumerations could be carried out by forming intersections between the Bipoint-locus and contact-loci, but this quasi-geometrical interpretation of the theory of characteristics is not fully brought to fruition. The rest of Cayley's memoir consists mostly in a detailed presentation of Chasles' theory of characteristics and Zeuthen's reformulation thereof, albeit with some notational innovations and a large number of numerical examples⁹⁴. The quasi-geometrical interpretation of the theory of characteristics did not bring to the fore new results or major changes in proofs, save for a new explanation of the flaws of De Jonquières' lemma, namely the analytical representation of series of curves as a rational equation $F(x, y, \lambda) = 0$. In a 1870 paper entitled *On Abstract Geometry*, Cayley would pursue his research into ω -dimensional spaces and their loci, dropping the question of the enumeration of curves or conics⁹⁵. The idea of interpreting conditions and systems of conics as hyper-surfaces and curves in a five-dimensional space would however prove crucial for Corrado Segre and Eduard Study in the 1880s, as we shall see in the last chapter of this dissertation⁹⁶.

6.3.2 Analytical proofs of the $\alpha\mu + \beta\nu$ formula

Cayley's motivations in rewriting the theory of characteristics using algebraic notations was to investigate a broader approach to 'abstract geometry'. By contrast, others sought to employ algebra to prove what they perceived to be a central conjecture of Chasles' theory, namely the $\alpha\mu + \beta\nu$ formula. More precisely, the claim that remained unproven was the existence, for every condition Z , of two numbers α, β such that in every system of conics of characteristics (μ, ν) , the number of conics satisfying Z is given by the linear expression above. While Chasles had observed this regularity but not proved it, it was nonetheless intimately tied to the systematic method by which 'modules' of conditions were obtained, that is to say the principle of correspondence, which always seemed to yield expressions of this form. This was, at least, the opinion of Zeuthen, who wrote in a review of Lindemann's edition of Clebsch's *Vorlesungen* for the *Bulletin des sciences mathématiques et astronomiques*⁹⁷:

⁹⁴ [Cayley, 1868a], pp.84-124.

⁹⁵ [Cayley, 1870].

⁹⁶In particular, see 8.3.2.

⁹⁷"The theorem of Mr. Chasles, which states that the number of conics in a system of characteristics μ and ν which satisfy a new condition is expressed by the formula $\alpha\mu + \beta\nu$, where α and β depend only on the new condition, has been originally found by some sort of induction. However, the large number of cases where the stated law was confirmed, and the lack of cases contrary to it, were not the only reasons we had to adopt it. This law had an intimate connection with the circumstance that it is always possible to determine the numbers in question via the principle of correspondence, always applicable to the determination of numbers of solutions to questions which can be expressed algebraically. It seems impossible, indeed, to reach by this principle expressions of a different form, the expressions of numbers

Le théorème de M. Chasles qui énonce que le nombre des coniques d'un système de caractéristiques μ et ν qui satisfont à une nouvelle condition s'exprime par la formule $\alpha\mu + \beta\nu$, où α et β dépendent seulement de la nouvelle condition, a été trouvé originairement par une sorte d'induction. Le grand nombre de cas où la loi énoncée s'était confirmée, et le défaut de cas qui y fussent contraires, ne formaient pas toutefois les seules raisons qu'on eût pour l'adopter. Elle était en connexion intime avec la circonstance qu'il est toujours possible de déterminer les nombres dont il s'agit par le principe de correspondance, applicable toujours à la détermination des nombres de solutions de questions qui sont exprimables algébriquement. Il semble impossible, en effet, de parvenir par ce principe à des expressions d'une autre forme, les expressions des nombres des seules coniques singulières ayant la même forme. Toutefois ces considérations étaient trop vagues pour constituer une démonstration formelle.

And yet, the search of a proof for this formula was not a priority for the majority of the geometers who took up the theory of characteristics. None of those we surveyed so far did in fact attempt to prove it, or even mentioned the need for such a proof⁹⁸. For figures such as cubics, no such formula was to be hoped; and in the case of surfaces, the actual computation of modules and characteristics was deemed to be more pressing than the investigation of the generality of the formula $\alpha\mu + \beta\nu + \gamma\rho$ by both Chasles and Schubert.

Those who first viewed proving such general formulae as central to the development of the theory of characteristics were mathematicians who, in fact, undertook to rewrite it entirely – using the analytical and algebraic tools at their disposal. In 1873, two proofs of Chasles' $\alpha\mu + \beta\nu$ formula were quasi-simultaneously published. One had been obtained in May 1872 by German mathematician Alfred Clebsch, a few months before his death at a young age. The paper, entirely devoted to proving the formula in question (which Clebsch described as a “remarkable observation”), was published posthumously the following year; it would constitute Clebsch's only contribution to the theory of characteristics⁹⁹.

of singular conics alone having the same form. Nonetheless, these considerations were too vague to constitute a formal proof”, [Zeuthen, 1876], pp.120-121.

⁹⁸Zeuthen in 1865 did prove that this formula was in general correct for all contact conditions, but this was a consequence of his actual computation of the modules of such conditions.

⁹⁹ [Clebsch, 1873].



Alfred Clebsch (1833-1872)

A few months later, on March 19th 1873, Halphen communicated a three-part memoir to the Société Mathématique de France, which was later published in the *Bulletin*, on the same subject¹⁰⁰. In the first part of this memoir, Halphen set out to prove Chasles' formula, as well as a few other results of the same kind on second-order surfaces and complexes of lines. For Halphen, this was a continuation of the work he had already done on systems of straight lines in space between 1869 and 1872, albeit on more elaborate geometrical figures (see 6.1.3).

Halphen's 1873 proof proceeded by attaching an algebraic curve to any given system of conics, so as to be able to transfer methods from the algebraic analysis of curves to the theory of characteristics. More precisely, given a system of conics of characteristics (μ, ν) , Halphen considered a straight line Δ in the plane of the system, and a point a on the line. Denoting m and m' the two points at which a conic of the system intersects Δ , Halphen then formed the curve whose coordinates are

$$x = \frac{1}{2}(am + am') , y = \frac{am \cdot am'}{K}$$

where $K > 0$ is a constant. There is a one-to-one correspondence between the points of this curve and the conics of the system¹⁰¹; as a result, Halphen called this curve the “courbe indicatrice” (*indicatrix*) of the system with respect to Δ . Halphen then correlated the properties of the indicatrix to those of the system. For instance, the degree of this curve, Halphen shows, is the first characteristic of the system, that is to say μ . Furthermore, degenerate conics of the systems are obtained amongst intersections of the indicatrix and

¹⁰⁰ [Halphen, 1873b]. Halphen was not aware of the existence of Clebsch's paper when writing his own memoir.

¹⁰¹Note that this is yet another way of solving the problem of indexing infinite families of curves. In Chasles' and De Jonquières' texts, this was accomplished either via a parameter λ , or through purely verbal means (see 4.1.2). Here, a transfer between a curve and a system is devised, which makes use of the algebraic analysis of curves.

special loci in the plane. Thus, the intersections of curves constructed via the points m and m' can be enumerated using analytical methods, and then equated to formulas involving Chasles' characteristics.

Clebsch did more than transfer algebraic properties onto geometrical concepts: in fact, he began his own proof by translating the notions at the heart of Chasles' theory into those he had expounded in his own work on invariant theory (and in particular, two recently published memoirs¹⁰²). First, a general algebraic equation is given which captures what a system of conics is; then, geometrical conditions are shown to translate into the vanishing of invariants of certain forms. Lastly, Clebsch uses the computational techniques associated to the theory of invariants to prove Chasles' theorem, and to illustrate it via a couple of examples. Through such a translation, Clebsch explains, Chasles' formula illustrates a more general property of quadratic forms (and not only ternary forms, but also with an arbitrary number of variables). This is consistent with strategies Clebsch had employed in various other papers, where the “geometrical clothing” of an equation was to serve as a guide for algebraic reasonings and computations¹⁰³.

Clebsch begins his memoir proper with an algebraic equation for systems of conics, which he views as a “mobile conic” (*bewegliche Kegelschnitt*) of equation $f(x_1, x_2, x_3) = 0$, whose coefficients are algebraic functions of a parameter λ , and where x_1, x_2, x_3 are homogeneous coordinates of the plane. These functions of λ , Clebsch notes, can be irrational¹⁰⁴; to rationalize them, one must introduce a second parameter μ . Adding a third parameter χ , Clebsch also ensures that all these functions are homogeneous of order ρ . As a result, the parameters are linked by the following homogeneous equation of order σ :

$$F(\chi, \lambda, \mu) = 0$$

Each triplet (χ, λ, μ) can be viewed as the (homogeneous) coordinates of a point in the plane in which the system of conics lies; then $F = 0$ defines an algebraic curve in said plane. To each conic of the system corresponds one and only one point of the curve. Clebsch then shows that this correspondence can be made univocal (*eindeutig*), that is to say such that to each point of the curve corresponds exactly one conic of the system. To that end, Clebsch considers the coordinates (χ, λ, μ) of the pole of a fixed straight line G with regard to the mobile conic of the sheaf. The curve $F = 0$ is the locus of these poles,

¹⁰² [Clebsch, 1872a], [Clebsch, 1872b]. On the history of the theory of invariants, see [Dieudonné, 1971], [Crilly, 1986], [Crilly, 1988], [Parshall, 1989].

¹⁰³“Es scheint keinem Zweifel unterworfen, dass man diesen Beweis auf quadratische Formen mit beliebig vielen Veränderliche ausdehnen kann, und dass man daher in dem zu erweisenden Satze eine allgemeine Eigenschaft quadratischer Formen vor sich hat”, [Clebsch, 1873], pp.1-2. See [Lê, 2017], where this presentation of Clebsch's program is made clear in the context of his work on the quintic equation.

¹⁰⁴Clebsch refers to Cayley's 1868 memoir, where De Jonquières' analytical representation of systems of curves was shown to be incorrect; see 4.1.2, 6.3.

and each conic of the system corresponds to only one such pole. Clebsch asserts that G can be chosen so that, conversely, to one such pole corresponds only one conic.

Supposing this isn't the case, then for each conic A , there is a second conic B , so that the poles of G with respect to both conics be identical. This implies that G is one of the sides of the common self-polar triangle (*Polardreieck*) of both conics¹⁰⁵. However, Clebsch notes, the number of all straight lines G is doubly infinite, whereas the number of conics A (and therefore of conics B) is simply infinite. For the number of self-polar triangles common to A and B to be doubly infinite, Clebsch explains, one of two possible cases must happen. Either every conic A and a conic B must have G as a side of their self-polar triangle for an infinite of lines G , or there must be a finite number of conics B such that all straight lines in the plane are side of the self-polar triangle of A and B . The latter case is only possible when A and B coincide, therefore, Clebsch concludes, only the first case remains. As a result, every pair of conics in the system has an infinity of straight lines as sides of their self-polar triangle, and they have a double contact on these straight lines. Clebsch shows that this case can only happen for exceptional systems of conics, and that the property is true for these systems as well. In the end, Clebsch has shown that each system of conics can be put in a $(1, 1)$ -correspondence to the curve of the poles of the conics with regard to an adequately chosen straight line. Note that the construction is different from that given in Halphen's 1873 memoir, as the indicatrix was constructed directly from the intersections of the conics of the system and an arbitrary fixed straight line¹⁰⁶.

After obtaining this correspondence, Clebsch moves on to propose "another interpretation of the equation of the system of conics". Instead of looking at $f(x_1, x_2, x_3) = 0$ as the equation of a variable conic, and at χ, λ and μ as three parameters (determining the coefficients of f) linked by the relation $F = 0$, Clebsch suggests looking at (χ, λ, μ) as the (homogeneous) coordinates of a mobile point in the plane. From this standpoint, he continues, $f(\chi, \lambda, \mu) = 0$ is the equation of a doubly infinite system of curves of order ρ , whose coefficients are determined by the parameters x_1, x_2, x_3 . The advantage of this viewpoint is that both curves $f = 0$ and $F = 0$ depend on the point $x = (x_1, x_2, x_3)$.

Two such curves always have $\rho\sigma$ intersection points. However, Clebsch adds, only certain curves $f = 0$ are 'mobile', that is to say varying with (χ, λ, μ) . For any value of x , the corresponding triplets (χ, λ, μ) correspond to conics in the system passing through x ; their number, as a result, is the first characteristic of the system, which Clebsch denotes

¹⁰⁵A 'self-polar triangle' (relative to a conic) is a triangle whose sides are the polar lines of its vertices, and whose sides are the poles of its vertices. If two conics intersect at four distinct points, they have one and only one common self-polar triangle. If they are tangent in two points, they have an infinity of such triangles. In all other cases, they have none. See [Woods, 1922], p.100.

¹⁰⁶However, Halphen would use this property in his 1876 memoirs on the theory of characteristics, see 6.4.3.

a. The other intersection points, which do not vary with (χ, λ, μ) , are “exceptional points of first category” of the curve F . They are points at which the equation $f = 0$ does not depend on x anymore since all the coefficients of f vanish. Their number is $\rho\sigma - a$. Clebsch then explains that a similar reasoning can be carried out with respect to the tangent equation of the system of conics $\varphi = 0$, which is an homogeneous equation in χ, λ, μ of order 2ρ when formed “the usual way”. There are b intersection points of $F = 0$ and $\varphi = 0$, where b is the second characteristic of the system. This dual equation gives $2\rho\sigma - b$ exceptional points of the second category.

Having completed his analysis of the concept of a system of conics, Clebsch continues by finding an algebraic translation for geometrical conditions on conics. Conditions, Clebsch explains, can be understood as the vanishing of an invariant, denoted

$$\Pi = 0$$

The invariant Π contains not only the coefficients of the conic, but also the coordinates of the given elements of the condition; such as the given points through which the conics are required to pass, or the coefficients of the given curves which conics are required to touch. If π denotes the degree of Π in the coefficients of f , then $\Pi = 0$ can be viewed as a curve in (χ, λ, μ) of order $\pi\rho$. The curves satisfying the condition in the system are intersection points of the curves $\Pi = 0$ and $F = 0$, their number therefore is $\pi\rho\sigma$. It remains only, adds Clebsch, to subtract from this value the number of triplets (χ, λ, μ) which are intersections of the two curves, but are independent of the elements of the condition. In particular, Clebsch sets out to show that only the exceptional points described previously can represent such pathological solutions to the problem, and to enumerate them with their multiplicities to obtain Chasles’ formula.

To do so, Clebsch mobilizes his past researches into the theory of invariants. With Clebsch’s notations, the conic f could be written as a_x^2 , a quadratic ternary form (since a conic is a second-order curve in three homogeneous parameters). Every invariant of such a form, Clebsch had shown, could be written as a function of the reduced system of forms u_x, f, φ and A , where $\varphi = (abu)^2$ and $A = (abc)^2$. From there, Clebsch deduced that every form in the reduced system corresponding to Π could be written as

$$f^\alpha \cdot \varphi^\beta \cdot A^\gamma \cdot F(f \cdot \varphi, A \cdot u_x^2)$$

for some integers α, β, γ . This is a form in the coefficients of f of degree $\alpha + 2\beta + 3(\gamma + \delta) = \pi$. Therefore, Clebsch continues, for all forms in the reduced system associated to Π , the following equation holds:

$$\alpha + \gamma + \delta = \pi - 2(\beta + \gamma + \delta)$$

However, the left-hand term, as well as $\beta + \gamma + \delta$, do depend on the form. Clebsch defines two numbers μ, ν as extrema of combinations of these terms¹⁰⁷, which characterize Π and correspond to the coefficients of the module of the condition¹⁰⁸ Π . Indeed, Clebsch shows that Π can be represented as a homogeneous function in the coefficients of f of order μ , and as a homogeneous function in the coefficients of φ of order ν . From there, Clebsch is able to find through intricate computations (into which we shall not go) that the number of intersection points between the curves attached to Π and F , minus the special points, is $\mu a + \nu b$.

Three years later, in Lindemann's 1876 edition of Clebsch's *Vorlesungen*, a different account of Chasles' theory of characteristics would be given¹⁰⁹. Several of the remarks, both historical and mathematical, introduced in these notes cannot have been written by Clebsch himself, as they refer to developments posterior to his death. In particular, is mentioned the *Geometrie der Anzahl*, which only came to the fore with papers of Schubert's¹¹⁰ published between 1874 and 1876. Therefore, this text must largely be attributed to Lindemann, rather than to Clebsch. The chapter written for the *Vorlesungen* displays much more interest in the enumerative results of Chasles' proper, as bespeak the tables of numbers which are reproduced therein.

In it, a much shorter proof of the $\alpha\mu + \beta\nu$ formula is given, which also relies on the notion of an invariant. Lindemann begins this proof by considering five "equations of conditions" (*Bedingungsgleichung*) $\Pi_i = 0$ in the coefficients $a_{i,k}$ of the punctual equations (*Punktgleichung*, as opposed to tangential equations) of a conic. Denoting q_i the degrees of these equations, Lindemann equated the first characteristic μ of a system determined by the first four conditions to the product of the four first degrees:

$$\mu = q_1 \cdot q_2 \cdot q_3 \cdot q_4$$

Imposing a fifth condition upon the coefficients $a_{i,k}$, explained Lindemann, yields a finite number of solutions, namely

$$\mu \cdot q_5 = q_1 \cdot q_2 \cdot q_3 \cdot q_4 \cdot q_5$$

¹⁰⁷With Chasles' notations, these are α and β (see section 4.2.3).

¹⁰⁸It is here that Halphen would locate what he perceived to be a critical mistake in Clebsch's memoir, according to his letters to Zeuthen: "Je trouve excellent tout ce qui précède [...]. C'est là que commencent mes observations. [...] Les deux nombres que Clebsch appelle μ, ν [...] sont les deux coefficients du *module* de la condition $\Pi = 0$. Ils doivent donc s'échanger entre eux quand on transforme Π corrélativement. Or cet échange n'a pas lieu entre les deux nombres de Clebsch", see [Jordan *et al.*, 1924], pp.630-631.

¹⁰⁹[Lindemann, 1876], pp.390-425. In the second volume of the French translation (by Adolphe Benoist) of this text, this entire section was heavily rewritten in order to 'correct' Lindemann's assertions using the 1876 memoirs of Halphen. In particular, Chasles' formula is said to be false in this text. See [Benoist, 1880], pp.113-130. We come back to Halphen's refutation toward the end of this chapter, see 6.4.

¹¹⁰These texts are discussed in the next chapter, see 7.1.3.

However, among the μq_5 conics, there are some conics which are solutions in appearance only, which “fulfill the condition $\Pi_5 = 0$ identically”. For instance, explains Lindemann¹¹¹,

Für eine Dopellinie verschwindet bekanntlich die linke Seite der Linienkoordinatengleichung $F = (abu)^2 = u_\alpha^2$ unabhängig von den u ; es wird daher *jede* in unserm Systeme enthaltene Dopellinie der Bedingung $\Pi_5 = 0$ genügen, sobald die Coëfficienten $a_{i,k}$ in Π_5 sich theilweise der Art zu Coëfficienten $\alpha_{i,k}$ der Linienkoordinatengleichung vereinigen lassen, dass die letzteren homogen in Π_5 vorkommen. Es wird dann jede der λ Dopellinien β -mal als eine Lösung unseres Problems zu zählen sein, wenn wir mit β den Grad bezeichnen, zu welchem Π_5 die $\alpha_{i,k}$ enthält.

Letting α denote the degree of Π_5 in the $a_{i,k}$, Lindemann writes $q_5 = \alpha + 2\beta$. The number of real solutions, he continues, is

$$q_5\mu - \beta\lambda = \alpha\mu + \beta(2\mu - \lambda)$$

which concludes the proof by posing $\nu = 2\mu - \lambda$.

This proof is remarkably shorter and simpler than Clebsch’s; in particular, it does not rely on any technical result other than basic manipulations on invariant symbols, and the geometrical interpretation of invariants. It would be deemed wholly unsatisfactory by Halphen¹¹², but appreciated by Zeuthen, especially due to its relative simplicity compared to the proofs of Halphen and Clebsch. In his review of the *Vorlesungen* for the *Bulletin des sciences mathématiques et astronomiques*, Zeuthen wrote¹¹³:

Il était donc juste que les géomètres s’intéressassent vivement aux démonstrations ingénieuses de Clebsch et de Halphen, bien que même, en s’éloignant

¹¹¹“For a double line [point-pair], the left-hand side of the line-coordinate equation $F = (abu)^2 = u_\alpha^2$ vanishes independently from u ; therefore, *any* double line contained in our system will satisfy the condition $\Pi_5 = 0$ as soon as the coefficients $a_{i,k}$ in Π_5 are partially joined to the coefficients $\alpha_{i,k}$ in the line coordinate equation, and that the latter appear homogeneously in Π_5 . Then, each double line λ will be counted β times as a solution to our problem, where β denotes the degree to which Π_5 contains the $\alpha_{i,k}$ ”, [Lindemann, 1876], pp.398-399.

¹¹²Halphen had a very low opinion of Lindemann’s book in general, as a private letter from Zeuthen to Halphen indicates, wherein the Danish mathematician wrote: “A un égard je suis très disposé à défendre M.Lindemann envers vous. Vous dites (‘entre nous’) que tout ce que l’on voit dans son livre sur cette théorie vous paraît être un pillage éhonté, augmenté des erreurs les plus grossières”, Bibliothèque de l’Institut, Paris, Ms 5264 222, Letter Zeuthen to Halphen, Dated October 5th 1876.

¹¹³“It was thus fair that geometers quickly be interested in the ingenious proofs of Clebsch and Halphen, even though, by moving far from the considerations we just spoke of, they became long and hard to follow. Mr. Lindemann managed to establish an algebraic proof whose simplicity corresponds to that of the theorem and of the considerations on the principle of correspondence which led to it. [...] It is very difficult to assert that in proofs of this kind there is no weak spot left; but, at any rate, we believe that the path chosen here is the right one, even if there might remain in the details some caution to have or some expression to correct”, [Zeuthen, 1876], pp.121-122.

beaucoup des considérations dont nous venons de parler, elles devinssent longues et difficiles à suivre. M.Lindemann a réussi à établir une démonstration algébrique dont la simplicité correspond à celle du théorème et des considérations sur le principe de correspondance qui y ont conduit. [..]

Il est très-difficile d'affirmer qu'en des démonstrations de cette espèce il ne reste plus aucun point faible; mais, en tout cas, nous croyons que la voie choisie est bonne, quand même il y aurait dans le détail encore quelque précaution à avoir ou quelque expression à corriger.

However, private letters from Zeuthen to Halphen¹¹⁴ (or, later on, Schubert's reactions to Halphen's publications) seem to indicate that these analytical proofs were to a large extent incomprehensible for those who had pursued more geometrical approaches.

6.4 Halphen's analytical refutation of Chasles' theorem

From this survey of the early readings of Chasles' theory of characteristics, it appears clearly that no one agreed as to what the main task ahead was. Halphen and Clebsch wanted to prove Chasles' $\alpha\mu + \beta\nu$ theorem, Maillard, Zeuthen, and Schubert wanted to compute the characteristics of elementary systems and organize them in tables, others still, such as Cayley, used the theory of characteristics as an example and a case-study for broader, novel geometrical theories. To this diversity in goals and directions corresponds a diversity in methods, both for proving results and for displaying them. Schubert's work on notations to achieve a synthetic expression for the number of surfaces satisfying 9 conditions, Maillard's tables, or Clebsch's construction of equations which can be read in various ways, all bespeak different understandings of what constitutes a mathematical result, its generality, and its proper mode of exposition.

To these layers of epistemic variation, one more would be added in 1876, as Halphen begun to have doubts over his past proof of Chasles' $\alpha\mu + \beta\nu$ formula, especially after he had found significant weaknesses in the proofs of Clebsch and Lindemann. By the end of July, he was convinced that the formula was outright false¹¹⁵; two months later,

¹¹⁴“J'avoue du reste que la démonstration de Clebsch m'a semblé toujours extrêmement difficile ; par conséquent, quand même je n'ai pas trouvé des objections à faire avec détails, je n'aurais pas osé adopter le théorème à la seule base de cette démonstration”, Ms 5624 224, Letter Zeuthen to Halphen, dated August 11th 1876.

¹¹⁵“Il est entendu que je maintiens absolument ce que je vous écrivais l'autre jour : je ne doute plus de l'inexactitude du théorème $\alpha\mu + \beta\nu$. Assurément le plus curieux en ce moment à mes yeux était de reconnaître bien exactement les fautes commises dans les démonstrations qui ont été données de ce prétendu théorème”, Letter Halphen to Zeuthen, dated July 29th 1879, [Jordan *et al.*, 1924], pp.629-630.

he presented his first counter-examples before the Congrès de l'Association Française in Clermont-Ferrand, then before the Paris Académie des Sciences. Over the following years, he would substantiate these counter-examples with the help of an alternative theory of conics and their degenerations, thereby raising a difficult question to all those who had pursued the theory of characteristics by assuming this formula to be true. Beyond refuting results based on this formula, Halphen had refuted the core argument behind the theory's claim to simplicity and systematicity, by showing that neither $\alpha\mu + \beta\nu$, nor any other such short expression could capture in general the number of conics satisfying five conditions.

6.4.1 Halphen's 1876 counter-example and its significance

Halphen's first published counter-example to Chasles' formula was presented during one of the weekly meetings of the Paris Académie des Sciences¹¹⁶, on September 4th 1876. A couple of months later, Halphen submitted a memoir explaining this very refutation in more detail to the Académie: Chasles, alongside Bonnet and Puiseux, were to act as referee. While the report they wrote was not made public, a brief excerpt of this memoir was published in the *Comptes-Rendus* in November, the same year¹¹⁷. These two texts, while both contending the falsity of the same formula, exhibit different strategies.

In his initial communication, Halphen opened with a concrete counter-example to Chasles' formula: he constructed two specific systems of conics S and S' , both of characteristics $\mu = \nu = 1$, and a specific condition K , which was satisfied by respectively 3 and 4 conics in the two systems. This contradicts Chasles' formula, because the number of conics satisfying this specific condition in a system should only depend on the latter's characteristics¹¹⁸.

The condition K proposed by Halphen is the following: on a given straight line, any conic intercepts a segment mm' . Furthermore, from a given point, the conic is “seen” at a certain angle α , which is the angle of the two tangents to the conic drawn from this point. The condition is that the ratio $\frac{mm'}{\sin(\alpha)}$ be equal to a given value¹¹⁹. The two systems in which Halphen claims this condition is satisfied by different numbers of conics are the system S of all conics touching a given curve at two given points, and the system S' of all conics having a third-order contact at a given point of a given curve. S' is, Halphen explains, a “particular case” of S ; it is the case when the given points in S coincide. It is

¹¹⁶ [Halphen, 1876a]. Interestingly, Chasles was also present on this very day at the Académie, delivering one of his communications on the principle of correspondence and systems of pairs of segments; [Chasles, 1876a]. There is no record of a reaction from Chasles to Halphen's counter-example.

¹¹⁷ [Halphen, 1876b].

¹¹⁸ [Halphen, 1876a], pp.537-538.

¹¹⁹This is not a projective condition, as Halphen notes; however, he claims it would be easy to remediate this situation. In his expanded memoirs, he does give a procedure to turn metrical conditions into projective ones, see 6.4.4.

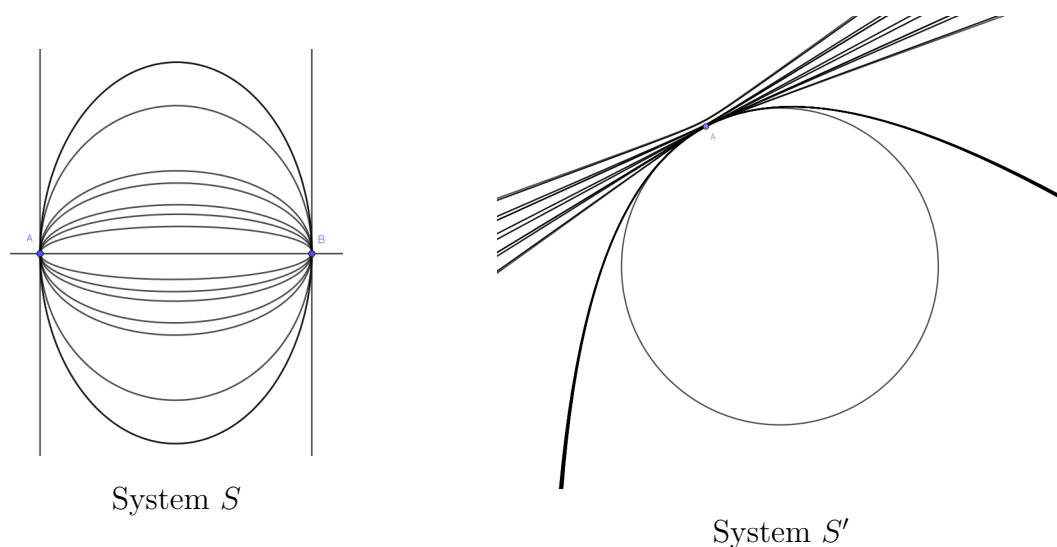
“easy to find”, Halphen continues, that 4 conics in S satisfy K , but only three in S' . No further explanation, however, is provided.

This is, at first, extremely puzzling. How could it be that, in the face of three published proofs and of hundreds of examples manufactured by Chasles and others, this somewhat simple example would fail to verify the $\alpha\mu + \beta\nu$ formula – especially considering these systems are defined rather classically by contacts, and the condition K itself seems rather elementary? According to Halphen, the flaw with former proofs and justifications for Chasles’ formula – his own included – was, in each case, that they failed to take into account the fact that conics can present three “modes of degeneration”. While line-pairs and point-pairs (that is to say conics made of respectively two lines and one point, one line and two points) had been thoroughly dealt with, Halphen explains, it had not been noticed that a third kind of degeneration exists, namely conics made of one line and one point (on the line). While line-pairs and point-pairs are dual (or, in Halphen’s words, “correlative”), this third mode is self-dual (Halphen denotes these modes of degeneration respectively A , A' and B). The presence of this third mode is what causes Chasles’ formula to sometimes be falsified, according to Halphen¹²⁰. Conics of form B constitute a higher mode of degeneration, because they degenerate both punctually and tangentially¹²¹, and their presence can be the only reason for the discrepancy between the number of conics satisfying K in the systems S and S' .

The enumeration of degenerate conics in both systems does indeed point to a real difficulty. For the sake of simplicity, let us consider a particular case of the first system, namely a system of conics touching two straight lines at specified points (see fig. above). This system contains one point-pair (depicted on the figure), namely the double-line which joins the two specified points. It also contains one line-pair, formed by the pair of tangent lines and their intersection point (on the figure above, this point is at infinity). These numbers of degenerate conics correspond to the values provided by Chasles’ theory, as the system has for characteristics $(1, 1)$, therefore $\omega = 2\mu - \nu = 1 = \lambda$. In the other system, the same numbers of degenerate conics should be expected. However, if one views the system S' indeed as a special case of the system S , wherein both stipulated points become infinitely close, then these two degenerate conics coincide, and are seemingly of this third kind: they consist of one straight line (the tangent to the given curve at the given point), and of one point (the contact point). Depending on how one interprets the requirement that degenerate conics satisfy the condition K (which, at this point, might seem unclear,

¹²⁰While Halphen does not seem to have been aware of it, this third mode of degeneration had already been discussed by British mathematicians in the pages of the *Quarterly* and the *Messenger* between 1866 and 1868, see in particular [Hirst, 1866], [Taylor, 1867], [Salmon, 1867a], [Cayley, 1868a]. This discussion was not thought by these mathematicians to have any consequences on the theory of characteristics *per se*.

¹²¹We present the justification for the existence of this third mode of degeneration below, see 6.4.3.



given that one should stipulate what the values of mm' and α are in such exceptional cases), then one might count different numbers of solutions in each system.

Little was done in Halphen's initial communication, in September 1876 to explain where this third degeneration of conics comes from, or why it matters so greatly to Chasles' formula. Neither would Halphen add much to answer these questions in his subsequent communications in the same year. However, in the short excerpt of his memoir published in the *Comptes-Rendus* in November, Halphen put forth a slightly different presentation of his results, whereby he put the emphasis less on his negative assessment of the validity of Chasles' formula, and rather on a more positive presentation of equivalent statements¹²².

Halphen opened by recalling the existence of counter-examples, which he had made public a few months prior. In order to "make the true significance of previously acquired results [on the problem of enumerating conics in a system satisfying one condition] and of the new solution easier to grasp", Halphen suggested a comparison with the problem of enumerating points on an algebraic (plane) curve satisfying a given condition, such as inflexion points, or points of intersection with another given curve (whose numbers are given by formulae such as Plücker's or Bézout's). In such questions, Halphen continued, one begins by "characterizing a curve by its order, then its order and ordinary singularities, lastly by its order and all kinds of singularities". Similarly, for Halphen, the problem of enumerating conics can be solved in three steps (*stages*). In the first one, only tangential singularities, that is to say degenerations A , are taken into account. In the second, the correlative degenerations A' are included. These are the "ordinary singularities" for conics. Finally, higher singularities are included in the form of degenerations B .

To each of these three stages corresponds a different theorem. For instance, Halphen

¹²²This alternative presentation might have been suggested by his epistolary exchange with Zeuthen, see 8.2.1.

explains¹²³:

THÉORÈME I. – Si un système ne contient que la singularité A , le nombre des coniques de ce système qui satisfont à une condition quelconque est le produit de deux nombres, dont l'un ne dépend que du système, l'autre que de la condition. [..]

THÉORÈME II. – Si un système ne contient que les singularités ordinaires, le nombre des coniques de ce système qui satisfont à une condition quelconque est $\alpha\mu + \beta\nu$, μ and ν étant les caractéristiques du système, et α, β des nombres ne dépendant que de la condition. [..]

Dans tous les autres cas, le résultat est d'une forme beaucoup plus compliquée.

What Halphen suggested here is a new framing of the significance of his counter-example. Rather than merely refuting a formula previously thought to be true, Halphen's counter-example points to a renewed understanding of past results, including De Jonquières' $2(n-1)N$ formula (or, using the notations of the theory of characteristics, the $\alpha\mu$ formula). Indeed, the first theorem stated above is none other than De Jonquières' formula, which Chasles had criticized. In Halphen's presentation, however, this theorem is not outright false, but only limited to systems of conics in which only tangential degenerations are present. Similarly, Chasles' celebrated formula is limited to systems of conics in which only normal (i.e. A or A') singularities occur. Even in some specific systems of conics with B -degenerations, one of these two formulae can be shown to be correct. In complete generality, however, no simple, all-encompassing formula can be expected, Halphen claimed¹²⁴.

Thus, Halphen moved from presenting his results by way of a counter-example refuting a theorem long believed to be true, to framing his contribution as an improvement on past results, an enhanced theory of systems of conics. The theory of algebraic curves had made progress by refining the concept of intersection (and, relatey, that of singularity); similarly, his forthcoming memoir was to bring forth a more fine-grained description of the singularities of a system of conics (and, in so doing, of the number of elements in a system satisfying a given condition).

¹²³“Theorem I. – If a system only contains the singularity A , the number of conics of this system which satisfy any condition is the product of two numbers, one of which depends only on the system, the other one of the condition. [..] Theorem II. – If a system contains only ordinary singularities, the number of conics in this system which satisfy any condition is $\alpha\mu + \beta\nu$, where μ and ν are the characteristics of the system, and α, β numbers depending only on the condition. [..] In all other cases, the result has a much more complicated form”, [Halphen, 1876b], p.887.

¹²⁴However, he was able to give what he called an “image” of the general result: he was in possession of a method to construct two plane (algebraic) curves, attached respectively to the system and the condition, such that the numbers of conics in the system (μ, ν) satisfying a condition Z of parameters α, β was obtained by subtracting μ , the number of intersections these two curves have along the axis y , from the expression $\alpha\mu + \beta\nu$.

6.4.2 A difficult publication

Halphen's 1876 communications only gave readers a glimpse into this full-fledged theory of systems of conics (and second-order surfaces). This theory would be published with important delay across several papers, in various journals and countries. Halphen's initial memoir for the Paris Académie would only be published in 1878, by the *Journal de l'Ecole Polytechnique*¹²⁵. In between these two dates, Halphen had sent a shorter memoir to the London Mathematical Society, which was read publicly on June 13th 1878, and inserted in the *Proceedings*. Felix Klein, then editor of the *Mathematische Annalen*, immediately asked Halphen for the right to re-publish this latter memoir in his own journal, without any modification¹²⁶. All of these texts, written in French, share the goal of expounding a complete theory of characteristics for systems of conics, wherein Chasles' formula is corrected; however, they present a few variations in technical content, to which we return below.

The relatively slow publication of Halphen's ideas was noticeable for those involved in the theory of characteristics: after a global announcement in 1876 which did not fail to attract the interest of many a geometer, it became important for a richer explanation of this refutation to be available. Zeuthen, writing to Halphen on October 19th 1876, made this need clear¹²⁷:

En tout cas, il est hardi de publier quelque chose en cette matière avant l'apparition de votre nouvelle théorie. Et celle-ci, se fera-t-elle attendre jusqu'à Avril ? Ne serait-il pas possible à M. Résal de la faire imprimer avant.

As printing kept being delayed, Zeuthen quickly grew frustrated by the situation. In December 1877, he wrote again to Halphen to enquire about the state of his manuscripts¹²⁸:

La lenteur de ceux qui devraient se faire un plaisir à s'empresse de publier vos mémoires est tout-à-fait intolérable. Il devient par exemple difficile de publier quelque chose sur les caractéristiques avant de connaître toute la portée de vos découvertes à cet égard.

Zeuthen's letters seem to indicate that he gained access to Halphen's memoirs only in September 1878.

¹²⁵ [Halphen, 1878b].

¹²⁶ [Halphen, 1878a], [Halphen, 1879].

¹²⁷ "At any rate, one must be brave to publish anything on this matter before the publication of your new theory. And this one, will we have to wait for it until April? Is it not possible for Mr. Résal to publish it any earlier", Ms 5624 223, Letter Zeuthen to Halphen, dated October 19th 1876. Zeuthen is most likely referring to Aimé-Henry Résal.

¹²⁸ "The slowness of those who should be glad to hurry and publish your memoirs is absolutely intolerable. It becomes for instance difficult to publish anything on the characteristics before knowing the scope of your discovery in this regard", Ms 5624 226, Letter Zeuthen to Halphen, dated December 15th 1877.

Besides the slow printing of his memoirs, Halphen found it difficult to communicate his results and methods due to its technical aspects. His memoirs, even in the hands of the British mathematicians of the London Mathematical Society (including Arthur Cayley), were hardly understood. Hirst, who was also responsible for the circulation of Halphen's ideas toward London, related in his diaries the circumstances of the presentation of Halphen's memoir before the London Mathematical Society, in June 1878 in the following terms¹²⁹:

I communicated to the Math. Society on the 13th Halphen's results on the Theory of the Characteristics of Systems of Conics. Smith was in the chair and Henrici and Cayley was [sic] present (Cayley stopped with me at Greenwich) Halphen's paper was an intricate but an important one. The question was whether Chasles' celebrated formula $\alpha\mu + \beta\nu$ for the number of conics in a system (μ, ν) satisfying four conditions which satisfy a fifth condition was or was not generally true. I tried to disentangle the question of subtleties and to show that it was true if all degenerate conics were properly counted but that it required no complementary term $-\lambda$ dependent upon [illegible] points of the system if proper conics alone were taken into consideration. I spoke for an hour and Cayley followed me. He was joint referee with me and I have since received at least five letters from him in 3 of which he stated Halphen's correction to be incorrect and in the other 2 correct. So intricate is the analytical treatment of the question. Geometrically I have no doubt of the correctness of the view I stated

Cayley and Hirst were not the only ones who struggled to make sense of Halphen's intricate analytical methods. Upon receiving the same memoirs, Zeuthen wrote several times to Halphen, asking for explanations regarding various reasonings or definitions. Schubert, in his polemical exchange with Halphen over second half of the year 1879, seems to have understood little of the subtleties of the latter's analytical classification of degenerate figures – something made apparent by Halphen's growing exasperation before what he perceived to be an impossible dialogue¹³⁰.

The combination of a delayed publication, and of an expert use of intricate analytical methods which few of the geometers involved in the theory of characteristics mastered, meant that Halphen's views were often accepted, but rarely understood. Many, especially in France, would systematically agree with Halphen's conclusions with respect to the validity of Chasles' formula; but few would engage with his theoretical approach to these questions. This would be the case, for instance, of De Jonquières himself: in a letter

¹²⁹Hirst's diaries, Journal XIV, pp.145-146.

¹³⁰See 8.2.2.

written in 1883, having retired from his military career, he saw Halphen's refutation as confirming his earlier doubts over Chasles' formula – apparently not paying too much mind to the fact that his own formula had been subject to an even stronger limitation¹³¹:

Mon cher capitaine,

Je lis, à la page 2 de votre beau mémoire sur les “caractéristiques”, cette phrase : “La diversité des exemples (ligne 7) .. fit croire à la généralité de cette proposition”, et vous citez en note (même page) l'opinion de M. Zeuthen qui n'y avait pas confiance. Vous auriez pu ajouter (puisque vous avez bien voulu me citer) que, dès le début de la question et sans attendre l'avis de M. Zeuthen, je n'y avais pas cru.

Vous en trouverez la preuve dans le §XXVIII du mémoire [...] “sur les contacts multiples d'ordre quelconque des courbes de degré n avec une courbe fixe de degré m , etc.”¹³². Il y est dit “[...] Ce fait expérimental peut découler ..”

Vous trouverez la même pensée exprimée encore plus catégoriquement page 7 (§10) et page 8 (§11) du mémoire intitulé “Recherches sur les séries ou systèmes de courbes et de surfaces”, et surtout à la page 6 de ma “Lettre à M. Chasles sur une question en litige”, opuscule que vous n'avez peut-être pas connu et dont je me fais un plaisir de vous adresser aujourd'hui un exemplaire, avec deux autres factum (du même temps) que j'exhume de la poussière où ils étaient plongés.

Il a fallu votre envoi, monsieur, pour me faire tirer de l'oubli ces anciens vestiges d'une lutte où j'étais seul à croiser le fer contre un illustre mathématicien qui ne ménageait pas les coups et en usait parfois avec moi d'une façon peu courtoise. Quoi qu'il en soit, vous y verrez que je n'ai jamais vu dans le succès des caractéristiques qu'un fait expérimental curieux, jusqu'au jour où vous

¹³¹“My dear captain, I read, page 2 of your beautiful memoir on the ‘characteristics’, this sentence: the diversity of examples (line 7).. led to the widespread belief in the generality of this proposition’, and you cite in a note (same page) the opinion of M. Zeuthen, who did not trust it. You could have added (since you were kind enough to cite me) that, from the beginning and without waiting for M. Zeuthen’s opinion, I did believe in it either. Of that, you will find a proof in §XXVIII of the memoir [...] ‘on the multiple contacts of any order that curves of degree n have with a fixed curve of degree m , etc.’. There, it is said that ‘[...] This experimental fact can follow..’ You will find the same thought expressed even more categorically page 7 (§10) and page 8 (§11) of my memoir titled ‘Research on the series or systems of curves and surfaces’, and especially page 6 of my ‘Letter to M. Chasles on a question in dispute’, opuscule which you may not have known and which I am happy to send you a copy today, with two other factum (of the same period) which I now withdraw from the dust they were gathering. It took a letter from you, sir, to bring back to my memory these old vestiges from a fight during which I was alone, crossing swords with an illustrious mathematician who did not hold back, and sometimes resorted to rather low blows against me. In any case, you will see that I never saw in the success of the characteristics more than a curious experimental fact, up until that day when you made their prestige vanish, leaving nonetheless untouched what I said in the aforementioned memoir regarding my own theory (not only for conics, but for all C_n and S_n)”, Ms 5624 69, Letter De Jonquières to Halphen, dated June 17th 1883.

¹³²The text to which De Jonquières alludes here is [de Jonquières, 1866a], pp.316-317.

avez fait évanouir le prestige, laissant subsister toutefois en ce qui concerne ma théorie (non seulement pour les coniques, mais pour les C_n et les S_n) ce que je disais en être exact dans le mémoire précité : “Recherches sur les séries..”

Halphen was seemingly unconvinced by De Jonquières' references, as the latter wrote back only four days later to insist that, by the use of the term “expérimental”, he was already in 1866 doubting “la parole du maître”. However, De Jonquières' subsequent defense of his own theory – which he then took to have prevailed – shows a limited understanding of Halphen's memoirs. In particular, De Jonquières seems to believe that his formulae are correct when systems contain no singular curves whatsoever¹³³; however, this is almost never the case in a system of curves, and at any rate, it is not what Halphen had showed.

6.4.3 A new classification of degenerate conics

In the rest of this chapter, we follow the 1878 memoir published in the *Journal de l'Ecole Polytechnique*, focusing on how, after Clebsch, Halphen undertook a systematic algebraic translation of all of the geometrical notions at the heart of the theory of characteristics¹³⁴. This memoir is divided into five sections. The first one deals with the singularities of systems of conics; the second one with the algebraic representation of conditions; and the third one uses the results obtained in the first two to refute Chasles' formula and prove Halphen's alternative theorems. Section four consists in a series of examples, while section five provides analogous results for systems of second-order surfaces; we will not discuss these two final sections here.

Halphen's study of the singularities of systems of conics (which he also describes as “figures in the system which are not conics, but limits of conics¹³⁵”) begins with the following remark: singularities correspond to special cases of the general equation of the second degree. There are two such special cases, namely

$$(ax + by + c)^2 = 0 \text{ and } (ax + by + c)(dx + ey + f) = 0$$

In punctual coordinates, continues Halphen, these two cases correspond respectively to one

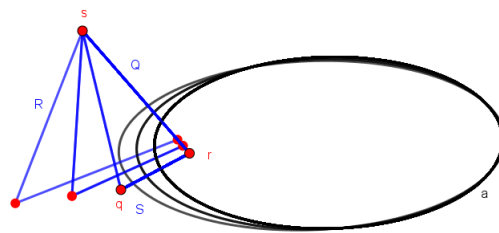
¹³³“Ce que je voulais seulement établir, c'est que ma théorie, bornée aux cas où le système ne contient aucune courbe singulière ou décomposée, (ce que l'examen des conditions permet a priori de prévoir s'il s'agit de système élémentaire), subsistait tout entière et de plus était démontrée, tandis que celle de M.Chasles ne reposait que sur des allégations, appuyées des seules qualifications de prodigieux, admirables, qu'on n'aurait pas pu prévoir, etc, et qui, dans ces termes si absolus, si tranchants, disons si dédaigneux, se sont précisément trouvées fausses ! Tout cela est bien rétrospectif, et si je me permets de vous en parler, c'est dans un pur intérêt historique”, Ms 5624 67, Letter De Jonquières to Halphen, dated June 21st 1883.

¹³⁴For a contemporary exposition of an enumerative theory inspired by Halphen's memoirs, see [Casas-Alvero & Xambò-Descamps, 1986].

¹³⁵ [Halphen, 1878b], p.33.

(double) line and to two lines; in tangential coordinates, they correspond to one (double) point and two points. There remains to determine what cases in both coordinate systems can occur simultaneously to obtain a classification of singularities of systems of conics, that is to say whether one can have one line and one point, one line and two points, and so on.

To do so, Halphen fixes a straight line Q in the plane of the system, and a point s on Q (see fig. below). Given a conic a , he forms the polar line S of s with regard to a , and the pole q of Q with respect to a . Then the intersection r of the lines Q and S will be such that it is the pole of the straight line sq . In other words, the triangle qrs is self-polar with respect to a . Unless s is on a , or Q touches S , it is a proper triangle: q, r and s are distinct and not aligned. Halphen shows that this remains true when a is converging toward a degenerate conic, provided s and Q are chosen adequately. The intersection points m, m' of the degenerating conic a and the line Q converge toward two, possibly identical limit points, which are different from s (for s is chosen to be on none of the conics near the degenerating conic). The three points s, m, m' are always on the line Q , therefore so is their harmonic conjugate, that is to say the unique point on Q so that its anharmonic ratio, taken with the three other points, is -1 . This point, then, converges toward a point that is not s . But this point is also the intersection of Q and the polar line of s with respect to A . Since this is true of all lines Q , the polar of s converges towards a line which does not pass through s . Similarly, when a degenerates, r does not converge toward a point on R , and q on Q ; so that the triangle corresponding to the degenerate conic remains proper.



Constructing this series of triangles allows Halphen to write the punctual equation of each conic of the system as:

$$g_1 Q^2 + g_2 R^2 + g_3 S^2 = 0$$

where $Q = 0, R = 0, S = 0$ are the equations of the three sides of the self-polar triangle qrs , and the g_i 's are coefficients determining one conic, but also functions of a parameter which describes the entire system of conics. Note that in the equation above, the factors Q, R, S change with each conic, but the general form of the equation remains the same

across the entire system of conics. A tangential equation for each conic can be obtained in a similar fashion by forming the (dual) equations of the points q, r, s .

When a conic degenerates, at least one of the g_i 's vanishes; however, not all of them can vanish at the same time. Supposing, for instance, that g_3 has a finite limit when the conic degenerates (that is to say, supposing that $\frac{g_1}{g_3}$ and $\frac{g_2}{g_3}$ are bounded in the neighborhood of a degenerate conic), there remain three possible cases.

In the first case, only g_1 is infinitely small. The equation of the conic becomes that of two straight lines passing through q , and S intersects the conic at two infinitely close points. Since S is the polar line of the point s , which was chosen arbitrarily, the tangent lines of an arbitrary point touch the conic at two infinitely close points, which are the intersection of the two lines to which the conic is reduced. The conic is composed of two lines and one point; and it is classified as a degeneration of type A .

In the second case, both g_1 and g_2 are infinitely small and of the same order. The equation of the conic becomes that of one line, namely S , on which lie q and r . Since S was chosen arbitrarily, this conic intersects an arbitrary straight line in two infinitely close points, but the tangents drawn from arbitrary points do not coincide. The conic is composed of one line and two points; it is classified as a degeneration of type A' .

In the third case, both g_1 and g_2 are infinitely small, but of different orders¹³⁶. Supposing the order of g_1 to be superior to that of g_2 , Halphen denotes their ratio as $(1 + h)$. The conic presents the specificities of both previous cases: any straight line intersects the conic at two infinitely close points, as is evidenced by the fact that the equation of the conic becomes $S^2 = 0$. Furthermore, the tangents drawn from an arbitrary point coincide, since the correlative equation has also two coefficients vanishing. The conic is composed of one line and one point; it is classified as a degeneration of type B .

Furthermore, by taking the correlative system of conics a' , the degenerations A and A' are swapped, but degenerations B correspond to one another, with the number h becoming its inverse number $h' = \frac{1}{h}$. Halphen is not directly interested in the number h itself, but rather in two other numbers whose ratio is h . When a conic a degenerates, the quantities $\frac{g_2}{g_3}$ and $\frac{g_1}{g_3}$ become infinitely small, and of order m and $m + n$. For conics of type A , $m = 0$. For conics of type A' , $n = 0$. For conics of type B , neither m nor n is zero, and $h = \frac{m}{n}$. Halphen shows that these two numbers do not depend on the mode of representation of the system, and calls them respectively the order and class of the degenerate conic¹³⁷.

¹³⁶Another way to understand B -degenerations, which Zeuthen would resort to in his presentations of Halphen's theory (whether for Meyer's and Klein's *Encyklopädie* or his own textbook), is to view them as conics whose axes a, b become infinitely small in a manner such that the ratio $\frac{a^m}{b^n}$ has a finite limit, for some positive, different integers m, n .

¹³⁷Using Clebsch's construction of an algebraic curve in a one-to-one correspondence with the system of conics, these numbers m and n can be given further meaning; see [Halphen, 1878b], pp.38-39.

The crucial part of Halphen’s analysis here is that B -degenerations are obtained as normal ‘singularities’ of a system of conics. In other words, these degenerate conics are not simply conics satisfying both A and A' modes of degeneration simultaneously, but, in Halphen’s theory, a third, equally legitimate mode of degeneration, on its own right. It must be noted that in order to appear as such, these degenerations must be viewed as limits of proper conics. If just given the description of these conics as made of a straight line and a point, one could explain them away for instance as the intersection of two exceptional loci in the abstract space of conics. This analytical framework, and the considerations of limits with various possible convergence rates, is key to Halphen’s classification¹³⁸.

6.4.4 What’s a (projective) condition?

It is not enough to change the classification of degenerate conics to refute Chasles’ theorem: one must also show that B -degenerations yield *solutions étrangères*, that is to say curves which do not meaningfully satisfy a condition, and thus must be subtracted from the total number of solutions. To that end, Halphen gave an algebraic definition of a condition, and put forth a criterion for the proper satisfaction of a condition by a conic (degenerate or not).

A condition, for Halphen, can always be represented by an algebraic, homogeneous equation $\psi = 0$ in the coefficients $a_{i,j}$ (which Halphen only writes as a) of the general, usual equation of the conic (that is to say, not the one he had constructed to classify degenerate conics). However, such an equation is not necessarily projective¹³⁹, that is to say invariant under any homographic substitution. To remediate this situation, Halphen proposes two slightly different solutions in his memoirs for the *Proceedings of the London Mathematical Society* and the *Journal de l’Ecole Polytechnique*. In what follows, we focus on the latter, but will indicate some deviations from the earlier, England-bound text in footnotes.

Halphen first studies the effects of changes of homogeneous coordinates on the equation $\psi = 0$. To that end, he fixes three arbitrary points u, v, w in the plane, of coordinates u_1, u_2, u_3 etc., and replaces in a_x^2 the coordinates x_1, x_2, x_3 by¹⁴⁰

$$x_1 = (vwX), x_2 = (wuX), x_3 = (uvX)$$

¹³⁸In modern presentations, these degenerate conics are obtained by blowing up the variety of complete conics (that is to say, the closure in $\mathbb{P}^5 \times \tilde{\mathbb{P}}^5$ of the graph of the map which transforms a conic locus into its conic envelope) along the Veronese variety (that is to say, the locus of all conics whose matrices have a determinant equal to 0); see [Casas-Alvero & Xambò-Descamps, 1986], pp.3-6.

¹³⁹Halphen identifies enumerative problems as part of “projective geometry”, [Halphen, 1877], p.149.

¹⁴⁰In the memoir for London, four fixed points in the plane were used.

where the following notation is borrowed from invariant theory:

$$(tuv) = \begin{vmatrix} t_1 & t_2 & t_3 \\ u_1 & u_2 & u_3 \\ v_1 & v_2 & v_3 \end{vmatrix}$$

Similarly, the form a_x^2 has become a quadratic function of the coefficients X_1, X_2, X_3 , which can be written as $A_{1,1}X_1^2 + 2A_{1,2}X_1X_2 + \dots^2$, where the $A_{i,j}$ are coefficients which can all be expressed as algebraic functions of the coefficients $a_{i,j}$ and u_i, v_i, w_i . Denoting $\Delta = (u_1v_2w_3)$, and c the degree of ϕ (in the coefficients $a_{i,j}$), Halphen obtains the following factorization:

$$\Delta^{2c}\psi = \theta(u, v, w)\Psi(u, v, w, A)$$

ψ and Ψ coincide if and only if the same triangles are used as a basis for the homogeneous coordinates in which the condition is expressed in both cases.

To ensure that a condition ψ is projective, Halphen applies a homographic substitution¹⁴¹ of determinant δ to the plane, and writes:

$$X_i = m_iX'_1 + n_iX'_2 + p_iX'_3, \quad u_i = m_iu'_1 + n_iu'_2 + p_iu'_3 \text{ etc.}$$

After some computations, Halphen shows that

$$\theta(u, v, w)\Psi(u, v, w, a) = \theta(u', v', w')\Psi(u', v', w', A')$$

From there follows that Ψ and θ are projective relations; after which Halphen asserts that Ψ is the projective form of the condition ψ . Furthermore, from a simple algebraic proposition it follows that θ is a power of the linear form that is the determinant of the triangle (u, v, w) , so that for an adequately chosen reference triangle, the condition can be written purely as $\psi(u, v, w, a)$, that is to say as a covariant of a_x^2 whose variables are the coordinates of three points u', v', w' . The coordinates of the triangle uvw are what Halphen calls the “arbitraries” of the condition: they capture the data necessary to define the condition.

6.4.5 Eliminating independent solutions

At the end of this analysis, Halphen has analyzed the concept of condition more finely than Clebsch had, or at least the status of the arbitraries of the conditions is more thoroughly described. In particular, Halphen can combine this analytical description of conics with

¹⁴¹Despite Halphen's use of substitutions, no mention of Jordan's 1860 *Traité* is made. Furthermore, Halphen uses the term “groupe” in a way that has nothing to do with groups of substitutions.

that of systems of conics previously described, by substituting to the coefficients $a_{i,k}$ the expressions involving the triangle qrs and the coefficients g_i . Doing so, Halphen obtains

$$\psi(u, v, w, a) = \mathcal{D}^{2\theta} \phi(u, v, w, q, r, s, g_1, g_2, g_3)$$

where \mathcal{D} is the determinant of the triangle qrs , θ some integer depending only on the condition and the triangle uvw , and ϕ is the algebraic function obtained by substituting the variables.

This rewriting, among other things, allows Halphen to discuss the concept of solution of an enumerative problem anew. A solution is a curve in a system, defined by the triangle qrs and the coefficients g_1, g_2, g_3 , satisfying a condition, expressed projectively by ϕ and the triangle uvw ; therefore, the equation

$$\phi(u, v, w, q, r, s, g_1, g_2, g_3) = 0$$

expresses the fact that a curve of a system satisfies a condition.

This framework allows to refresh the demarcation between two types of solutions, which could already be found in the works of Clebsch or Chasles, that is to say the demarcation between proper and false solutions of an enumerative problem. To that end, Halphen presupposes that the condition is “independent” from the system, that is to say that there is no relation between the coefficients of the triangle uvw and those of the system. Thus, fixing a given condition, Halphen can consider the zeros of ϕ that are independent of uvw , that is to say the conics of the system whose coefficients g_i and whose self-polar triangles qrs are such that $\phi = 0$ whatever the numerical values of the coordinates of uvw may be. These independent solutions must be subtracted from the final number of conics in a system satisfying a given condition, for they do not properly satisfy the condition, they are “foreign solutions”, typically exemplified by the point-pair conic which is ‘tangent’ to a given curve because it always crosses it at a single point, irrespectively of the specific position of the given curve.

In fact, Halphen shows that a foreign solution is necessarily a degenerate conic. Taking a conic a of the system, which is a solution independent of the arbitraries of the condition, and to which is associated a non-degenerate triangle qrs , Halphen considers the effect of a homographic substitution, which transforms a into a conic a' , the triangles uvw and qrs into triangles $u'v'w'$ and $q'r's'$ (all of which can be assumed to be proper triangles), but which preserves the ratio of the g_i 's. Therefore,

$$\phi(u', v', w', q', r', s', g_1, g_2, g_3) = 0$$

If a is a zero of ϕ independently of uvw , then a' is a zero of ϕ independently of u', v', w' . Since every conic of the plane can be viewed as a certain homographic transformation of the initial conic a , this would mean that every conic satisfies the condition, that is to say that ϕ is constantly equal to zero, which is only possible when at least two of the g_i 's are already zero¹⁴².

The rest of Halphen's memoir consists in the enumeration of these foreign solutions which must be subtracted. In so doing, Halphen shows that B -degenerations in the system give rise to a number of solutions independent of the data of the condition, which cannot be expressed by an $\alpha\mu + \beta\nu$ formula¹⁴³, as certain conditions are, indeed, satisfied by every B -degeneration, irrespective of the triangle uvw .

Halphen's memoir can be viewed as an even more radical attempt than Clebsch's at a systematic extraction of the algebraic content of the geometrical notions present in Chasles' theory: conditions, systems, degenerate conics, but also the significance of a solution are all attributed equational equivalents. However, through this translation, Halphen had significantly altered these notions, thereby falsifying the formula which guaranteed the very generality of Chasles' theory (that is to say, for Chasles, the very reason for which this theory was so important and worthwhile).

Indeed, how are we to understand that Chasles' observation, verified across hundreds of examples, and over the course of decades of geometrical practice, could be falsified by an example as simple as the one given by Halphen? Since the validity of the $\alpha\mu + \beta\nu$ formula hinges upon the manner in which degenerate solutions are taken into account, this counter-example could not have been an *experimentum crucis*, a case which, once studied, immediately and definitively decides whether the formula is correct or not. To be made into a counter-example, Halphen's condition K and his two systems S and S' must be viewed in a specific way, and the criteria for a conic to be a solution to an enumerative problem must be spelled out in a specific way: in this case, through a certain analytic equation. By mobilizing a language foreign to Chasles', Halphen constructed a novel technology for the computation of numbers of solutions, and in so doing, co-constructed a novel concept of solution.

Conclusions

Over the course of a decade, almost every part of the theory of characteristics had undergone multiple and incompatible transformations; whether it be notations, concepts,

¹⁴²In the memoir for the *Journal de l'Ecole Polytechnique*, these assertions are backed by an analytical study of the function ϕ ; in the other memoirs, however, they are more loosely stated and established.

¹⁴³In a manner similar to Clebsch's, Halphen redefines these characteristic numbers within his algebraic framework.

methods of proof, geometrical meaning, goals and motivations, or even the truth-value of a central theorem. Thus, the history of the early reception of this theory is the history of strategical adaptations, reorganizations, rewritings, but also reinsertions into other theoretical frameworks.

Across this motley collection of continuations of Chasles' theory, some important conceptual questions have emerged. The classification of degenerate conics, their integration into a mathematical language – whether by stipulating what constitutes their incidences and tangencies, by describing them as loci in an abstract space, or by writing equations for them –, the fine-tuned discussion of their status *qua* solution to an enumerative problem: these difficult questions were shared much more widely than methods or notations.

By pursuing these questions by means of literary technologies foreign to Chasles', geometers had uncovered a new kind of epistemic problem. The question of generality, which had been central to Chasles' efforts in the shaping of the theory of characteristics (and of the principle of correspondence as a method of proof), had now been turned upside down. Indeed, Chasles had put forth tools for the systematic enumeration of geometrical objects in absolute generality; but this generality proved to be double-edged. Halphen had definitely shown that there was a real risk of counting meaningless solutions through the blind application of these tools.

However, by the end of the 1870s, these transformations had not led to the stabilization of a collectively-shared theory or discipline. Halphen's refutation of the $\alpha\mu + \beta\nu$ formula had perhaps convinced a wide group of geometers around him (especially in France), but this did not mean that his alternative theory of systems of conics would be developed much further, or that his methods would circulate. In fact, there were many who still held the formula to be valid – or, at the very least, to deserve some more defending. In particular, in the enumerative geometry constructed by Hermann Schubert between 1874 and 1880, the $\alpha\mu + \beta\nu$ formula plays a structural role, both as a paradigmatic example of the kind of mathematical knowledge to be strived for, and as a central justification for the soundness of its notations. In the next chapter, we present the shaping of this geometrical theory, before turning in the last chapter to the ensuing disputes between Schubert and Halphen over the validity of the $\alpha\mu + \beta\nu$ formula.

“A Wonderful Machine”: the shaping of Schubert’s *abzählende Geometrie*

Introduction

While Halphen successively proved and refuted Chasles’ formula, several others were engaging with Chasles’ theory of characteristics with different goals in mind, such as expanding it toward cubics or other geometrical figures. One, in particular, would rapidly gain fame for such work: Hermann Schubert. While his scientific output stopped in the direct aftermath of his 1870 dissertation, he came back to enumerative problems but a few years later. In 1874, he began publishing results on the enumeration of cubics (in the plane as well as in space), which were crowned the following year by the Danish Academy of Sciences in Copenhagen, on Zeuthen’s recommendation¹. Schubert’s research led to the discovery of several impressive solutions of complex enumerative problems, such as the 5 819 539 783 680 twisted cubics tangent to twelve quadric surfaces, in three-dimensional space.

But Schubert’s research is inadequately represented as a mere extension of Chasles’ theory of characteristics for cubics and other figures. The large numbers and complex enumerative formulae Schubert had obtained were built on the back of a profound reinterpretation of the symbols of the theory of characteristics, of their status as geometrical objects, and of the computations which could be carried out with them. In this chapter, we show in succession how Schubert built upon a seemingly banal observation by Halphen to carry out this reinterpretation, as well as how this reinterpretation was used to construct a new proof of Chasles’ formula, and, more substantially, to turn enumera-

¹ [Bureau, 1993], p.63. A quasi-exhaustive list of Schubert’s publications, compiled by his biographer Werner Bureau, can be found in the 1979 reprint edition of his 1879 book *Kalkül der abzählenden Geometrie*, see [Schubert, 1979], pp.18-21.

tive problems into a full-fledged, autonomous mathematical discipline, namely *abzählende Geometrie*. Not only did Schubert coin this term, he delineated its symbolism, its inferential rules, and its goals. These rules and goals would turn out to be orthogonal to those identified by Halphen and Clebsch. Instead of seeking to prove Chasles’ theorem, Schubert put at the heart of his project the generation and combination of symbolic formulas and the reduction of conditions to minimal sets of elementary conditions. Furthermore, Schubert’s algebraic symbols served not as a tool for the thorough and rigorous investigation of geometrical concept; rather, letters representing conditions were endowed with autonomous meaning modeled on an analogy with the algebra of systems of numbers. To these changes in mathematical moves correspond a changes in epistemic values, and to the values of rigor and precision promoted by Halphen, Schubert preferred those of fruitfulness and simplicity. This would appear most clearly in the polemical exchanges between Schubert, Zeuthen, and Halphen; which we will discuss in the next chapter.

Beyond Halphen and Zeuthen, the sheer fruitfulness and efficiency of Schubert’s calculus was admired by many; but the nature of the justifications and proofs Schubert summoned to ground it was widely judged unsatisfactory. The tension between the power of Schubert’s calculus and the growing dissatisfaction regarding its justifications became such that in 1900, in his famous Paris address delivered at the *International Congress of Mathematics*, David Hilbert included the rigorous foundation of this calculus as the fifteenth of his twenty-three problems². While most mathematicians later agreed that such rigorous foundation had been provided by Van der Waerden, in his series of papers on algebraic geometry in the 1920s and 1930s³, it remains to be proven that all of Schubert’s enumerative results can actually be obtained in this more rigorous framework. Furthermore, there are substantial philosophical issues to this claim, which we shall briefly touch on below, and in the next chapter.

Schubert’s mathematics have long fascinated geometers of various ilks. As such, there exist already several reconstructions thereof⁴, written by leading geometers, trained in modern intersection theory and post-Grothendieck algebraic geometry. However, we still lack an understanding of the inner workings of his symbolism. Certain symbolic practices and certain types of proofs, which Schubert constantly uses and puts at the heart of his theory, are difficult to make sense of in these modern readings. Chiefly among them ranks Schubert’s rewriting of Chasles’ principle of correspondence, and the symbolic inferential practice attached to it. Furthermore, these contemporary reconstructions focus almost exclusively on the 1879 *Kalkül*, and largely ignore the slow genesis of this symbolism, with all the difficulties, conceptual or technical, it had to overcome. Therefore, the purpose

² [Gray, 2000], p.178.

³ [Schappacher, 2007].

⁴ [Kleiman, 1976b], [Ronga, 2006].

of this chapter is to present a detailed account of the shaping of Schubert's geometrical practice. To that effect, our strategy in this chapter will be to follow Schubert as he read Chasles, then Halphen, Maillard, and Zeuthen. We then show how Schubert built on top of his original reading, a new way of rewriting, and expanding, these results. To do so, we first present Schubert's highly-structured geometrical ontology, as well as the intricate architecture of his theory - its objects, its notations, and its stated goals. Then, we present the main computational techniques deployed by Schubert, some of which he created through a highly-inventive reading of the texts of Chasles and his students. Lastly, we present the details of Schubert's geometry of the triangle by way of an example of the actual practice attached to this theory.

7.1 Reinventing a meaning for Chasles' symbols

7.1.1 Halphen's observation

On April 28th 1873, Halphen gave a seemingly innocuous communication at the Académie des Sciences, one to which he would not return in the future, and to which he never gave a particularly important role⁵. And yet, as Schubert read it a few months later, it would prove key in opening up the possibility of a new symbolism for geometrical conditions, and for enumerative geometry as a whole.

In this paper, Halphen started with Chasles' $\alpha\mu + \beta\nu$ theorem for conics and its analog for second-order surfaces (which takes the form of a sum $\alpha\mu + \beta\nu + \gamma\rho$, where μ, ν, ρ are numbers of surfaces satisfying elementary conditions), which at the time he both held to be true. Remember that Chasles called these sums 'modules' of a condition (see section 4.2.3), and that he had given general expressions for the number of conics satisfying any five conditions of known modules $\alpha_i\mu + \beta_i\nu$ (for i ranging from 1 to 5). In particular, Halphen recalls Chasles' general expressions for the number of conics (resp. second-order surfaces) satisfying 5 (resp. 9) conditions whose modules are known. These expressions were linear combinations of products of the coefficients α, β, γ of these modules (see fig. below⁶).

⁵ [Halphen, 1873c]. In Halphen's 1885 autobiographical *Notice*, only one sentence is devoted to this paper: "Enfin, je demeurais l'inventeur de certains *produits symboliques*, grâce auxquels tous les résultats de cette théorie acquéraient une admirable simplicité", [Halphen, 1885], p.8. A footnote indicates that Schubert, in particular, made use of these symbolical products.

⁶In Chasles' text, expressions such as $\Sigma 5\alpha 4\beta$ refer to the sum of all possible products of one coefficient from each of the nine conditions, wherein four coefficients have to chose amongst the α_i 's and five amongst the β_i 's. For instance, $\Sigma 9\alpha$ is simply $\alpha_1 \times \dots \times \alpha_9$, and $\Sigma 8\alpha 1\beta$ is $\sum_{i=1}^9 (\beta_i \prod_{j \neq i} \alpha_j)$.

$$\begin{aligned}
 N(9Z) = & 1.(9\alpha) + 2\Sigma 8\alpha.1\beta + 4\Sigma 7\alpha.2\beta + 8\Sigma 6\alpha.3\beta + 16\Sigma 5\alpha.4\beta \\
 & + 32\Sigma 4\alpha.5\beta + 56\Sigma 3\alpha.6\beta + 80\Sigma 2\alpha.7\beta + 92\Sigma 1\alpha.8\beta + 92\Sigma 9\beta \\
 & + 3\Sigma 8\alpha.1\gamma + 9\Sigma 7\alpha.2\gamma + 17\Sigma 6\alpha.3\gamma + 21\Sigma 5\alpha.4\gamma + 21\Sigma 4\alpha.5\gamma \\
 & + 17\Sigma 3\alpha.6\gamma + 9\Sigma 2\alpha.7\gamma + 3\Sigma 1\alpha.8\gamma + 92\Sigma 8\beta.1\gamma + 80\Sigma 7\beta.2\gamma \\
 & + 56\Sigma 6\beta.3\gamma + 32\Sigma 5\beta.4\gamma + 16\Sigma 4\beta.5\gamma + 8\Sigma 3\beta.6\gamma + 4\Sigma 2\beta.7\gamma \\
 & + 2\Sigma 1\beta.8\gamma + 6\Sigma 7\alpha.1\beta.1\gamma + 12\Sigma 6\alpha.2\beta.1\gamma + 24\Sigma 5\alpha.3\beta.1\gamma \\
 & + 48\Sigma 4\alpha.4\beta.1\gamma + 80\Sigma 3\alpha.5\beta.1\gamma + 104\Sigma 2\alpha.6\beta.1\gamma + 104\Sigma 1\alpha.7\beta.1\gamma \\
 & + 18\Sigma 6\alpha.1\beta.2\gamma + 36\Sigma 5\alpha.2\beta.2\gamma + 72\Sigma 4\alpha.3\beta.2\gamma + 112\Sigma 3\alpha.4\beta.2\gamma \\
 & + 128\Sigma 2\alpha.5\beta.2\gamma + 104\Sigma 1\alpha.6\beta.2\gamma + 34\Sigma 5\alpha.1\beta.3\gamma + 68\Sigma 4\alpha.2\beta.3\gamma \\
 & + 104\Sigma 3\alpha.3\beta.3\gamma + 112\Sigma 2\alpha.4\beta.3\gamma + 80\Sigma 1\alpha.5\beta.3\gamma + 42\Sigma 4\alpha.1\beta.4\gamma \\
 & + 68\Sigma 3\alpha.2\beta.4\gamma + 72\Sigma 2\alpha.3\beta.4\gamma + 48\Sigma 1\alpha.4\beta.4\gamma + 34\Sigma 3\alpha.1\beta.5\gamma \\
 & + 36\Sigma 2\alpha.2\beta.5\gamma + 24\Sigma 1\alpha.3\beta.5\gamma + 18\Sigma 2\alpha.1\beta.6\gamma + 12\Sigma 1\alpha.2\beta.6\gamma \\
 & + 6\Sigma 1\alpha.1\beta.7\gamma + 1(9\gamma).
 \end{aligned}$$

[Chasles, 1866d], p.413.

Halphen had observed that, in these sums, the numerical coefficients could be read as numbers of conics (resp. surfaces) satisfying certain numbers of elementary conditions⁷. More precisely, the numbers could be interpreted in a way that factored Chasles’ expression into a product of modules.

For instance, given five conditions on conics, of modules $\alpha_i\mu + \beta_i\nu$, Halphen formed the following product by replacing the characteristics μ and ν by p and d , two empty symbols (which Halphen refers to as ‘variables’):

$$(\alpha_1p + \beta_1d) \times \dots \times (\alpha_5p + \beta_5d)$$

By replacing, in the expanded form of this product, every symbol $p^i d^{5-i}$ by the number of conics passing through i given points and touching $5 - i$ given straight lines, Halphen explained, one obtains exactly Chasles’ general formula for the number of conics satisfying five conditions. The same rewriting works for the much larger expression for the number of surfaces.

In fact, as Halphen had noticed, this phenomenon is more general: in particular, it applies to conditions of higher multiplicity (or, to use Halphen’s words, higher ‘order’). Remember that Chasles had extended the notion of ‘module’ to the so-called ‘multiple conditions’⁸. Conditions on conics such as ‘to touch a given line at a given point’, or ‘to have a contact of the second order with a given curve’ could not be attributed a module of the form $\alpha\mu + \beta\nu$, for the simple reason that they counted as two or more inseparable conditions, and thus there are no conics in a system of conics $(Z_1, Z_2, Z_3, Z_4) \equiv (\mu, \nu)$ which

⁷Note that a similar trick had previously been given by [Prouhet, 1866], p.202. This paper is never mentioned by Schubert or Halphen. To an extent, the general expression for the number of second-order surfaces satisfying 9 conditions given by Schubert in his dissertation, and discussed in the previous chapter, is also very comparable to the result stated by Halphen in the case of simple conditions. See [Schubert, 1870], p.370, and 6.2.2.

⁸ [Chasles, 1864e]. See section 4.2.6.

satisfy Z in general. To deal with multiple conditions, Chasles then extended the concept of characteristics beyond systems defined by four conditions. Given three arbitrary conditions Z_1, Z_2, Z_3 , Chasles formed the two systems $(1p., 3Z) \equiv (\mu', \nu'), (1d., 3Z) \equiv (\mu'', \nu'')$. These are systems in the usual sense of the term, and thus they also have characteristics in the usual sense of the term. Note that, per definition, $\mu'' = \nu'$. Chasles would then claim that $\mu', \nu' = \mu'', \nu''$ could then be called the three characteristics of the triplet of conditions $3Z = (Z_1, Z_2, Z_3)$, because for every double condition C_2 , the number of conics satisfying $3Z$ and C_2 was given by a formula of the form:

$$N(3Z, C_2) = \alpha\mu' + \beta\nu' + \gamma\nu''$$

where α, β, γ only depend on C_2 . The right-hand term in this equation would then be called the module, in an extended sense of the term, of the double condition C . For instance, the module of the double condition ‘to have a double contact with a given conic section’ is $(\mu' + \nu'' - \frac{1}{2}\nu')$. This means that for any given three conditions Z_1, Z_2, Z_3 , one can compute the three characteristics defined above, and, using this module, find out how many conics satisfy $3Z$ and C_2 . The general existence of these modules – that is to say, the analogous version of Chasles' theorem for systems defined by three conditions – is often referred to as Cremona's theorem. It must be emphasized here that Halphen, in the second part of his 1873 memoir on the theory of characteristics, had also proven Cremona's theorem, which he described as “following easily from Chasles' theorem⁹”. Similar generalizations of the notion of module can be easily carried out to conditions of higher multiplicity: for a triple condition C_3 , it suffices to form the characteristics of the four elementary systems $(2p., C_2), (1p., 1d., C_2), (2d., C_2)$ for an arbitrary double condition C_2 .

Considering various kinds of double conditions, Chasles had also given expressions similar to that for the general number of conics satisfying five simple conditions. To these expressions, Halphen would notice, the same notational trick applied; except now the formula was quadratic, and not linear, in the coefficients of the module. It is on this basis that he stated the following general theorem¹⁰:

Pour les coniques dans le plan, toute condition multiple d'ordre π peut être caractérisée par un polynôme homogène et de degré π , à deux variables p, d ,

⁹ [Halphen, 1873b], p.131.

¹⁰“For plane conics, every multiple condition of order π can be characterized by a homogeneous polynomial in two variables p and d , of degree π , which is called its module. If one replaces, in this polynomial, each symbol $(p^i d^{\pi-i})$ by the number of conics passing through i points, touching $\pi - i$ straight lines, and satisfying another multiple condition, of order $5 - \pi$, the result of this substitution is the number of conics which satisfy these two multiple conditions”, [Halphen, 1873c], p.1075. An analogous result for surfaces is given on the next page.

nommé module. Si l’on remplace, dans ce polynôme, chaque symbole $(p^i d^{\pi-i})$ par le nombre des coniques qui passent en i points, touchent $\pi - i$ droites et satisfont à une autre condition multiple, d’ordre $5 - \pi$, le résultat de cette substitution est le nombre des coniques qui satisfont à ces deux conditions multiples.

The module of the condition ‘to have a double contact with a given conic section’, which Chasles had already computed in 1864 (and given above), would be rewritten by Halphen as $(p^2 + pd - \frac{1}{2}d^2)$.

Halphen did not prove this general result either in this paper, or in later publications. In its simpler cases, such as that of a collection of simple conditions, his theorem can be obtained through simple, albeit tedious, computations. However, it remains unclear if Halphen was in possession of arguments for the generality of this claim, or if this was based on a induction (on, admittedly, a large number of cases). Notably, Chasles had not given a general formula for the number of conics satisfying a double and a triple condition.

If Halphen had a proof for it, however, it did not circulate. Thus, the import of this theorem in its public form was a reorganization of the knowledge already present in Chasles’ formula. Halphen’s rewriting makes visible symmetries and relations between modules that were not apparent under Chasles’ notations; and, in particular, the use of the variables p and d condenses information that was distributed across the symbols μ', ν', ν'' . The synthetic power of Halphen’s observation is obvious in a second theorem he gave in the same communication, which immediately follows from the first one¹¹:

Le module d’une condition composée est le produit des modules des conditions composantes. Et, en particulier, le nombre des coniques qui satisfont à des conditions, dont la somme des ordres de multiplicité est égale à 5, est représenté par le produit symbolique des modules de ces conditions, dans lequel chaque symbole $(p^i d^{\pi-i})$ est remplacé par le nombre des coniques qui passent en i points et touchent $5 - i$ droites.

The composition of a condition is now recast by Halphen as a symbolic product, and the condition ‘to have two contacts with a given curve U_m ’ can be meaningfully expressed in terms of the simple condition ‘to have a contact with a given curve U_m ’. This is something of which Chasles’ theory was not really capable, mostly due to a lack of appropriate expressive resources. However, despite how suggestive this comparison between composition and multiplication is, Halphen would limit himself to drawing direct consequences

¹¹“The module of a multiple condition is the product of the modules of the conditions composing it. And, in particular, the number of conics which satisfy conditions whose orders of multiplicity add up to five is represented by the symbolic product of the modules of these conditions, in which each symbol $(p^i d^{\pi-i})$ is replaced by the number of conics passing through i points and touching $5 - i$ straight lines”.

of Chasles' general formulae, and to reformulating them. In particular, the symbols p and d are devoid of (geometrical) meaning: they play here a purely instrumental role. Furthermore, these symbolic products are justified, and thus restricted to cases where a formula such as Chasles' $\alpha\mu + \beta\nu$ has been proven. These two limits would disappear in Schubert's interpretation of Halphen's observation, thus allowing for an entirely new approach to the algebra of geometrical conditions.

7.1.2 From multiplications of modules to symbolic products of conditions

This observation would be of little interest for Halphen in his later research. It is a symbolic device which relies on the assumption that Chasles' formula is true for five given conditions, thus being of no use for proving or refuting said formula. At first glance, its interest seemingly lies more in its computational efficiency than in theoretical uses. And yet, Schubert would be very impressed by this device, which he described as “marvelous machine¹²” in a letter to Halphen; and he would push this machine far beyond its intended uses. In his first 1874 paper for the *Göttingen Nachrichten*, marking his comeback to mathematical research after a three-year pause, Schubert built on Zeuthen's and Maillard's work on the theory of characteristics for spatial curves of the third order¹³. This first successful foray into the enumerative geometry of cubics had been motivated by exchanges with German mathematician Rudolf Sturm who, without explicitly referring to Chasles' theory, had also been making some progress toward a classification of elementary systems of curves of the third order¹⁴, and was closely read by Schubert throughout the 1870s. Along with a second paper, published the following year and in the same journal¹⁵, Schubert's enumerative theory of cubics would be rewarded with a prize in 1875 from the Danish Royal Academy, on Zeuthen's recommendation: as such, this work immediately gained international recognition¹⁶. And yet, the profound changes it brings to Chasles' theory, and to the works of Halphen, Maillard, or Zeuthen, are striking. At the heart of these changes lies a new symbolism introduced by Schubert, and for which Halphen's observation was crucial.

In the first of these two papers, Schubert focuses on elementary systems of C_3^3 curves, that is to say plane curves of the third order in three-dimensional space¹⁷. Schubert

¹²“Die symbolische Multiplication, welche übrigens doch schliesslich von Ihnen herstammt, ist in der That eine prachttvolle Machine”, Letter Ms 5624 165, Schubert to Halphen, dated May 18th 1876

¹³ [Schubert, 1874]. See 6.2.1, 6.2.2.

¹⁴For instance, [Sturm, 1875].

¹⁵ [Schubert, 1875].

¹⁶ [Schubert, 1879b], p.339. The winning paper was not directly published, but its results served as the basis for later texts, in particular [Schubert, 1878] and [Schubert, 1879b], pp.163-165.

¹⁷Every curve is in a plane, but curves in a given system may be in different planes. Schubert also

asserts that the special case for which the plane of each curve in the system is the same is precisely what Zeuthen and Maillard have already dealt with. He presents his own work as a generalization of their work for the case in which the plane in which each curves lies can vary. For curves of the third order, Schubert lets Σ_n denote elementary systems of curves which satisfy $10 - n$ elementary conditions. It is important to note that Schubert, from the onset, defines systems as collections of curves satisfying certain conditions, whose total multiplicity may not add up to $10 - 1 = 9$. Unlike for Chasles, thus, a system of cubics could be defined by, say, three simple conditions – it would then be called a system of level 3. For any three numbers x, y, z whose sum a is smaller than 10, (x, y, z) denotes the elementary system of curves Σ_{10-a} whose plane contains x given points, which cross y given lines, and which touch z given planes.

Schubert moves on to discuss the possible degenerate curves of such system. A system Σ_1 contains degenerate curves, such as the figure σ , which is composed of a conic and a ‘branche simple’ (or *Ordnungsgerade*, Schubert giving here both the French and the German term) – that is to say, a cubic whose equation can be factored into the product of a quadratic and a linear polynomials. Schubert’s crucial notational move is to let the same ‘sign’ (*Zeichen*) σ also represent the number of such degenerate curves in the system¹⁸:

Von den nachstehenden Zeichen bedeutet jedes entweder die daneben stehenden Bedingung oder die Zahl der ihr genügenden Curven eines Systems.

Relying on the enumerative geometry of the cubic expounded by Zeuthen and Maillard in particular, Schubert gives a complete classification of the possible modes of degeneration of cubics, which he all denotes with similar signs (lowercase Greek letters). There again, the signs always stand for the numbers of such curves as well. Similarly, Schubert lists the degenerate curves that appear in Σ_2 systems, such as an infinity (∞^1) of curves σ . This shows that signs denote numbers regardless of whether or not there are finitely many such degenerate curves in the system; in other words, infinities of figures (∞^1, ∞^2 etc.) are also numbers of curves.

The next step in the setting-up of Schubert’s symbolism is to denote geometrical conditions. Here again, Schubert introduces signs which stand both for the condition and the number of curves of a system satisfying it, for instance denoting μ the condition for a curve of having its plane pass through a (given) point. For each condition, Schubert gives a second interpretation of the number of curves satisfying it; for instance, μ is a third of the order of the system of rays generated by the tangent lines of the curves¹⁹. Conditions

touches on curves of the fourth order in this paper. For the sake of brevity, we will not discuss this aspect of his work.

¹⁸“Each of the following signs means either the condition next to it or the number of curves in a system which satisfy this condition”, [Schubert, 1874], p.270.

¹⁹Tangents can be defined because the curve is planar.

are not all denoted by Greek lowercase letters. For instance, the condition ‘to contain a given point’ is denoted by the letter P . Some conditions also involve subscripts. For instance, c denotes the condition of having its cusp in a given plane, or, alternatively, the order of the locus of the cusps of the curves of the system. Later, Schubert defines c_g as the condition of having its cusp on a given straight line²⁰. Schubert’s list of conditions is divided into four sections, which correspond (implicitly) to conditions of order 1, 2, 3, and 4; conditions of higher orders are not necessary because of duality.

Once these signs are introduced, Schubert can introduce a crucial operation on them; namely a Halphen-inspired symbolic product of these signs. Discarding the instrumental role of Halphen’s multiplication of module - and the fact that it was, ultimately, to be understood as the multiplication of polynomials -, Schubert suggests that²¹:

Die Nebeneinanderstellung – das symbolische Produkt – mehrerer der eben angeführten Zeichen bedeutet die Zahl die Curven eines Systems, welche gleichzeitig den diesen Zeichen angehörigen Bedingungen entsprechen; und das Produkt dieser Zeichen mit einem der sich auf die singulären Curven beziehenden Zeichen bedeutet die Zahl derjenigen durch letzteres dargestellten singulären Curven eines Systems, welche den durch die anderen Zeichen des Produktes angegebenen Bedingungen genügen.

In Schubert’s enumerative geometry of the cubic, one can multiply signs for conditions at will, provided the total dimension be below 10. However, condition-signs can only be multiplied by one sign standing for singular curves. Schubert immediately gives examples of such symbolic products, such as $\mu\mu\mu$ which stands for the number of curves in a Σ_3 which lie in a given plane. Indeed, μ stood for the condition that the plane of a curve contains a given point. Thus, $\mu\mu\mu$ stands for the triple condition that the plane of a curve contains three given points. Three points define one and only one plane; therefore, geometrically, this condition is equivalent to demanding that a curve lie in a given plane. The juxtaposition of condition-symbols thus formed a new composed condition, whose geometrical meaning is obtained by the conjunction of the simple conditions.

This idea of such a symbolic product is credited by Schubert to Halphen in a footnote to this very passage, without much in the way of explanation. It is easy to see how Halphen’s rewriting of the general formula for the number of conics satisfying five given conditions

²⁰Cubic curves can only have one cusp. If the curve does not have a cusp, the condition is not satisfied. This is, like many other geometrical reasonings, totally implicit in Schubert’s presentation.

²¹“The juxtaposition - the symbolic product - of several of the aforementioned signs denotes the number of curves in a system which simultaneously satisfy the conditions which belong to these signs; and the product of these signs with one of the signs referring to singular [degenerate] curves denotes the number of such singular curves in a system represented by the latter signs, which satisfy the conditions indicated by the other signs of the product”, [Schubert, 1874], pp.272-273.

falls under Schubert’s symbolism. Indeed, given five conditions of modules $(\alpha_i\mu + \beta_i\nu)$, these modules are in fact ‘numbers’ of conics satisfying the conditions themselves, as μ and ν are signs standing both for numbers and elementary conditions. The ‘symbolic product’ of these modules thus is interpreted as the number of conics satisfying simultaneously these five conditions. This product expands into an expression where the $\mu^i\nu^j$ (for $i + j = 5$) will turn into concrete, finite integers in the exact manner described by Halphen. However, it must be stressed here that Halphen precisely did not multiply modules of the form $(\alpha\mu + \beta\nu)$, but instead substituted variables p and d to the characteristics μ and ν . These variables were then to be replaced by concrete numbers at the end of the multiplication of modules. The same goes for the general procedure described in Halphen’s theorem, whereby modules of conditions of any order were written with added variables instead of characteristic numbers, only to be replaced by concrete numbers later on. Schubert, by comparison, re-established the symbols for characteristics in the modules, and expanded the symbolic products thus obtained. In Schubert’s symbolism, the symbols μ, ν stand not only for characteristics, that is to say numbers of curves satisfying elementary conditions, but also for these very conditions themselves. Thus, when products of modules are expanded, the symbols $\mu^i\nu^j$ whose total degree is equal to level of the system in which computations take place, they are immediately to be interpreted as concrete numbers, in a way that coincides with what Halphen had described.

With this rewriting, several of the checks that Halphen had put in place on his symbolic manipulation would disappear in Schubert’s text. For once, Schubert is applying Halphen’s observation to the geometry of cubics, wherein no equivalent to Chasles’ $\alpha\mu + \beta\nu$ theorem was known, let alone proven. Furthermore, the ambiguity of Schubert’s symbols between conditions and numbers imbues them with a certain autonomy, far removed from what Halphen had envisioned. For instance, Schubert writes sums with them, which he explains derive from Chasles’ principle of correspondence²². For instance, in a system Σ_2 , Schubert writes²³:

$$4\nu = \rho + 3c + 6\mu$$

where μ, c are defined as above, ν means to intersect a given line, and ρ to touch a given plane. Schubert gives no proof or explanation for this formula which, he claims, is already in the works of Maillard and Zeuthen (for the case $\mu = 0$). In Maillard’s thesis, in a manner analogous to Chasles’, symbols such as μ stood for numbers (either numbers of curves in a system, or the order or the class of a locus defined by a certain infinite collection of curves in a system), but not for conditions. The plasticity of Schubert’s notation allows him to import Maillard’s proofs, which indeed rely on Chasles’ principle of correspondence,

²²We shall come back to Schubert’s interpretation of the principle of correspondence below, see 7.3.2.

²³ [Schubert, 1874], p.273.

while imbuing their end formulae with renewed signification²⁴. And yet, at this stage, the sum of two signs remains an undefined operation: unlike multiplications, sums are not given any geometrical meaning. It is hard to see what in Halphen's theorem could justify adding modules or conditions.

There are other methods through which Schubert can obtain formulae involving his symbols, in particular the so-called 'principle of special position', which we shall discuss later in this chapter²⁵. It suffices to say for now that, in this paper, the results sought by Schubert are of two kinds: characteristics of elementary systems (and, consequently, numbers of cubics satisfying elementary conditions), and dependencies between these symbols. The first kind of result is, of course, very much the same as the results produced by Zeuthen and Maillard, and we shall not delve in detail into Schubert's computation of these numbers. The second kind of result, however, is rather new. After producing the list of equations following from the principle of correspondence, Schubert reorganizes it in order to make apparent that the numbers of degenerate conics depend solely on characteristic numbers (see fig. below, where σ, c, w, q, v correspond to singular conics arising in systems Σ_1 and Σ_2). Maillard did similar operations on his equations in his 1871 dissertation, but never explicitly discussed the relative dependency of symbols, nor did he try to find minimal subsets of letters from which all others could be expressed.

$$\begin{aligned}
1) \quad & 4\nu = \varrho + 3c + 6\mu \\
2) \quad & 4\varrho = \nu + 3w \\
3) \quad & \nu + 3w = 3v + \sigma + 3\mu \\
4) \quad & \varrho + 3c = 3q + \sigma \\
5) \quad & \nu + 3q = 3c + \sigma + 3\mu \\
6) \quad & \varrho + 3v = 3w + \sigma \\
7) \quad & \sigma = \frac{1}{2} (\nu + \varrho - 3\mu) \\
8) \quad & c = \frac{1}{3} (4\nu - \varrho - 6\mu) \\
9) \quad & w = \frac{1}{3} (4\varrho - \nu) \\
10) \quad & q = \frac{1}{6} (7\nu - \varrho - 9\mu) \\
11) \quad & v = \frac{1}{6} (7\varrho - \nu - 3\mu)
\end{aligned}$$

[Schubert, 1874], p.273.

After providing lists of equations following from the principle of special position, Schubert combines the formulas provided by these two principles, and carries out some algebraic operations on them such as multiplying both sides by the same sign. In so doing, he obtains other dependency results: he shows for instance that the numbers of some singularities

²⁴ [Maillard, 1871], pp.8-10. Maillard's formulae are not exactly the same as Schubert's.

²⁵ See in particular 7.3.1.

arising in higher-dimension systems cannot be determined solely by the characteristics. However, Schubert continues, “every number can be represented as a function of μ, ν, ρ and only one of [these numbers]²⁶”. This shows that, in the case of cubics in space, the analog to Chasles’ theorem involves four terms (the numbers of conics in space can all be represented as a function of the three characteristics). It is striking that this result contradicts, or rather invalidates Halphen’s theorem in the case of spatial cubics, which Schubert notices and even mentions²⁷:

Daher muss der oben in einer Anmerkung erwähnte Satz des Herrn Halphen von dem Produkte der Moduln für die Curve höherer Ordnung eine wesentliche Modification erleiden.

Schubert built symbols for the study of spatial cubics upon a result pertaining to conics, whose analog for cubics turned out to be invalid – that is to say, the $\alpha\mu + \beta\nu$ formula must be expanded to four terms for these curves, as one cannot hope to find two elementary conditions which would allow for a simple expression of all other modules of conditions. Halphen’s observation, therefore, was not understood by Schubert as logically grounding a certain use of symbols, but rather as suggesting a way to amalgamate numbers and conditions within a single set of signs and symbols.

7.1.3 The building of a theory

Schubert’s output in the wake of this initial paper on cubics is intense: between 1874 and 1880, besides the aforementioned book, he published nine papers for the *Mathematische Annalen*, three in *Crelle’s Journal*, and six more in the *Göttingen Nachrichten*. The vast majority of these papers, to some extent, make use of the symbolism described previously. Of course, this is not to say that Schubert’s geometry had been set in stone: the strategy and techniques laid out in the initial 1874 paper would be refined and transformed in the following years. The domain on which this new symbolism and its rules would prove useful for Schubert extended way beyond the enumeration of curves, as bespeaks the wide variety in topics in these papers, ranging from the conservation of the genus by some transformations to Bézout’s theorem or to the modes of singularity for complexes of lines (that is to say, the ways in which complexes of lines can degenerate).

In 1874, as he first expounded his new symbolism and operations, Schubert was still working within a Chaslesean framework, and his work could be described as belonging to

²⁶ [Schubert, 1874], pp.273-274. The numbers to which Schubert alludes here are the numbers of curves satisfying given conditions, which already hints at the notion that, for Schubert, conditions are none other than ‘geometrical numbers’, especially when viewed in a system of adequate dimension. We shall come back to this point in section 7.2.1.

²⁷“Therefore, M. Halphen’s theorem on the products of modules of higher-order curves, mentioned above in a footnote, must undergo a substantial modification”, [Schubert, 1874], p.278.

the theory of characteristics. Throughout the following years, Schubert built a distinct full-fledged mathematical theory, with its clearly delineated set of rules, goals, and notations. Eventually, Schubert would identify what he had built as “enumerative geometry” (*abzählende Geometrie*), that is to say a novel, distinct branch of mathematics²⁸. In so doing, Schubert rapidly subverted the relation between his enumerative geometry and the theory of characteristics, as the latter became a part of the former.

This is not the place to conduct an exhaustive survey of the many steps by which Schubert proceeded in the shaping of this theory. Rather, we shall identify some turning points in Schubert's understanding of what the central concepts and questions of this new theory were, and sketch the architecture of this theory as it was exposed in the 1879 book *Kalkül der abzählenden Geometrie*. This book, in many regards, served as Schubert's attempt at fixing once and for all the basic concepts, methods, and goals of a newly-created theory; and it sums up many of the results previously obtained, and whose circulation was much greater than that of Schubert's other papers²⁹. Only then will we come back to the issue of understanding the principles which underlay Schubert's geometrical practice, and, in particular, how the obtention of new enumerative formulae within this new symbolism was carried out.

The crucial turn in Schubert's geometry is undoubtedly the publication in 1876 of the first “Beiträge zur abzählenden Geometrie” (*Contribution to enumerative geometry*) in the *Mathematische Annalen*. This very long paper (116 pages) was initially announced as the first of a three-part series. The second part was published in the same journal in 1878 as another lengthy paper (110 pages) entitled “Die fundamentalen Anzahlen und Ausartungen der cubischen Plancurven nullten Geschlechts” (*The fundamental numbers and degenerations of planar cubics of genus zero*). Instead of a complete third part, Schubert published in 1879 a much shorter paper (4 pages) entitled “Beschreibung der Ausartungen der Raumcurve dritter Ordnung” (*Description of the degenerations of spatial*

²⁸While he had initially envisaged naming this theory “geometry of the number”, Schubert eventually coined his theory “abzählende Geometrie” after the term in use in German journals and books for descriptive geometry (*darstellende Geometrie*): “Der Ausdruck abzählende Geometrie ist wohl sonst noch nicht angewandt. Wohl aber war schon üblich zu sagen, ‘Abzählungsmethoden, z.B. die v. Chasles, v. Voss etc.’ Ich hatte in die ‘Fortschritten der Mathematik’ den Namen ‘Geometrie der Anzahl’ hineingebracht. Doch gefällt mir dieser Ausdruck jetzt nicht mehr, und es soll jetzt auch dort dieses Capitel mit ‘abzählende Geometrie’ überschrieben werden. Dieser Ausdruck ist dem sehr gebräuchlichen Ausdruck ‘darstellende Geometrie’ (z.B. v. Fiedler) nachgebildet. ‘Géométrie numérique’ scheint mir die einzig mögliche Übersetzung zu sein”, Cod Ms 5624 166, Letter Schubert to Halphen, dated May 21th 1876.

²⁹A systematic study of the global reception of this book is still much needed. However, to support this claim, one can point to the fact that this book would still be used as an introductory textbook by mathematicians well into the 20th century: for instance, De Vries recommended that his student Van der Waerden read it in the 1920s, see [Schappacher, 2007]. In section 8.3.1, we will discuss Eduard Study's initiation to enumerative geometry in Leipzig in 1885, which also began with a reading of Schubert's book under Felix Klein's advice.

curves of third order)³⁰. The abandonment of this project and the incomplete state of the third paper is most likely due to the imminent publication of Schubert’s book *Kalkül der abzählenden Geometrie* toward the end of the year 1879, wherein the content of these papers was gathered and systematized.

In the first of these *Beiträge*, Schubert identifies Chasles’ theory of characteristics (as well as his own contribution for the theory of cubics) as two analogous investigations, part of one single, broader theory. Indeed, at the beginning of the paper, Schubert presents Chasles’ theory of characteristics as a “well-known enumerative method³¹”, whose invention had been made possible by three crucial “moments” (*Momente*); namely the discovery of the principle of correspondence, the discovery of the $\alpha\mu + \beta\nu$ theorem (which, at this point, Schubert held to be valid and proven by Clebsch, Lindemann and Halphen), and the possibility of determining conic-numbers via the knowledge of both numbers of conic-degenerations (that is to say the number of point-pairs and the number of line-pairs), first expressed in Zeuthen’s dissertation³². The subsequent results obtained by Maillard, Zeuthen, and Schubert himself are then presented as further explorations and expansions of this initial enumerative method. Not all results pertaining to the enumeration of geometrical objects – or even just conics, for that matter – belong to this narrative: for instance, Schubert explicitly excludes the numbers obtained by Rudolf Sturm in a contemporary series of papers from this historical development. Sturm had given, like Schubert and Zeuthen, the characteristics of systems of cubics, even employing the term ‘Charakteristiken³³’; however, for Schubert, Sturm’s numbers derived from “direct enumeration”, and as such do not belong to this narrative of the development of Chasles’ and Zeuthen’s “enumerative theory³⁴”. Thus, what Schubert identified as the end-goal of his research was not merely to solve a set of similar problems, but rather to build a general theory and a general method of enumeration – of which Chasles’ theory of characteristics would only be a part, or rather, an application to the specific case of (planar) conics. To lay out the foundations of this theory, which he called “abzählende Geometrie”, would be the main purpose of this paper.

The first section of the 1876 *Beiträge* introduces the general terminology and symbolism of the theory, then lays out concepts such as “fundamental figures” (*Grundgebilde*) and “fundamental conditions” (*Grundbedingung*), wherefrom general concepts of figure and condition are derived. This *modus operandi* echoes Schubert’s 1870 dissertation, dis-

³⁰ [Schubert, 1876a], [Schubert, 1878], [Schubert, 1879a].

³¹ [Schubert, 1876a], p.1.

³² See 6.2.1.

³³ [Sturm, 1875]. Schubert was an attentive reader of Sturm’s papers, which he occasionally quoted in his own work. The methods employed by Sturm, however, include none of Chasles’, Zeuthen’s, or Schubert’s.

³⁴ [Schubert, 1876a], p.4.

cussed in the previous chapter³⁵, which also opened with a definition of the figures at play based only on fundamental concepts³⁶ (points, rays, planes, intersections etc.). Once these basic symbols and concepts are laid down, Schubert proceeds to show how general equations between fundamental conditions can be obtained, using the principle of special position³⁷. The third and longest section of the paper introduces the concept of “pairs of (main) elements” (*Paare von Hauptelementen*) and their coincidences (that is to say, conditions whereby two elements in a pair are required to coincide), which are then used to reformulate Chasles' principle of correspondence; which Schubert then applies systematically to various systems and various kinds of figures. In so doing, he obtains other long lists of formulae, specific each time to the figure at hand – by contrast with the equations obtained by working on fundamental conditions (or figures), which ought to serve in the investigation of any geometrical figure whatsoever³⁸. In particular, the end of this last section shows how this general theoretical apparatus, and the formulae obtained throughout the paper, allow for a complete “theory of characteristics of the point, the plane, and the ray³⁹”. What Chasles has done for systems of conics (or Halphen with systems of lines) is now interpreted as showing that all condition-signs can be reduced to functions involving only two fundamental signs (namely μ and ν). To show that his theory can do the same for fundamental objects (albeit with different collections of fundamental signs, of possibly different cardinals) thus remains of importance for Schubert, especially inasmuch as, by using fundamental figures as the building blocks of a general concept of figure, these results suggest a way toward theories of characteristics for each and every possible figure.

This theoretical architecture would not be entirely preserved in the 1879 *Kalkül der abzählenden Geometrie*. In his book, Schubert also opened with a series of definitions of fundamental objects and the exposition of an entire symbolism for these objects, then discussed separate kinds of formulae and the means to obtain them, before concluding on the ideal form the enumerative theory of a specific figure should take. However, the kinds of formulae identified in each text do not coincide, nor do the places respectively occupied by the principle of special position (which, by 1879, Schubert had completely reformulated, and renamed into the “principle of conservation of number”, a term still in use today) and the principle of correspondence. In what follows, we go over the main steps in the construction of Schubert's enumerative geometry, each time comparing the presentations from 1876 and 1879.

³⁵See 6.2.2.

³⁶ [Schubert, 1876a], pp.8-23.

³⁷ [Schubert, 1876a], pp.23-48.

³⁸ [Schubert, 1876a], pp.48-116.

³⁹ [Schubert, 1876a], p.114.

Our presentation of Schubert’s *abzählende Geometrie* (or at least the versions thereof he published throughout the 1870s) will be divided into two sections. First, we focus on the architecture of this theory, that is to say the set of fundamental concepts, notations, and goals which Schubert puts at the core of enumerative geometry. In a second section, we shall discuss how these concepts and symbols can be combined to actually yield computations and results.

7.2 The architecture of Schubert’s enumerative geometry

7.2.1 The objects of enumerative geometry

The first pages of Schubert’s 1874 theory of cubics were devoted to a succinct definition of systems of curves, as well as to a list of elementary conditions (which were not explicitly identified as such) and modes of degeneration; all of which were attributed a sign. The 1876 *Beiträge*, on the contrary, begin with a general presentation of a set of four “objects” (*Objecte*), described as “the most important *objects* of enumerative geometry⁴⁰”, and whose generality runs over the whole of geometry as opposed to being limited to planar or spatial curves⁴¹.

The first of these four objects is the ‘figure’ (*Gebilde*), which for Schubert is defined as the collection of individuals in a space ∞^c satisfying an algebraic equation involving c constants⁴². This equation is often referred to by Schubert as the ‘definition’ of the figure, especially later on in the *Kalkül*⁴³; however, as we shall see shortly, Schubert concurrently put forth another way of defining figures in terms of ‘fundamental figures’ which he sometimes preferred to use, depending on the theoretical context. This exponent c is thus called the ‘**number of constants**’ (*Constantenzahlen*) of the figure. Conics in a given plane, for instance, have a number of constants equal to five; while pairs of points in space are determined by $2 \times 3 = 6$ constants. In introducing this notion, Schubert uses more complicated examples, such as that of planar curves of order n having δ double points and χ cusps, whose number of constants is asserted to be equal to $\frac{1}{2}n(n+3) - \delta - 2\chi + 3$. This example shows that Schubert is allowed to take relatively complex

⁴⁰“Der 4 wichtigsten *Objecte* der abzählenden Geometrie”, [Schubert, 1876a], p.6. Emphasis in original.

⁴¹ [Schubert, 1876a], pp.6-9.

⁴² [Schubert, 1876a], p.8. The symbol ∞^c here denotes a c -fold infinite collection of objects. While a set-theoretical description of such collections would be anachronistic here, one can think of these collections for $s = 1, 2, 3$, etc. as biunivocally corresponding to the points of a straight line, a plane, a space, etc; or, equivalently, to the collection of c -uplets.

⁴³ [Schubert, 1879b], p.1, 7.

geometrical entities as a figure⁴⁴. Among the figures typically considered by Schubert must be mentioned curves, n -gons, and congruences of lines⁴⁵.

The second object introduced by Schubert is the ‘**condition**’ (*Bedingung*), which are defined as a ‘determination’ (*Festsetzung*) which may constrain the spatial position of a given figure⁴⁶. Forcing conics to pass through a given point, for instance, puts a constraint on them, and thus constitutes a condition. However, Schubert notes, there are properties of individual figures that cannot be taken as conditions, for they must be understood as integral part of the definition of the figure, such as the Plückerian numbers of plane curves, which are an ‘invariant determination’ and do not restrain the spatial extension of the individuals. As a result, to constitute a figure, planar curves must be considered with their Plückerian numbers specified⁴⁷. Here as well, concurrently to this general definition of conditions – which is mostly important in that it allows to produce the arithmetic of dimensions we mentioned earlier –, Schubert would also construct conditions starting from ‘fundamental conditions’, as we shall see later in this section.

Schubert calls a condition ‘composed’ (*zusammengesetzt*) when it expresses nothing more than the requisit that several other conditions, independent of one another, be simultaneously satisfied⁴⁸. It is ‘simple’ (*einzel*) if it cannot be decomposed thusly. Schubert understood this independence in the following manner: a condition constrains the relative position of a figure with respect to some other given figures (for instance, a given point through which curves are required to pass). Conditions are independent if they are defined by unrelated data; if no relation is supposed between the given figures defining them. The relative vagueness of this criterion is something which both Halphen and Study would frequently point out in their criticisms of enumerative geometry, to which we shall turn in the next chapter.

For a given figure, whose number of constants is c , the number of individuals satisfying a given condition will be a number smaller than ∞^c , that is to say a number of the form ∞^{c-b} . Schubert calls the number b the ‘**dimension**’ (*Dimension*) of the condition. Alternatively, Schubert also defines this number as the number of equations in the c constants required to analytically represent the condition⁴⁹. The dimension of a composed

⁴⁴In fact, even figures satisfying a certain condition can be in turn taken as a figure (of number of constants necessarily smaller than the original figure). For instance, ‘conics passing through one fixed point’ can be viewed as a figure in its own right, as well as a sub-figure of the figure ‘conics’.

⁴⁵See the computations of numbers of constants for such objects in [Schubert, 1879b], pp.2-3.

⁴⁶[Schubert, 1876a], p.8.

⁴⁷[Schubert, 1879b], p.3 and Lit.2, p.333. The Plückerian numbers of a curve are its order, its class, its numbers of nodes and cusps, of double points and double tangents, and its genus. Plücker's equations express simple arithmetical relations between these numbers, see [Salmon, 1873], pp.62-64. As a result of these relations, one cannot simply consider the collection of curves for which one of these numbers has a specific, fixed value.

⁴⁸[Schubert, 1876a], p.8.

⁴⁹These equations, which Schubert also calls ‘equations of condition’ (*Bedingungsgleichungen*) are never

condition is the sum of the dimensions of the simple conditions forming it.

Note that, at this point, a condition and the number attached to it are essentially tied to a certain figure. After all, a condition such as ‘passing through a given point’ can be meaningfully applied to a variety of figures, and Schubert never explicitly asserts or shows that the dimension of the condition remains the same for all figures. This is because his enumerative geometry only operates in a way that must begin with the selection of a figure, each successively introduced condition pertaining to the said figure. In particular, there is no property or study of the condition ‘passing through a point’ which Schubert would try to transpose from the geometry of, say, conics to the geometry of octagons. In 1879, Schubert would make this clearer by the fact that Schubert explicitly presents this general discussion as a basis for individual studies of Γ -geometries, that is to say the geometries of specific figures⁵⁰. Despite the overlap in notations, with the same letter denoting the condition ‘passing through a given point’ attached to various figures, such as curves of certain orders and with certain Plückerian numbers, the ambiguity does not subside in Schubert’s practice. Thus, the attachment of conditions and figures means that even some fundamental conditions are differentiated (and allocated different signs), such as ‘for a point to lie on a plane’ and ‘for a straight line to lie on a plane’⁵¹. There is no universal concept attached to the condition such as ‘lying on a plane’ for Schubert. The construction of figures and conditions from ‘fundamental figures and conditions’, however, will provide a way to ensure the permanence of the validity of equations on conditions across several geometrical figures which have similar building blocks.

From the application of conditions to figures, Schubert derives his third object, namely ‘**systems**’ (*System*) of figures. Systems are defined as the totality (*Gesamtheit*) of individuals of a certain figure satisfying a certain condition⁵². For a figure whose number of constants is c , and a condition of dimension b , Schubert defines the ‘**level**’ (*Stufe*) of the system formed by them as the number $a = c - b$. As a general rule, if a system of level a is defined by a figure of number of constants c and a condition of dimension b , one always has

$$a + b = c$$

Schubert would expand on this arithmetic of dimensions in the *Kalkül*, in a way that further answers the question of the relation between dimensions of a condition and numbers of constants of a figure⁵³. Indeed, Schubert noted that if a figure Γ can be viewed as a

explicitly formed.

⁵⁰ [Schubert, 1879b], p.9.

⁵¹ [Schubert, 1879b], p.5.

⁵² [Schubert, 1876a], p.9.

⁵³In 1879, Schubert also considers conditions with negative dimensions. These are obtained by forming a system whose level is greater than the number of constants of the figure, wherein the equation above can only be solved for a negative value of b . For instance, Schubert considers the system of points which

system of level a' of a figure Γ' , and if a given condition is of dimension b when applied to the figure Γ' , this condition is only of dimension $(b - a')$ when applied to Γ ⁵⁴.

When a system is of level zero, that is to say when $b = c$, the system is formed of a finite, possibly null, number of individuals. This **number** (*Anzahl*) itself is the fourth object at the foundation of Schubert's geometry. To the object-number is associated the cardinal number it represents, that is to say the cardinality of a system as described above. The reason why this finite number is the same whatever the position of the given elements of the conditions is said by Schubert to derive from "Algebra [Schubert, 1876a], p.9."; we shall come back to this issue in section 7.3.1 of this chapter on as we tackle Schubert's *principle of conservation of number*. (*wie oft*)

Thus, to each of the four "most important objects of enumerative geometry" corresponds a number: numbers of constants, dimensions, levels, and an unnamed cardinality. The symmetry seemingly does not entirely hold with this fourth pair of object and number, as the object itself is already number. In fact, it can be restored once the *Anzahlen* in question are understood as 'geometrical numbers', that is to say geometrical objects distinct in kind from the cardinality they are associated with, just as figures are associated with a cardinality, namely the number of constants defining them⁵⁵.

Indeed, while the figures, conditions, and systems would remain relatively untouched between the *Beiträge* and the *Kalkül*⁵⁶, the same cannot be said of the fourth object of enumerative geometry, namely 'Anzahlen'. While sections 1 and 3 of the first chapter of the *Kalkül* define successively these three objects and their assorted numbers without requiring justifications going further than the description of their analytical representations and the constants involved therein, the introduction of 'Anzahlen' would require novel explanations. Indeed, in section 4 of this chapter, Schubert considers the finite number N of individuals satisfying the definition of a figure whose number of constants is c , and satisfying a condition of dimension $d = c$. This condition expresses the requisit that the individual figures Γ are in a certain position with regard to other, given figures Γ' (for

must contain a given point. The point (in space) has three constants, and the system thus formed is of level four; since every point is reached ∞^1 times. Thus, the condition 'containing a point' must here be of dimension -1 . See [Schubert, 1879b], p.11. Negative dimensions did not seem to be present in Schubert's mind in 1876.

⁵⁴ [Schubert, 1879b], pp.9-10.

⁵⁵The term 'geometrical number' itself only appears in Schubert's writing in 1879, see [Schubert, 1879b], p.12. In a note written on November 31st 1879 for the Société Mathématique de France and sent to Halphen (but never published), Schubert used this term to translate the word 'Anzahl', especially while discussing his principle of conservation of number which he had introduced earlier this year in the *Kalkül*. The note is thus entitled "Sur le principe concernant la constance des nombres géométriques". It is preserved at the Bibliothèque de l'Institut in Halphen's papers (Ms 5621), and a transcription is provided in the annexes.

⁵⁶Of course, there are some differences, some of which we have already alluded to previously, but this is not the place for an exhaustive comparison of these two texts.

instance, a given point through which the figures Γ must pass). For the ‘Anzahl’ N to be of any relevance, Schubert explains that its crucial property is that it “remains always constant .. when the locus of these given figures Γ' is specialized⁵⁷”. Of course, this very phrasing is reminiscent of the principle of special position, which we mentioned previously. In 1879, however, Schubert had renamed and reworked it into the ‘*Princip von der Erhaltung der Anzahl*’. For Schubert, this principle was merely an import of the fundamental theorem of Algebra, which states that the number of roots of an equation remains unchanged by modifications of the constants of the equation, except when it becomes infinite. While this reworked principle would remain a crucial tool for the obtention of equations between conditions in 1879, as the principle of special position had been since 1874, it also became the foundation for the concept of geometrical number⁵⁸. Note that there is a difference in the German terminology between the ‘*Zahl*’ of roots an equation may have, and the ‘*Anzahl*’ N , the number which represents the individuals satisfying the enumerative problem. Thus, in 1879, the introduction of numbers by Schubert went much further than their identification as a frequency: they are fully geometrical objects, deriving from an algebraic-geometrical principle, and are associated with a ‘*Zahl*’ (another number of constants, of sorts) just like figures, conditions, and systems⁵⁹.

7.2.2 The symbols of enumerative geometry

Let us for now go back to the 1876 *Beiträge*, and the construction of enumerative geometry by Schubert. To the conceptual framework described above would immediately be added a ‘symbolism for conditions’ (*Bedingungssymbolik*), derived from Schubert’s earlier work on the theory of cubics. Once a certain figure has been defined and selected, each condition can be associated to a ‘symbol’ (*Symbol*⁶⁰). More precisely, each simple condition is associated to a single letter (*Buchstabe*), while a composed condition is associated to “the product of the symbols corresponding to the composing conditions, which will be called the *factors* of the composed condition⁶¹”. Note that Schubert does not discuss the

⁵⁷ [Schubert, 1879b], p.12.

⁵⁸We shall discuss these other uses of the principle of conservation of number in section 7.3.1.

⁵⁹The distinction between ‘ordinal numbers’ (*Zahl*) and ‘cardinal numbers’ (*Anzahl*) was crucial to several debates in the last third of the nineteenth century in Germany – whether in psychological, philosophical, or philological studies. Schröder, Helmholtz, and Du Bois-Reymond, for instance, all distinguished between the number of objects in a group of objects (*Anzahl*) and the pure number, accessible without recourse to external experience (*Zahl*); see [Darrigol, 2003], pp.546-551. Schubert was aware of some of these debates, especially regarding the philology of numbers, as we shall show in the next chapter.

⁶⁰Note that Schubert stopped referring to the symbol-letters as ‘signs’ (*Zeichen*) starting in 1876.

⁶¹ [Schubert, 1876a], pp.9-10. Schubert, throughout his books and articles (even beyond the confines of enumerative geometry), always emphasizes notations and choices of letters, symbols, and signs. This reflexive practice of the creation of notations might be advantageously viewed against the backdrop of his long career as a high-school teacher, but also of his lasting and profound interest for the philology and ethnography of numbers and number-signs. On this latter interest, see 8.2.4.

possibility for a composed condition to be expressed as different products. Furthermore, if a condition is composed of the same simple condition n times, then its symbol shall be the n^{th} power of the symbol for the simple condition.

Schubert immediately adds that these symbols do not merely denote the condition itself. If the condition is of dimension a , and a system of level a is being considered at the same time, then the symbol shall denote the finite number of figures in the system satisfying the condition. The requisit that the dimension of the condition equate the level of system for this identification to be possible would be scrapped in 1879. Once the concept of number had been clarified and made independent of the cardinality of the collection of figures satisfying a given condition in a given system, Schubert would be able to use the same symbol for a condition and the number of figures satisfying it regardless of the finiteness of this number⁶².

Associating letters to conditions, and forming products thereof, is not enough to meaningfully describe and explain even the formulae for cubics obtained in 1874, which involved sums of letters. In fact, even in the *Beiträge*, this lacunary explanation remained problematic for Schubert. Instead of providing a geometrical interpretation of these sums, as he had done for products, Schubert elected to define algebraic functions of symbols in the following manner⁶³:

Ganze lineare Function a -facher einzelner oder zusammengesetzter Bedingungen ist also ein kurzer Ausdruck für die Zahlenfunction, welche entsteht, wenn man für jede a -fache Bedingung die ihr rücksichtlich des gemeinten Systems a^{ter} Stufe zugehörige Anzahl einsetzt. Andere als ganze lineare Functionen von a -fachen Bedingungssymbolen können in unserm *Bedingungsalcül* nicht auftreten. Die Gleichsetzung von zwei solchen Functionen ergiebt eine *Formel* a^{ter} *Dimension*.

In particular, the ‘*module of a condition*’ is redefined by Schubert as a function which equates said condition to a linear function of other (and only other) conditions.

In the *Beiträge*, Schubert stated three rules for the manipulation of these formulae. The first one asserts that if certain formulae between condition-symbols are valid in a certain system, then every formula obtained through a linear transformation of these first

⁶²In fact, Schubert occasionally did it already in 1876; but it is only in 1879 that he would be able to openly and explicitly do so.

⁶³“Thus, a linear function of a -fold simple or composed conditions is an abbreviation for the numerical function which arises if one substitutes to each a -fold condition the number corresponding to it with regard to the intended a -level system. Only linear functions of a -fold condition symbols can occur in our calculus of conditions. To equate two such functions gives rise to a formula of dimension a ”, [Schubert, 1876b], p.10. Italics in original.

formulae will be valid in this system⁶⁴. Secondly, if a formula of dimension a is valid in a system Σ_a of level a , whose definition contains the p -fold condition Z_p , then a formula of dimension $(a + p)$ is obtained by adding the symbolic factor Z_p to each of its sides⁶⁵. This new formula is valid for the system Σ_{a+p} of level $(a + p)$, which is obtained by removing Z_p from the definition of Σ_a . Lastly, the module of the product of two conditions is equal to the function obtained by multiplying the modules and carrying out the necessary calculations to achieve the adequate necessary form. For instance, using a formula already given in the 1874 paper on cubics, Schubert writes $P = \mu\nu - 3\mu^2$, which is a module since the condition P is expressed as a function of other conditions⁶⁶. Then the module of the condition composed by P twice, that is to say by P^2 , is obtained by computing

$$P^2 = (\mu\nu - 3\mu^2)^2 = \mu^2\nu^2 - 6\mu^3\nu + 9\mu^4 = \mu^2\nu^2 - 6\mu^3\nu$$

since $\mu^4 = 0$ (for no plane passes through four given points in general). This yields the module of the composed condition P^2 . These three rules, and in particular the two last ones, allow for a symbolic multiplication of conditions and explain the meaning of such multiplications in terms of systems and composed conditions.

It must be stressed that, in 1876, the expression formed for instance by the symbolic sum of the letters denoting two 1-fold conditions is merely a function which to a first-level system associates the sum of the numbers of figures in this system satisfying each condition. In particular, by 1876, Schubert had provided no geometrical interpretation for such formulae with several factors. This is a point where Schubert’s 1879 *Kalkül* starkly departed from the *Beiträge*. As is well-known, Schubert then introduced sums of conditions (or, rather, of condition-symbols) by “attaching meaning to them⁶⁷”: the sum of two conditions y, z would denote the condition ‘to satisfy either y or z ’. This new interpretation of the symbolic algebra of conditions was briefly commented by Schubert via an explicit reference to Schröder’s 1877 *Operationkreis des Logikkalküls*, a book which built on Boole’s well-known logical algebra⁶⁸. Indeed, for Boole as for Schröder, sums and products could be interpreted as disjunctions and conjunctions of classes⁶⁹: if two signs x, y stand respectively for the classes of objects that are blue and solid, then the sign xy

⁶⁴In other words, if in a certain system the formula $f = g$ is valid, then for all linear functions ϕ , $\phi \circ f = \phi \circ g$ is valid in that system.

⁶⁵Schubert is, in a way, explaining how and why both sides of a formula $f = g$ can be multiplied by the same term without being invalidated.

⁶⁶As a reminder, P here denotes the condition ‘passing through a given point’, μ ‘having its plane go through a given point’, and ν ‘intersecting a given line’.

⁶⁷[Schubert, 1879b], pp.3-4.

⁶⁸On Boole, see the collection of essays in [Gasser, 2000]. On Schröder and his work on Boolean logic, see [Dipert, 1978], [Dipert, 1991], [Peckhaus, 1996], [Heis, 2013].

⁶⁹Schröder speaks of *Determination* and *Collektion*, [Schröder, 1877], p.2.

will stand for all solid objects that are blue, while the sign $x + y$ will stand for all objects that are either solid or blue (or both).

From the previous discussion, it is easy to see that both definitions of the sum in Schubert's enumerative geometry coincide numerically, that is to say when symbols represent finite numbers. However, it remains unclear why they coincide conceptually, that is to say why the geometrical meaning of these numerical functions does really correspond to that obtained by viewing additions as disjunctions of conditions. Furthermore, this reinterpretation of the sum is hardly used in the *Kalkül*: Schubert's derivations of equations and computations remain sensibly similar to those carried out in the *Beiträge*. Last, Schubert does not introduce an analogous interpretation of the minus sign $-$, despite it appearing in some of his equations⁷⁰. All of this makes the reference to Schröder little more than an interpretative veil put on top of techniques long in the making, and whose complexity cannot be wholly tamed by this neat logical recasting.

And yet, what was hardly more than a passing remark for Schubert has been the focus of many commentators, both contemporary and posterior. Indeed, through this logical interpretation of the symbolism of conditions, Schubert had tied his own work to the budding tradition of logical algebra, drawing together his geometry and Schröder's logic. For instance, the American philosopher Charles Peirce repeatedly described Schubert's geometry as an “application of the Boolean algebra” of logic to geometry⁷¹. Similarly, in his well-known 1883 address to the British Association, Arthur Cayley commented on Schubert's calculus right after discussing Boole's and Peirce's logic, in the following terms⁷²:

Connected with logic, but primarily mathematical and of the highest importance, we have Schubert's *Abzählende Geometrie* (1878). [...] The noticeable point is that the symbols used by Schubert are in the first instance, not numbers, but mere logical symbols [...] and these logical symbols are combined together by algebraic laws.

The thesis that Schubert's geometry was “modeled on Ernst Schröder's logical calculus⁷³”, as Schubert's biographer Werner Burau puts it, quickly found its way into historical narratives which leave to Schröder little more than the role of a glorified translator of Boole into German. Of course, there is no denying that Schubert knew of and read Schröder. One can even suspect that Schubert titled his 1879 book after Schröder's use of the term *Kalkül*, as this latter occurs seldom in earlier text (and is usually spelt *calcül*).

⁷⁰Divisions cannot be meaningfully defined in the algebra of geometrical conditions.

⁷¹ [Peirce, 1994], in CP 3.526, §13, Introduction to the Logic of Quantity, as well as in CP. 4.131, 4.134.

⁷² [Cayley, 1883], p.459. For more on this address and its general content, see [Smadja, 2007].

⁷³ [Burau & Renschuch, 1966], p.16.

Thus, despite the appreciation that Schubert may have had for Schröder’s book, it remains important to insist on the rather superficial nature of this connection for the inferential practices at the heart of the newly-created enumerative geometry.

7.2.3 Fundamental figures, fundamental conditions

Both in 1876 and in 1879, right after introducing the symbolism of his geometry, Schubert turned to “build” the figures which would inhabit it. While we have already discussed the various objects which occur in enumerative geometry, the architecture of the collection of *Gebilde* which can be taken as objects of said geometry is itself structured and organized as well. Indeed, all figures can be decomposed into systems of three “main elements”, namely points, planes, and rays⁷⁴:

Jede Definition eines Gebildes fasst dasselbe im Allgemeinen auf als eine in gewisser Weise *verkettete Gruppe von Systemen anderer Gebilde*, welche wieder als aus Systemen noch anderer Gebilde bestehend gedacht werden, und so fort; und zwar wird dieser Process der Zerlegung, nach der modernen Anschauungsweise der projectivischen Geometrie, immer derartig angestellt, dass man schliesslich *auf drei Gebilde* stösst, welche als nicht weiter zerlegbar betrachtet werden und dadurch *völlig gleichen Anspruch auf Ursprünglichkeit* erhalten. Desshalb hat die abzählende Geometrie diese drei *Hauptelemente* des Raums, *Punkt, Ebene* und *Strahl*, and die von ihnen erzeugten Systeme, welche *Oerter* heissen sollen, *vor* allen übrigen Gebilden zu behandeln.

The ontology of geometrical figures constructed here by Schubert is pyramidal, with the basis consisting of the three ‘main elements’: points, rays, and planes⁷⁵. By forming systems of these elements – that is to say, loci –, one generates a second kind of figure, a simple example of which is the point-pair. By linking together these systems with the help of geometrical conditions, one can generate further figures indefinitely, such as the triangle. In this sense, the ‘main elements’ are described as the “building blocks” (*Bausteine*) of all other figures⁷⁶. First among these figures, come those which can be generated as loci of main elements.

⁷⁴“Every definition of a figure can in general be conceived of as a group of systems, linked together in a certain way, of other figures, which can also be thought of as systems of yet other figures, and so on; and this process of decomposition, according to the modern conception of projective geometry, is always carried out in such a manner that one eventually comes across three figures which are not considered to be further decomposable, and thus have all equal claim to primitiveness. Therefore, enumerative geometry has to treat these three main elements of space, namely point, plane, and ray, and the systems they generate, which are called loci, before all other figures”, [Schubert, 1876a], p.15.

⁷⁵This is comparable to what Schubert had already proposed, in a less systematic way, in his 1870 dissertation, see 6.1.2.

⁷⁶ [Schubert, 1879b], p.4.

Due to the main elements being of number of constants three (for points and planes) or four (for rays), loci can only be systems of levels between 0 and 4. In fact, Schubert continues, fourteen such loci can be formed, which constitute the fourteen “fundamental figures” (*Grundgebilde*) of enumerative geometry, presented in the chart below. The same list of fourteen fundamental figures would be reproduced in the *Kalkül*, although without the classification by level of systems (and with additional names for each of these systems).

Grundgebilde			
	des Punktes	der Ebene	des Strahls
0 ^{ter} Stufe	Punkt	Ebene	Strahl
1 ^{ter} Stufe	Punktaxe	Ebenenaxe	Strahlbüschel
2 ^{ter} Stufe	Punktfeld	Ebenenbündel	{ Strahlenfeld, Strahlenbündel }
3 ^{ter} Stufe	Punktraum	Ebenenraum	Strahlenaxe
4 ^{ter} Stufe			Strahlenraum

[Schubert, 1876a], p.16.

The figures formed here all correspond to various ways of “carrying” (*Tragen*) the main element: a *Bündel* is when the main element is carried by a point (which is why there is no *Punktbündel*), while in a *Feld* the element is carried by a plane. For instance, a *Strahlenfeld* is the collection of rays lying in a plane, while a *Strahlenbündel* is the collection of rays passing through a point.

This decomposition of all figures into these three main elements is described by Schubert as characteristic of the “modern conception of projective geometry”. While Schubert gave no explicit reference to texts embodying this ‘modern conception’ in the 1876 *Beiträge*, the 1879 *Kalkül* contains further clues as to what this geometry he is alluding to is. The concept of *Grundgebilde* is borrowed by Schubert from the German tradition of the “geometry of position” (*Geometrie der Lage*), as would be explicitly acknowledged in the *Kalkül*⁷⁷. Indeed, the term occurs prominently in both Von Staudt’s famous *Geometrie der Lage* as well as in later adaptations thereof, such as Reye’s 1866 homonymous book⁷⁸. The fundamental figures identified by both Von Staudt and Reye certainly feature amongst those of Schubert, but there seems to be no consensus on the exact classification and number of these figures, with Reye for instance counting only six. More so than the exact ontology of spatial figures, what Schubert has retained from his reading of the geometry of position is the idea of decomposing said figures into elements, and classifying composed figures by their levels or dimensions.

⁷⁷ [Schubert, 1879b], p.4.

⁷⁸ [von Staudt, 1847], pp.10-12 for instance; [Reye, 1866], pp.1-9. For the role of these fundamental figures in Von Staudt’s synthetic geometry, see [Nabonnand, 2006], pp.112-117.

To these 14 fundamental figures correspond “fundamental conditions” (*Grundbedingungen*). The classification of these fundamental conditions differed between 1876 and 1879, with their number going from 14 to 11.

In the 1876 *Beiträge*, Schubert defined a fundamental condition as “a condition which expresses that a point or a plane or a ray must belong to a fundamental figure as element⁷⁹”. Thus, Schubert associated one fundamental condition to each fundamental figure. For a locus constituted of points, for instance, Schubert considers conditions associated to *Punkträume*, *Punktfelder*, *Punktaxen*, *Punkte* (note that the order is the inverse of that of levels); which he respectively denotes p_0, p_1, p_2, p_3 and names respectively *Raumbed[ingung]*, *Feldbed[ingung]*, *Axenbed[ingung]*, *Punktbed[ingung]*⁸⁰. p_1 , the fundamental condition associated to the *Punktfeld*, can be understood as the condition that a point be on a given plane, as a *Punktfeld* is a plane made of points. Similarly, p_2 expresses the condition that a point be on a given ray, and p_3 that a point be (on) a given point.

A. Für Punktörter

p_0	Raumbed.	
p_1	Feldbed.	c
p_2	Axenbed.	c_g, ν
p_3	Punktbed.	C, P, Π

B. Für Ebenenörter

e_0	Raumbed.	
e_1	Bündelbed.	μ
e_2	Axenbed.	μ_g, ν'
e_3	Ebenenbed.	M, P', Π'

C. Für Strahlenörter

s_0	Raumbed.	
s_1	Axenbed.	g
s_2	Feldbed.	g_e, ρ
s_{II}	Bündelbed.	g_μ, ρ'
s_3	Büschelbed.	g_s, t, β
s_4	Strahlbed.	G, T, B, S

[Schubert, 1876a], p.18.

As we saw previously, a condition is only fully meaningful when associated to a certain figure. Therefore, these interpretations of the fundamental conditions can give rise to

⁷⁹“eine Bedingung, welche aussagt, dass ein Punkt oder eine Ebene oder ein Strahl dem Grundgebilde als Element angehören soll.”, [Schubert, 1876a], p.17.

⁸⁰See table below, reproduced from [Schubert, 1876a], p.18. Schubert abbreviates *Bedingung* as *bed.* in each of these terms.

several other conditions, when applied to higher-order loci generated by fundamental elements. On the right of the chart above are symbols which Schubert would use to denote the various conditions to which these fundamental conditions can give rise to, depending on the level of the system they are applied to. For instance, the two symbols c_g, ν associated to p_2 will be used later in the *Beiträge* to respectively denote the condition that a (point-)curve have a cusp on a given straight line, intersect a given straight line.

The conditions p_0, e_0, s_0 cannot be applied to systems of level higher than 0, that is to say to anything other than fundamental figures themselves (in other words, they are conditions of dimension 0). As such, they do not appear in Schubert’s list of fundamental conditions in the 1879 *Kalkül*, which is why it only amounts to 11 conditions. There are other notable differences in the presentation and description of these fundamental conditions between the two texts. These variations bespeak of a profound change in Schubert’s understanding of the relationship between fundamental figures and the objects of enumerative geometry.

Indeed, in 1879, fundamental conditions were not associated in so straight-forward a manner to fundamental figures. Schubert defined these conditions as “conditions which require that a ‘main element’ of the figure Γ lie in a given fundamental figure⁸¹”. Schubert then gave a list of 11 conditions, divided into three groups: three point-conditions, three plane-conditions, and five ray-conditions. The symbols for these conditions were not tied to the levels of fundamental figures as systems of main elements, but rather to the geometrical interpretations that they allowed for. Thus, what was p_3 in 1876 would be introduced as the condition of symbol P , which expresses that a point p be given. Of course, this condition P and its dimension are relative to the choice of the figure Γ , and thus Schubert does away with the nuanced distinction between C, P and Π , the three conditions which can arise out of p_3 depending on the number of constants of Γ . Schubert’s 1879 terminology and symbolism are definitely looser and more flexible than that of his previous paper, which may partially be explained by the years of geometrical practice he accumulated between the redaction of both texts.

In the *Kalkül*, then, the 11 fundamental conditions given by Schubert are the following ones⁸²:

- I. If a point is denoted by p , then:
 - 1. The symbol p stands for the *condition* that this point p lies in a given plane.

⁸¹“Unter den einem Gebilde Γ auferlegbaren Bedingungen spielen die fundamentalste Rolle die *Grundbedingungen*, das sind Bedingungen, welche verlangen, dass irgend ein dem Gebilde Γ angehöriges Hauptelement in einem gegebenen Grundgebilde liegen soll.”, [Schubert, 1879b], p.4.

⁸² [Schubert, 1879b], p.5.

- 2. The symbol p_g , that the point p lies on a given straight line (*Gerade*⁸³).
- 3. The symbol P , that the point p is given.
- II. If a plane is named e , then:
 - 1. The symbol e stands for the *condition* that the plane e contains a given point.
 - 2. The symbol e_g , that the plane e contains a given straight line.
 - 3. The symbol E , that the plane e is given.
- III. If a ray (*Strahl*) is named g ,
 - 1. The symbol g stands for the *condition* that the ray g intersects a given straight line.
 - 2a. The symbol g_e , that the ray g lies in a given plane.
 - 2b. The symbol g_p , that the ray g goes through a given point.
 - 3. The symbol g_s , that the ray g belongs to a given pencil of rays.
 - 4. The symbol G , that the ray g is given.

Whenever possible, we shall use these descriptions of the conditions to explain Schubert’s formulae, as they are much easier to understand than the heavily formal discourse through which he formulated much of his reasoning in earlier texts.

Having formed fundamental figures and conditions from ‘main elements’, Schubert can generate all possible figures by constructing systems with them. Figures, Schubert explained in 1876, were to be understood as “support of loci” (*Träger von Oerten*⁸⁴):

Die Gebilde als Träger von Oertern. Ein geometrisches *Gebilde* repräsentirt im Allgemeinen eine Gesamtheit mehrerer in gewisser Weise verketteter Punkttörter, Ebenenörter, Strahlenörter verschiedener Stufen, von denen einige das ganze Gebilde zu erzeugen im Stande sind. In jedem einzelnen Falle folgt an, der genauen Definition eines Gebildes die Definition jedes dieser Oerter und die Art und Weise ihrer Verkettung. Gewisse dieser Oerter bezeichnen wir als die *Plücker’schen Oerter* des Gebildes.

⁸³We translate *Gerade* by straight line, and ray by *Strahl*. The straight line is a figure, whereas the ray is an element of space, that is to say something which constitutes figures.

⁸⁴“The figure as a support of loci. In general, a geometrical figure represents a collection of several point-loci, plane-loci or ray-loci of various levels, linked in a certain way, some of which are able to generate the entire figure. In each individual case, the exact definition of a figure is followed by the definition of each of these loci and the way in which they are interlinked. We call some of these loci ‘Plückerian loci’ of the figure”, [Schubert, 1876a], p.19.

From main elements, systems were generated, which Schubert called loci or fundamental figures. From fundamental figures, other loci can be generated and linked together to form other figures. A figure, in turn, is a collection of all the these loci formed by the main elements attached to the figure. For instance, Schubert lists among the Plückerian loci for a planar curve (in space) the plane-locus of its plane; it is of course a system of level 0 because it contains only one plane. The point-locus of the points of the curve is another such locus, forming a system of level 1, and so is the ray-locus of its tangents. These three loci, Schubert comments, have usually been considered by Chasles and others as “elementary loci”, and used as the basis for investigations into the enumerative geometry of planar curves. However, Schubert adds, the choice of these three loci is “merely conventional⁸⁵” (*nur conventioneller*), as other Plückerian loci can be formed, such as the point-locus formed by the singular points of the curve and its dual plane-locus. Schubert concludes that Chasles was mistaken in viewing the point-locus and ray-locus of conics (or planar curves), formed by their points and tangents, as being intrinsically elementary (and, thus, the conditions ‘passing through a point’ and ‘touching a straight line’ as intrinsically elementary). Indeed, as Zeuthen had shown, these other Plückerian loci (which Schubert calls ‘singular loci’) could serve the same role just as well.

In Schubert's terminology, this means that the characteristics are none other than ‘elementary numbers⁸⁶’. We will return to this issue when discussing Schubert's reinterpretation of Chasles' $\alpha\mu + \beta\nu$ formula as expressing not so much a property of the characteristic numbers (μ, ν) , but simply showing that two independent numbers were sufficient in solving all of the enumerative geometry of conics.

On the other hand, fundamentalness is not merely conventional: fundamental conditions can be defined for any figure, as conditions whose symbolic factors only express fundamental conditions on the Plückerian loci of the figure⁸⁷. Similarly, fundamental numbers can be defined as numbers of elements in a system satisfying fundamental conditions. This is why, for Schubert (and at least in 1876), the main purpose of enumerative geometry was the determination of the fundamental numbers of a given figure.

In short, Schubert is giving an alternative definition of the figure; which we might call a ‘constructive definition’ to preserve the analogy with the status of loci in Ancient Greek Geometry. It is unclear how this definition totally recapture the algebraic definition of figures (and only that).

⁸⁵ [Schubert, 1876a], p.20.

⁸⁶Schubert makes this clear in a private letter to Halphen, where he writes: “Was macht eigentlich Herr Maillard ? Ich habe von ihm seit seiner berühmten Doctor Dissertation über die Charact. (bester wohl ‘Elementaren Anzahlen’) der cub. Plancurven nichts wieder gehört”, Ms Cod 5624 167, Letter Schubert to Halphen, dated Juli 12th 1876.

⁸⁷ [Schubert, 1876a], p.20.

In 1879, Schubert did away with the reference to Plücker⁸⁸; only introducing systems of main elements as loci (*Oerter*⁸⁹). Certain other loci have specific names, granted by use and habit; as is the case for instance of the point-systems of first level which will be called curves in the rest of Schubert’s *Kalkül*. However, in theory, enumerative geometry is preoccupied with determining the numbers associated with any system thus created from fundamental figures and conditions.

While the heavy theoretical architecture of the objects of enumerative geometry, as exposed in the 1876 *Beiträge*, would be mostly abandoned in favour of more flexible (and more ambiguous) notations and wordings with the 1879 *Kalkül*, the fact that geometrical objects ought to be decomposable into fundamental figures remained an important tenet of Schubert’s geometrical practice. In the very first lines of his 1880 paper on the enumerative geometry of the triangle, for instance, Schubert introduced the figure-subject of his work in terms of fundamental figures⁹⁰:

Das im Folgenden behandelte Gebilde besteht aus drei in fester Ebene befindlichen Punkten a, b, c (Ecken) und deren Verbindungsstrahlen α, β, γ (Seiten), so dass α und a , β und b , γ und c einander gegenüberliegen. Wir nennen dieses Gebilde, dessen Constantenzahl 6 ist, kurz “*Dreieck*”, fassen es aber zugleich als Dreiseit auf, d. h., wir rechnen zu seinen wesentlichen Bestandtheilen ebenso gut seine 3 Seiten, wie seine 3 Ecken, was natürlich nur dann von Bedeutung wird, wenn zwei Ecken oder alle drei Ecken zusammenfallen.

Note how triangles are introduced as a figure composed of points and rays, satisfying certain conditions so as to form a sub-figure of the figure formed by the totality of triplets of points and triplets of rays (whose number of constants would be not 6, but $3 \times 2 + 3 \times 2 = 12$). These conditions, here, are merely that the points belong to the appropriate rays. The usual terminology in use in the geometry of the triangle is then reintroduced to simplify wordings. The building of triangles upon fundamental figures is particularly useful when Schubert wants to introduce degenerate forms of triangle, and conditions associated to such forms; as it allows to express them through formulae that involve the symbols for points and rays a, b, c and α, β, γ .

⁸⁸Other than using Plücker’s formulae, Schubert only mentions Plücker’s name briefly when introducing the term Γ -geometry, [Schubert, 1879b], p.9. By this term, Schubert means the enumerative geometry in which a certain figure Γ is taken as the common, stable reference with regard to which all conditions, systems, and numbers will be expressed. Of course, this use is a reference to Plücker’s *Neue Geometrie des Raumes* and its introduction “the straight line as an element of space”, [Plücker, 1865], [Plücker, 1868].

⁸⁹“Die durch beliebige Bedingungen definirten Systeme von Hauptelementen nennen wir auch Oerter, im Anschluss an den bei der Analysis euclidischer Constructionsaufgaben üblichen Ausdruck ‘*geometrischer Ort*’”, [Schubert, 1879b], p.8.

⁹⁰ [Schubert, 1880a], p.153. We come back to Schubert’s geometry of the triangle in 7.4.

As fundamental figures serve as building blocks for all geometrical figures, so do numbers of such figures satisfying fundamental conditions serve a crucial role in the computation of other geometrical numbers. There, it is important to note that some of the numbers associated to these fundamental objects are said by Schubert to be “axiomatic⁹¹” (*axiomatisch*). For instance, Schubert explains, three propositions must be taken as axioms pertaining to the (enumerative) geometry of the ray:

- If there is a point which lies on two given rays, then there exists a plane which contains these two rays, and conversely.
- There is one and only one ray which contains two given points.
- There is one and only one ray which lies on two given planes.

While these axioms may seem self-evident, they in fact carry a greater generality than might appear at first sight, as they are to be read through the principle of conservation of number, which states that the numbers involved in these axioms do not depend on the relative positions of the given elements. We shall come back to this principle in section 7.3.1.

7.2.4 *Produktsatz und Charakteristikentheorie*: the ultimate goal(s) of enumerative geometry

Having described the architecture of Schubert’s geometry, and in particular its foundations, we now turn to its objectives. Schubert’s *Kalkül* opens with a presentation of the general goal of enumerative geometry as a mathematical discipline, namely to answer questions of the following form⁹²:

Wieviel geometrische Gebilde von bestimmter Definition erfüllen gewisse gegebene **Bedingungen**?

Later in this book, Schubert would bring forth a more specific description of what specific kind of answer this question entails.

Some of Schubert’s results have become famous on their own, among which stands out his enumeration of the 5,819,539,783,680 twisted cubics tangent to twelve given quadrics in space, a result whose obtention has been compared to “landing a jumbo jet blindfolded⁹³” by modern-day geometers. Indeed, finding such concrete numbers of objects satisfying

⁹¹ [Schubert, 1879b], p.89. Schubert also described some of these fundamental numbers as being axiomatic in the 1876 *Beiträge*, see [Schubert, 1876a], p.25.

⁹²“How many geometrical figures of a determined definition are there, which satisfy certain given conditions?”, [Schubert, 1879b], p.1. Emphasis in original.

⁹³ [Harris, 1992], p.2. For Schubert’s computation, see [Schubert, 1879b], p.184.

conditions was a key goal of Schubert’s methods, as the many tables of numbers he reproduced in his book show (see fig. below⁹⁴).

Tabelle sonstiger Kegelschnittanzahlen.

$P\gamma^6 = 18$	$P\mu\gamma^5 = 6$	$P^2\gamma^5 = 4$	$T\gamma^5 = 12$
$P\gamma^5\varrho = 24$	$P\mu\gamma^4\varrho = 10$	$P^2\gamma^4\varrho = 6$	$T\gamma^4\varrho = 20$
$P\gamma^5\varrho^2 = 28$	$P\mu\gamma^4\varrho^2 = 16$	$P^2\gamma^4\varrho^2 = 8$	$T\gamma^4\varrho^2 = 16$
$P\gamma^4\varrho^3 = 24$	$P\mu\gamma^3\varrho^3 = 16$	$P^2\gamma^3\varrho^3 = 8$	$T\gamma^3\varrho^3 = 8$
$P\gamma^4\varrho^4 = 16$	$P\mu\gamma^3\varrho^4 = 12$	$P^2\gamma^3\varrho^4 = 8$	$T\gamma^3\varrho^4 = 4$
$P\gamma^3\varrho^5 = 8$	$P\mu\gamma^2\varrho^5 = 6$		$T\gamma^2\varrho^5 = 2$
$P\gamma^6 = 4$			
<hr/>			
$\alpha\gamma^5 = 58$	$\alpha^2\gamma^4 = 32$	$\alpha^2\gamma^5 = 13$	$\alpha^3 = 4$
$\alpha\gamma^5\varrho = 64$	$\alpha^2\gamma^4\varrho = 26$	$\alpha^2\gamma^4\varrho = 8$	$\alpha\gamma^5 = 4$
$\alpha\gamma^4\varrho^2 = 52$	$\alpha^2\gamma^3\varrho^2 = 16$	$\alpha^2\gamma^3\varrho^2 = 4$	$\alpha\gamma^4\varrho = 6$
$\alpha\gamma^4\varrho^3 = 32$	$\alpha^2\gamma^3\varrho^3 = 8$		$\alpha\gamma^3\varrho^3 = 4$
$\alpha\gamma^3\varrho^4 = 16$	$\alpha^2\gamma^2\varrho^4 = 4$		$\alpha\gamma^3\varrho^4 = 2$
$\alpha\gamma^3\varrho^5 = 8$			$\alpha\gamma^2\varrho^5 = 1$
$\alpha\gamma^6 = 4$			

[Schubert, 1879b], p.96.

However, this is not all there is to enumerative geometry. Schubert produced a vast amount of lists of various sorts – be it of numbers, or of formulae. These lists do not have the same status: some sum up the enumerative geometry of a given figure by providing a structured list of results, while others list useful formulae which are to be used in the building of said enumerative geometry of a given figure. There remains to understand what it means to study and expound the enumerative geometry of a given figure.

To that end, one must ponder Schubert’s reinterpretation of Chasles’ achievements through the theory of characteristics, as displayed in both the *Beiträge* and (with greater detail and a more systematic approach) in the *Kalkül* (as in many ulterior texts). Indeed, remember that at the beginning of the *Beiträge*, Schubert had identified three “moments” (*Momente*) of particular importance in the genesis of Chasles’ theory of characteristics: the discovery of the principle of correspondence, the discovery of the $\alpha\mu + \beta\nu$ theorem, and the reduction of the computation of numbers of conics to that of the two numbers of degenerate conics. In Schubert’s reinterpretation, while the first moment is that which provided Chasles with a method for generating geometrical numbers, the second is that which showed that all generations of such numbers hinge upon the determination of only two numbers, namely the characteristics μ and ν . The third moment (for which Zeuthen should be equally credited) showed that this was no crucial property of these two numbers themselves, but rather that any two well-chosen numbers could suffice – and, in particular, the numbers of degenerate conics had proven even more useful than the characteristics⁹⁵.

In this interpretation of Chasles’ results, the $\alpha\mu + \beta\nu$ theorem is called by Schubert a “Produktensatz”, and immediately compared to theorems we discussed in the previous

⁹⁴In this table, which just precedes the enumeration of Chasles’ 3264 conics touching five given conics, are counted planar conics in space, hence the 8-dimension conditions formed in the left rows.

⁹⁵ [Schubert, 1876a], pp.1-4.

chapter, such as Halphen's formula for the geometry of rays in space, or Chasles' analogous formula for second-order surfaces. Schubert would only properly define *Produktensätze* much later in the *Beiträge*, at a point toward the end of the article where he himself set out to prove one on a different figure⁹⁶:

Im Allgemeinen wollen wir unter *Produktensatz* jeden Satz verstehen, welcher eine *Fundamentalzah*l des Systems derjenigen Elemente, die zweien von ein und demselben Elemente erzeugten Systemen gemeinsam sind, durch *Fundamentalzahlen* dieser beiden Systeme ausdrückt.

In the case of Chasles' theorem, the two systems in question are two systems of level 1 and 4, composed of conics generated by (for instance) points; while the fundamental number is simply the number of conics common to both systems⁹⁷. In other words, what is being sought is the number of elements common to two systems of level 1 and 4: in that sense, it is a number of intersections of two systems (which are composed of loci), and it is in this sense that Schubert often compares Chasles' theorem to Bézout's theorem⁹⁸. Chasles' theorem expresses this number via fundamental numbers of both systems, namely the characteristics (μ, ν) of one, and the numbers (α, β) of the other system (where α and β can be shown to be fundamental numbers of this other system via a proof presented in 7.3.3.

Chasles' and Zeuthen's re-foundation of the theory of characteristics upon the numbers of degenerate conics (as opposed to characteristic numbers themselves) only show that another *Produktensatz* is possible, wherein the fundamental number of these two systems of conics is made to depend not on the characteristic numbers of these systems, but on other fundamental numbers, tied with their degenerate elements. Having obtained *Produktensätze* for a certain figure, Schubert continued, one can build its "true theory of characteristics" (*eigentliche Charakteristikentheorie*). While Schubert gives no explicit definition of such a theory, the way he presents his examples thereof seems to indicate that a "theory of characteristics of a certain figure" is obtained when "one can replace any condition imposed on a figure, which expresses nothing other than the fact that an element of a Plückerian locus of this figure satisfies some condition, by some fundamental

⁹⁶"In general, by 'product-theorem' we mean every theorem which expresses a fundamental number of the system of the elements common to two systems, both of which are generated by one and the same (type of) element, through fundamental numbers of both systems", [Schubert, 1876a], p.91. Italics in original.

⁹⁷More generally, for a figure of number of constants c , the intersection of two systems of level α and β so that $\alpha + \beta = c$ is of level 0, and thus its fundamental number can only be the number of its elements.

⁹⁸The analogy between Chasles' theorem and Bézout's theorem was a common one at the time; it can be found in the texts of Halphen or Study for instance. However, the meaning of this analogy changed: Halphen, for instance, had constructed actual curves whose points corresponded to the conics in both systems, so that the intersection of these two curves yielded Chasles' formula. Study, on the other hand, interpreted both systems as geometrical objects in a multi-dimensional projective space.

conditions of this figure⁹⁹”. Little more is said on this ideal for the enumerative-geometrical theory of a figure, which is only presented at the very end of what was supposed to only be the first out of three extended memoirs¹⁰⁰.

It is remarkable that Chasles’ theorem is here recast as one (historically, but not conceptually) important theorem within a class of similar theorems¹⁰¹. Furthermore, such theorems are not limited to systems whose added levels equate the number of constants of the figure at hand; that is to say, there can be *Produktensätze* which do not give a number of elements common to two systems, but merely other characteristic numbers of the system formed by the system of elements common to two other systems¹⁰². However, this category, which no one seems to have picked up, was quickly dropped by Schubert. By 1879, it completely disappeared from his writings, only to come back in ulterior papers (such as that on the enumerative geometry of the triangle) under the name of “Productenformeln”. Instead, Schubert’s focus was now on what he called the *Charakteristikenprobleme*.

Schubert’s 1879 *Kalkül* is structured in a way that makes it clear what the purpose of enumerative geometry should be. As we already discussed, the first chapter is entirely devoted to the fundamental concepts and notations of this new geometry, while chapters 2 and 3 discuss the two main methods used to obtain formulae involving the symbols introduced for a given figure. Chapter 4 and 5 add to these methods, by introducing notations and examples of (respectively) modes of degenerations, and multiple coincidences. However, the last and sixth chapter, entitled “Die Charakteristikentheorie”, clearly and explicitly sets up the tasks ahead of the prospective enumerative geometry, the fulfillment of which these methods are merely tools for. That is to say that Schubert was not content with giving methods to obtain geometrical numbers; he had a precise idea of what sorts of results were constitutive of the enumerative geometry of a certain figure, once well-studied. Indeed, this chapter opens with a general formulation of the *Charakteristikenproblem* for an arbitrary figure¹⁰³:

⁹⁹“Jede einem Gebilde auferlegte Bedingung, welche nichts anderes aussagt, als dass ein Element eines Plücker’schen Orts dieses Gebildes irgend welche Bedingung erfüllt, ist immer durch fundamental Bedingungen des Gebildes ersetzbar”, [Schubert, 1876a], p.115.

¹⁰⁰The other two parts of the *Beiträge* were eventually published, but the last one is very short, while the second one only puts into practice these concepts without making their definition more explicit.

¹⁰¹Lê has recently studied the contemporary genesis of a broader, yet analogous category for geometrical theorems, namely “closure theorems” or “Schliessungssätze”, see [Lê, 2018].

¹⁰²Schubert’s formulation means that one could consider *Produktensätze* for various fundamental numbers associated to the common elements of two systems of conics of level 2 and 2, for instance.

¹⁰³“Let a figure Γ with number of constants c be an element of some totally arbitrary i -level system Σ , and of some totally arbitrary $(c - i)$ -level system Σ' . A finite number x of figures Γ is common to both systems. We assume that for all possible values of i , the number x can be represented as the sum of m products composed of two factors, of which the first always yields the number of figure in Σ which satisfy a i -fold condition. The problem is considered to be solved, regardless of how large the number m has become and of which i -fold and $(c - i)$ -fold conditions had to be used to form the products. Since formulas have to be constructed for all possible values of i and $c - i$, the solution of the problem of characteristics

Ein Gebilde Γ mit der Constantenzahl c sei Element eines ganz beliebigen i -stufigen Systems Σ und auch Element eines ganz beliebigen $(c - i)$ -stufigen System Σ' . Beiden Systemen ist eine endliche Anzahl x von Gebilden Γ gemeinsam. Es wird für alle möglichen Werthe von i verlangt, die Anzahl x als Summe von m Producten darzustellen, deren jedes aus zwei Faktoren besteht, von denen der erste Faktor immer angiebt, wieviel Gebilde aus Σ eine i -fache Bedingung erfüllen. Das Problem gilt als gelöst, gleichviel wie gross die Zahl m wird und gleichviel, welche i -fachen und welche $(c - i)$ -fachen Bedingungen zur Bildung der Producte verwandt werden mussten. Da man für alle möglichen Werthe von i und $c - i$ Formeln aufzustellen hat, so besteht die Lösung des Charakteristikenproblems in der Aufstellung von $\frac{1}{2}c$ oder $\frac{1}{2}(c - 1)$ Formeln, je nachdem c gerade oder ungerade ist.

These $\frac{1}{2}c$ formulae are called *Charakteristikenformeln*, and the conditions involved in their factors *Charakteristiken*. The numbers α and β in Chasles' formula are not considered by Schubert to be characteristics of the system Σ_1 just yet, but, as he and Hurwitz had proven in 1876, these two numbers could be expressed as functions of the characteristics of the system Σ_1 , as we shall explain in section 7.3.3. Note that Chasles' formula itself is not yet a full solution to the *Charakteristikenproblem* for the geometry of conics: to it must be added Cremona's $\alpha\rho + \beta\sigma + \gamma\tau$ formula for the number of conditions belonging simultaneously to a Σ_2 and Σ_3 system. For the geometry of the point, Schubert notes, this problem is what Bézout's theorem solves.

These problems are exactly what Schubert would tackle in papers written in the wake of this book. In the aforementioned paper on the geometry of the triangle, for instance, Schubert produces *Productenformeln* for all possible values of i in increasing order. Sections 4, 5, and 6 of this paper respectively aim to give and prove formulae for the number of triangles common to systems of level one and five, two and four, three and three¹⁰⁴. Once this is done, Schubert moves on to discuss more specific problems, for instance counting infinitely small triangles. Nonetheless, the primary objective of enumerative geometry as Schubert thought of it was, for any possible figure Γ , and for all i from 1 to $\frac{1}{2}c$, to find systems of conditions a_i and b_i in the systems Σ_i and Σ_{c-i} , such that one could write equations of the form

$$x_i = a_1b_1 + \dots + a_mb_m$$

where x_i denotes the number of individuals contained simultaneously in both systems. Once that is accomplished, the a_i 's and b_i 's can be called characteristic numbers of both

consists in the setting up of $\frac{1}{2}c$ or $\frac{1}{2}(c - 1)$ formulae, depending on whether c is even or odd", [Schubert, 1879b], p.282.

¹⁰⁴ [Schubert, 1880a], pp.172-176, 176-180, 180-183.

systems, a term which now refers to any collection of geometrical numbers with which it is possible to write equations of this form¹⁰⁵. This is not an intrinsic property of any figure, nor are they unique, which is why Schubert calls this terminology ‘improper’¹⁰⁶: as Zeuthen had shown, instead of μ and ν , one could have just as well taken λ and $\tilde{\omega}$, the numbers of degenerate conics of each kind in a system (see section 6.2.1).

7.3 Computations in Schubert’s enumerative geometry

Having now described the fundamental concepts and the goals of *abzählende Geometrie*, we turn to its inner workings, that is to say the methods used by Schubert to compute geometrical numbers and solve *Charakteristikenprobleme*.

In the *Beiträge*, two principles are introduced as the main tools to derive formulas in the conditions related to a given figure. Indeed, Section 2 of this text, entitled “General formulae between fundamental conditions” (*Allgemeine Formeln zwischen den Grundbedingungen*), opens with an introduction of the “the principle of special position” (*Das Princip der speciellen Lage*¹⁰⁷) (§7). Schubert then gives an alternative denomination for this principle, namely that of “principle of conservation of number” (*Princip von der Erhaltung der Anzahl*), which is the name that would be used in later texts. In fact, Schubert describes this principle as following from a proposition borrowed “from algebra” necessary for the very definition of the finite numbers associated to systems of level 0¹⁰⁸. However, the principle itself is only associated to the computational technique which he then uses in paragraphs 8 to 13 to study pairs of fundamental conditions, and does not appear within passages which contain definitions of systems or figures.

Next, section 3 of the *Beiträge*, entitled “Pairs of main elements and their coincidences” (*Die Paare von Hauptelementen und ihre Coincidenzen*), opens with an introduction of

¹⁰⁵Note that part of Schubert’s general problem entails the finding of geometrical numbers β_1, \dots, β_m such that any condition z can be written as $z = \sum \alpha_i \beta_i$, for some coefficients α_i . In other words, results such as Chasles’ $\alpha\mu + \beta\nu$ are but a part of the answer to Schubert’s problem, even in the lone case of conics.

¹⁰⁶“Später nannte man missbräuchlich Charakteristiken eines Gebildes Γ alle auf Γ bezüglichen Anzahlen, ohne sich darüber Rechenschaft abzulegen, ob für ein solches Gebilde Γ ein dem Chasles’schen Satze analoger Satz existirt”, [Schubert, 1879b], p.274.

¹⁰⁷This principle had already been introduced in Schubert’s 1874 paper on cubics, see [Schubert, 1874], p.274.

¹⁰⁸That is to say, systems of a figure whose number of constant is a defined by a condition of dimension a . Schubert needs something that resembles the fundamental theorem of algebra to assert that these finite numbers do not depend on the specific position of the elements defining the aforementioned condition. See [Schubert, 1876a], pp.9, 23. Note that sometimes we speak of ‘the figures of a system’ instead of its ‘elements’. The ambiguity is present in Schubert’s writing; however, one should not mistake the elements of a system and the general figure defined as a collection of systems.

the concepts of “pair of elements” and of “coincidences” (§14), then Schubert's reinterpretation of the principle of correspondence (§15). The following paragraphs, until paragraph 26 apply said principle to pairs of points, pairs of rays etc.; as well as to degenerate figures. Paragraphs 27 and 28 begin to do the same for algebraic curves and surfaces. The last paragraphs of the first *Beiträge* show how the principle of correspondence, as well as the formulae obtained via the principle of conservation of number, are sufficient for Schubert to obtain *Productensätze*. In particular, in paragraphs 29 and 30, Schubert solves the problem of characteristics for fundamental figures such as points in space.

In the 1879 *Kalkül*, we find a slightly modified order of presentation of the computational apparatus used to solve the problem of characteristics. The principle of conservation of number is introduced early in section 1, amidst definitions and notations¹⁰⁹. There, it serves not a computational purpose, but rather to define the very concept of (finite) numbers of figures satisfying conditions of a sufficiently high dimension. Crucially, for Schubert, it also acts as the “precondition for the applicability of the results and methods derived in the following sections¹¹⁰”.

Instead of a division by principles, the *Kalkül* operates a division in kinds of formulae. Section 2 is devoted to “Incidenzformeln” (incidence formulae), and Section 3 to “Coincidenzformeln” (coincidence formulae). The principle of correspondence is introduced midway through this third section, but by no means structures it - although it is used rather extensively throughout the rest of the book. Section 4 contains formulae pertaining to modes of degeneration of figures, and Section 5 formulae pertaining to multiple-coincidences. These kinds of formulae are all defined in terms of modes of correlation between the fundamental figures which constitute the figure Γ under study. For instance, incidences are defined by Schubert as figures arising from special positions between main elements, such as when a point lies on a ray. Incidence formulae are obtained using the fundamental formulae of such figures¹¹¹. The method put forth by Schubert for solving *Charakteristikenprobleme* in the sixth and final section of the *Kalkül* (as well as in subsequent texts) is thus to first define the figure Γ in terms of fundamental figures (and, if needed, fundamental conditions), and second to list the formulae of these various kinds which arise from this reduction to fundamental figures. Through a combination of these formulae, Schubert then computes a *Produktensatz*.

In what follows, we shall present these various techniques and how they relate to one another. In a first subsection, we describe Schubert's principle of conservation of number, its evolution, and its uses. We then turn to the techniques introduced in Schubert's

¹⁰⁹ [Schubert, 1879b], pp.12-19.

¹¹⁰“Die Giltigkeit des Princips von der Erhaltung der Anzahl ist die Vorbedingung für die Anwendbarkeit der in den folgenden Abschnitten abgeleiteten Resultate und Methoden”, [Schubert, 1879b], p.18.

¹¹¹We shall come back to these formulae in more detail later in this section.

Beiträge, namely the use of coincidences of pairs of elements, and of a reinterpreted principle of correspondence. We analyze how these techniques were used by Schubert and Hurwitz to prove Chasles’ $\alpha\mu + \beta\nu$ formula. We then turn to Schubert’s 1879 rewriting and reorganization of these techniques, which we present through the example of the enumerative geometry of the triangle published in the wake of the *Kalkül*. This is a good example for two reasons. First, it is a rather simple and visual case-study to become familiar with these methods. Second, and more importantly, it was one of the main example mobilized by Halphen in his polemical epistolary exchange with Schubert throughout the years 1878-1880. The epistemological content of this exchange, however, will only be studied in the next chapter.

7.3.1 The principle of conservation of number and incidence formulae

As we already mentioned, the principle of special position was first introduced in Schubert’s 1874 paper on cubics (see 7.1.2). It would be reproduced in the 1876 *Beiträge* with further comments and details as an important tool for deriving formulae from fundamental conditions. In fact, in 1876, Schubert also suggests other names for this principle, such as “principle of indifference”, or the one which he would keep in future articles and books, namely the “principle of conservation of number”. Schubert encapsulated this principle in the following statement¹¹²

Die räumliche Lage der Gebilde, welche gewisse einem Gebilde A auferlegte Bedingungen verursachen, ist für die Anzahl der Gebilde A , welche diese Bedingungen erfüllen, gleichgültig, sobald diese Anzahl überhaupt endlich bleibt.

A simple example of the sort of formulae which Schubert derives from this principle is the following¹¹³. For rays (that is to say straight lines) in space, the symbols g, g_p, g_e introduced earlier can be understood to express the fundamental conditions that a ray respectively intersects a given ray g , passes through a given point p , or lies in a given plane e . Consequently, the composed condition g^2 expresses the requisit that a ray intersects two independently given rays. The number of rays satisfying this condition, and thus the symbol g^2 itself, does not vary when the position of the two given rays vary¹¹⁴. Thus Schubert takes them to be coplanar (in a then-given plane e), that is to say to intersect

¹¹²“The spatial position of the figures which generate certain conditions imposed on a figure A is indifferent to the number of the figures A which satisfy these conditions, as long as this number remains finite”, [Schubert, 1876a], p.23.

¹¹³ [Schubert, 1876a], p.25.

¹¹⁴Indeed, it is crucial to notice that while the principle is asserted with respect to supposedly-finite numbers, here it is used to compute a formula between conditions whose dimension is strictly smaller than the number of constants of the figures to which they are applied. This suggests, once more, that Schubert

a then-given point p . For a ray to intersect them, it must then either go through p , or lie in e . Hence,

$$g^2 = g_p + g_e$$

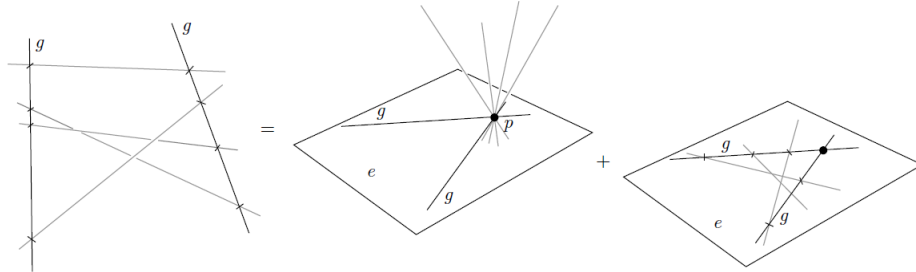


Figure from [Ronga, 2006], p.4.

However, as attentive readers will have noticed at this point, Schubert did not yet possess in 1876 an interpretation of the sum of symbols as a disjunction of conditions. In fact, Schubert here relied once more on this interpretation of enumerative formulae as functions which we presented earlier (subsection 2.2). To the principle “generally expressed” above, Schubert added a “particular form¹¹⁵”:

Kann den beiden Gebilden, welche eine aus zwei Factoren X und Y zusammengesetzte Bedingung XY veranlassen, eine derartige *specielle* Lage zu einander ertheilt werden, dass XY sich in die Bedingungen $z_1, z_2, z_3, ..$ *spalten* muss, so ist:

$$XY = z_1 + z_2 + z_3 + ..$$

Eine nach dem Pr. d. sp. L. abgeleitete Formel a^{ter} Dimension hat also links von Gleichheitszeichen ein *Product* zweier Symbole mit der Dimensionssumme a , rechts eine *Summe* a -facher einzelner oder zusammengesetzter Symbole.

Schubert does not explain in what consists the “splitting” of a composed condition other than by means of examples such as the one given above, and he attributes no geometrical meaning to the sums involved here. In the following sections of the *Beiträge*, this principle is only applied to other fundamental figures (and pairs thereof), albeit in ways which lead to much more sophisticated results than the example given above. This is

thinks of conditions in general as geometrical numbers, and not just in cases where they represent finite numbers of solutions.

¹¹⁵“If the two figures which give rise to a composed condition XY out of two factors X and Y can be given such a special relative position, so that XY must be *split* (*spalten*) into the conditions $z_1, z_2, z_3, ..$, then:

$$XY = z_1 + z_2 + z_3 + ..$$

A formula of a^{th} dimension, derived from the principle of special position, has for its left-hand term a *product* of two symbols whose dimensions add up to a , and for its right-hand term a *sum* of a -fold simple or composed symbols”, [Schubert, 1876a], pp.23-24.

no accident: for Schubert, at least in 1876, such was the role of the principle of special position to give fundamental results “free from the usual principle of correspondence and *Productensätze*¹¹⁶”, which in turn would be required as soon as more non-fundamental figures enter the picture - be they as simple as pairs of points. While the principle is presented as providing equations between products and sums, at several points Schubert transforms these equations to make subtractions appear, which are not given geometrical interpretations either.

In the 1879 *Kalkül*, unlike in the *Beiträge*, Schubert put forth the principle of conservation of number very early on, amidst definitions and notations. More precisely, the principle is stated right after the definitions of the fundamental objects of enumerative geometry and the basic symbolic laws operating on them, including the interpretation of products and sums as conjunctions and disjunctions of conditions; and right before the first discussions of the actual computations this symbolism allows. Schubert opens his discussion of the principle with the proposition which served to define the fourth kind of number at the center of enumerative geometry, namely ‘geometrical numbers’¹¹⁷:

Ist ein algebraisches Gebilde Γ mit der Constantenzahl c einer einzelnen oder zusammengesetzten c -fachen Bedingung z unterworfen, so giebt es im Allgemeinen eine *endliche Anzahl* N räumlicher Individuen, welche sowohl der Definition des Gebildes Γ , als auch der c -fachen Bedingung z genügen.

The principle is presented as “an important instrument of research” (*ein wichtiges Forschungsinstrument*), crucial for the determination of said geometrical numbers. It is also used to give meaning to enumerative formulae that Schubert is able to state, but yet to prove so early in his exposition of the *Kalkül*. While a nod is given to past formulations of this principle, it is now presented as possessing four distinct “forms”¹¹⁸:

- I. Eine Anzahl wird unendlich oder bleibt erhalten, wenn die gegebenen Gebilde *speziellere Lagen im Raume* einnehmen, also etwa unendlich fern werden.

¹¹⁶ [Schubert, 1876a], p.48.

¹¹⁷“If an algebraic figure Γ , of number of constants c , is subjected to a c -fold simple or composed condition, then there is in general a *finite number* N of spatial individuals, which satisfy at the same time the definition of the figure Γ and the c -fold condition”, [Schubert, 1879b], p.12.

¹¹⁸“I. A number either becomes infinite or is conserved if the given figures adopt *more special positions in space*, for instance if they become infinitely distant.

II. A number either becomes infinite or is conserved if the given figures adopt *more special positions with respect to one another*, for instance if given points lie on given rays.

III. A number either becomes infinite or is conserved if the given figures Γ' which were considered general to begin with, are repaced by *more special figures*, which satisfy the definition of Γ' , for instance if a general given conic section is replaced by a conic section whose points form two straight lines, and whose tangents form two bundles of rays whose vertex lies at the intersection of both straight lines.

IV. A number necessarily becomes infinite for a certain position of the given figures if it is observed at a value greater than N , while at another position it gave rise to a value which is exactly N ”, [Schubert, 1879b], pp.12-13.

- II. Eine Anzahl wird unendlich oder bleibt erhalten, wenn die gegebenen Gebilde *speciellere Lagen zu einander* einnehmen, also z.B. gegebene Punkte auf gegebene Gerade fallen.
- III. Eine Anzahl wird unendlich oder bleibt erhalten, wenn an die Stelle der zunächst allgemein gedachten gegebenen Gebilde Γ' *speciellere Gebilde* treten, welche die Definition der Γ' erfüllen, also z.B. an die Stelle eines gegebenen allgemeinen Kegelschnitts ein Kegelschnitt tritt, dessen Punkte zwei Gerade und dessen Tangenten zwei Strahlbüschel bilden, deren Scheitel in den Schnitt dieser beiden Geraden fällt.
- IV. Eine Anzahl wird bei einer gewissen Lage der gegebenen Gebilde nothwendig *unendlich*, wenn für sie ein Werth *grösser als* N constatirt ist, während bei einer anderen Lage sich ein Werth ergibt, der genau gleich N ist.

In an endnote, Schubert identified some of his previous uses of the principle of conservation of number to some of these forms. For instance, what was used in his 1874 paper, or the $g^2 = g_p + g_e$ formula presented previously, he understood to belong to the second form of the principle, which more aptly corresponds to the name “principle of special position¹¹⁹”. Similarly, Schubert interprets a paper of De Jonquières’ as relying on the third form, and Hurwitz’ second publication ever as relying on the fourth form. In this last case, Schubert is referring to Hurwitz’ work on closure problems, for which the contraposition of the principle is more useful¹²⁰.

Schubert then displays the uses and importance of this principle through a list of examples of various kinds¹²¹. The very first example given by Schubert has since become paradigmatic; it is the enumeration of the straight lines (in space) intersecting four given lines¹²². To determine this number, Schubert ‘specializes’ the position of the given figures, namely the four straight lines, which, for the sake of clarity, I will denote g_1, g_2, g_3, g_4 . In particular, using the second form of the principle, Schubert specializes their **relative** positions, by supposing that g_1 and g_2 , resp. g_3 and g_4 , intersect¹²³. Then, for a line to intersect all four straight lines, it must either be joining the two intersection points; or it must be at the intersection of the two planes defined by the two pairs of given straight lines. Schubert does not justify this claim, from which he derives the fact that the number

¹¹⁹ [Schubert, 1879b], p.334.

¹²⁰On the constitution of these ‘closure problems’ as a category, and on the paper of Hurwitz mentioned by Schubert, see [Lê, 2018], especially pp.264-266.

¹²¹ [Schubert, 1879b], pp.13-18.

¹²²It is easy to see from a parametric equation of a straight line in space that the number of constants of this figure is 4.

¹²³While Schubert is not entirely explicit as to what sorts of specializations are allowed, he does not make stronger assumptions, for instance taking all four lines to lie in the same plane, or even to intersect at the four corners of a square.

N of straight lines satisfying all four conditions is 2 in general. Here is how it can be understood. If a straight line does not lie in the plane defined by g_1 and g_2 , then the only way it can intersect both of these lines is by passing through their intersection. Similarly, this line must either lie in the plane defined by g_3 and g_4 , or go through their intersection. Here, one must assume that Schubert specialized the relative position of these four lines in a way that prevents the intersection of g_1 and g_2 to be in the plane defined by g_3 and g_4 (and vice versa), so that the only two lines satisfying the four conditions are those he gave. Indeed, for other special positions of the given straight lines, the number N can be shown to be strictly greater than 2; such is the case for instance when three of the given straight lines have one point in common. Then, per the fourth form of the principle, the number N is actually infinite.

It must be noted that this example did not belong to the realm of applications of the principle of special position in 1876; as it is not a symbolic equation between a product and a sum. Other examples given by Schubert later in the *Kalkül*, however, are of this form. As the “most important application of the principle of conservation of number¹²⁴”, Schubert identifies incidence formulae, to which we shall return at the end of this section.

Having discussed the evolution of this principle from 1876 (and even 1874) to 1879, one constant claim of Schubert must now be emphasized. For Schubert, this principle was always a consequence of a fundamental proposition which he borrowed from algebra, namely the *Fundamental Theorem of Algebra* (FTA), which states that a polynomial equation of degree n always has exactly n roots, albeit possibly complex and multiple¹²⁵. Schubert never spelled out precisely why it follows from the FTA that his geometrical numbers are well-defined. It is likely that he thought that the number of figures which satisfy a given condition can always be understood as the number of roots of a general algebraic equation, whose coefficients depend solely on the locus of the data in the condition, and whose degree depends on the nature of the figure and of the condition. Of course, as we saw in chapter 5 and the Chasles-De Jonquières controversy, this means that his methods count solutions whose geometrical significance may be lacking, but this is never a concern for Schubert.

This principle soon became a major point of focus for commentators of Schubert’s geometry, especially in the wake of Zeuthen’s 1914 *Lehrbuch der abzählenden Methoden der Geometrie*. As one American reviewer of this book for the *Bulletin of the American Mathematical Society* put it, “the fundamental principle of enumerative geometry is the

¹²⁴ [Schubert, 1879b], p.16.

¹²⁵ “[Diese Princip] sagt in algebraischer Interpretation nichts anderes als dass Veränderungen der *Constanten* einer Gleichung die *Zahl* ihrer Wurzeln entweder unberührt lassen oder aber unendlich verursachen, indem sie die Gleichung zu einer identischen machen”, [Schubert, 1879b], p.12.

law of the ‘preservation of the number’¹²⁶. More recently, the German mathematician Werner Burau strongly emphasized this principle in his brief account of Schubert's life and works, and so did the American geometer Steven Kleiman in his paper on Hilbert's 15th problem¹²⁷. In parallel, this principle also became the main point of focus of those who criticized Schubert's geometry as being insufficiently rigorous; criticisms which we discuss in the next chapter.

Many of Schubert's readers and commentators identified the principle of conservation of number with Poncelet's *principe de continuité*. For instance, the second section of Pieri's translation of Zeuthen's entry for the *Encyklopädie* is simply entitled “Loi de la conservation du nombre (Principe de continuité)”, and Schubert is merely presented as one name in a long list of geometers who used and expanded on Poncelet's principle¹²⁸. At first glance, this comparison may seem reasonable. Both Poncelet's and Schubert's principles express something about the permanence of an object under the effect of a change in position. However, the comparison quickly falls short. Poncelet thinks in terms of continuous motions, and is concerned with the introduction of general geometrical figures, whose definition may depend on intermediary objects which become imaginary in the process. Poncelet's principle delineates a specific class of geometrical properties, namely projective properties, whose characteristic is precisely to be preserved by said continuous change¹²⁹. Schubert's numbers, on the other hand, sometimes blow up to infinity; and some of them quantify figures which satisfy metric conditions. In fact, there is little concern for projectivity in Schubert's geometry, and there are simply no references to Poncelet in all of Schubert's writings on geometry, save for a couple of unrelated occurrences. Lastly, Poncelet's principle goes “from the general to the general”¹³⁰: that is to say that from a figure in general circumstance of construction are deduced other general truths about said figure. On the contrary, Schubert's principle goes from the particular to the general: from a specialization of the data of a problem, he deduces a number or formula which is taken to be valid in the general case. In fact, if an anterior proposition is to be sought for comparison with Schubert's principle of conservation, Peacock's and Hankel's principle of permanence is likely to be a much better candidate¹³¹.

Both in 1876 and 1879, the principle of conservation of numbers is immediately employed by Schubert to obtain a large number of formulae between fundamental conditions. While the list of formulae remained roughly the same across both texts, the order and

¹²⁶ [Allen, 1915], p.86. See also [Zeuthen, 1914a], pp.1-11; 31-33, and [Zeuthen, 1905], pp.270-271.

¹²⁷ [Burau & Renschuch, 1966], p.14; [Kleiman, 1976b], pp.447-448.

¹²⁸ [Pieri, 1915], pp.268-274.

¹²⁹ [Poncelet, 1865], pp.xiii-xiv.

¹³⁰ [Chemla, 2016], pp.68-69.

¹³¹In the following chapter, we show how Schubert's conception of numbers was influenced by Hankel in various ways.

overall presentation thereof changed.

In 1876, Schubert began by writing the symbols expressing the conditions that a main element (*Hauptelement*, that is to say a point, a ray, or a plane) belong to a fundamental figure (say, a bundle of rays)¹³². These symbols, which he had listed previously, include for instance p , p_g , and P , which respectively stand for the conditions that a *Punktfeld* (a plane viewed as a point-figure) contains a given *Punkt* (point), that it contains a given *Punktaxe* (rays made of points), and that it contains – or indeed is – a given *Punktfeld*. From these definitions, it follows that $p^2 = p_g$, and that $pp_g = P$, hence

$$p^3 = P$$

For rays, we already mentioned the fact that it follows from the principle of special position that, for instance, $g^2 = g_p + g_e$.

Next, Schubert considers figures composed of two main elements, such that one is a support (*Träger*) for the other (this is different from a *Grundgebilde*, because the support is here regarded as a part of the figure too). A point can be carried by a point, by a ray viewed as a point-figure (*Axe*), or by a plane viewed as a point-figure (*Feld*). Proceeding to the same listing for rays and planes, Schubert finds that there are only four such pairs of main elements, namely¹³³:

- Point and ray: the point is a bundle (Bündel) to the ray; the ray an axis (Axe) to the point
- Plane and ray: the plane is a field (Feld) to the ray; the ray an axis to the plane
- Point and plane: the point is a bundle to the plane; the plane a field to the point
- Ray and ray: each of the rays is an axis to the other

In 1876, Schubert maintained a strongly formal approach to the ontology of his geometry, which makes his description of these conditions hard to understand. In 1879, however, he obtained the exact same list when introducing what he called “incidence formulae”. Incidences, for Schubert, are precisely these figures obtained by taking together two main elements, such that their relative position is specialized into an incidence. The four figures which can thus be formed are then described as¹³⁴:

- Point and ray, if the point lies on the ray, or, which is the same, if the ray goes through the point.

¹³² [Schubert, 1876a], pp.25-26.

¹³³ [Schubert, 1876a], pp.26-30.

¹³⁴ [Schubert, 1879b], p.25.

- Plane and ray, if the ray lies in the plane.
- Point and plane, if the point lies in the plane.
- Ray and ray, if both rays intersect.

An incidence formula is then defined as “an equation between the fundamental conditions of these four incidences”. What, then, is a fundamental condition for these figures? And how to compute with it?

Consider, for instance, the first of these figures; namely the system of a point p and a ray g , such that the point p lies on the ray g . Fundamental conditions on this figure are formed by combining fundamental conditions on p and g . For instance, the condition symbol pg represents the condition that the point p lies in a given plane, and that the ray g intersects a given ray. By the principle of conservation of number, Schubert can specialize the position of these given objects, for instance taking the given ray to lie in the given plane. Then, every figure whose point p lies on the given ray, and every figure whose ray lies in the given plane, satisfy the combined condition. In the terms of the 1876 framework, Schubert has “split” the product condition pg , and obtained the following incidence formula (after the 1879 terminology):

$$pg = p_g + g_e$$

where, as a reminder, p_g is the condition that a point lies on a given ray g , and g_e that a ray lies in a given plane e . Notice that $p_g = p^2$, as p_g means for a point to lie on a given ray, and p to lie in a given plane, so p^2 means to lie at the intersection of two given planes, i.e. on a ray¹³⁵.

While Schubert never explicitly mentions a principle of duality, he often transforms formulae into their duals in ways that are made clear by the use of dual letters; for instance, to the formula above corresponds the following one (for figures formed by a plane and a point, so that the point lies on the plane):

$$eg = e_g + g_p$$

where e is the condition that a plane contains a given point, e_g that it contains a given line, and g_p that a ray goes through a given point.

The main import of these formulae, Schubert explains, is that “the calculus of conditions dispenses us after a simple application of the principle of conservation of number

¹³⁵There seems to be a misprint in Schubert's book ([Schubert, 1879b], p.22), wherein one finds the equation $p_g = p^3$ followed by the equation $pp_g = p^3$. This error is fixed in later editions, such as [Schubert, 1979].

from further geometrical considerations¹³⁶”. That is to say, from the incidence formulae of the lowest degree possible provided by the principle (as well as their dual forms), it suffices to carry out algebraic computations to obtain all other incidence formulae. Whilst they do not pertain to *Hauptelemente* themselves, they are sufficiently fundamental to be applicable to various, complex figures. For instance, Schubert uses them to enumerate tangents to spatial curves in the following pages of the *Kalkül*. Indeed, a tangent to a curve can be thought of as composed of a straight line and a point in an incidence relation (i.e. the point must be on the line), both of which also must touch the curve - although the verb ‘touch’ means something different for a line and for a point. Similarly, tangent planes to a surface can be investigated by means of incidence formulae.

7.3.2 Coincidences and correspondences

Incidence formulae are as far as Schubert goes with the sole help of the principle of conservation of number. The next kind of figure which he considers, both in the *Beiträge* and in the *Kalkül*, is the “pair of main elements” (*Hauptelementen-Paar*); and in particular pairs in a special position of coincidence¹³⁷. Note that the incidences discussed previously were not pairs of main elements, but only a sub-figure thereof, obtained by adding the condition that one of the two main elements lie on the other. There are naturally six kinds of pairs of main elements, three of which can present coincidences; those are called respectively “point-pairs” (*Punktpaar*), “plane-pairs” (*Ebenenpaar*), and “ray-pairs” (*Strahlenpaar*). Point-pairs and plane-pairs are dual figures, so Schubert hardly devotes any space discussing the latter. A coincidence is here defined by Schubert as one of these figures in which the two elements are infinitely close; in other words, it is a degenerate form (*Ausartung*) of the figure. It is a figure whose number of constants is that of the pair, minus one. For instance, the number of constants of the point-pair is $3 + 3 = 6$, and that of the related coincidence is 5. This might be surprising: why should two infinitely-near points not have the same number of constants as a single point? In the *Beiträge*, Schubert defines the point-pair as a figure whose Plückerian locus is not only composed of two points, but also of a ray which joins these two points¹³⁸ (which he calls the *Verbindungsstrahl*). Thus, such a coincidence is determined by a ray in space, and by one point on this ray; that is to say, its number of constants is $4 + 1 = 5$.

Just like for incidences, Schubert identifies as his main task to find the formulae

¹³⁶“Unser Bedingungskalkül dispensirt uns also schon nach einmaliger Anwendung des Principis von der Erhaltung der Anzahl von allen weiteren geometrischen Ueberlegungen”, [Schubert, 1879b], p.26.

¹³⁷ [Schubert, 1876a], p.48; [Schubert, 1879b], p.42.

¹³⁸ [Schubert, 1876a], p.49. In the *Kalkül*, the framework of the Plückerian loci is dropped. Instead, Schubert gives an ad hoc definition of the coincidence of two points wherein it is stipulated that one ray must be entirely determined as well; see [Schubert, 1879b], p.43.

which express coincidence conditions in terms of fundamental conditions, which he calls coincidence formulae¹³⁹. However, while the “source” of all incidence formulae was the principle of conservation of number, that of all coincidence formulae is another principle, namely Chasles' principle of correspondence¹⁴⁰. Schubert here explicitly refers to Chasles' 1864 paper; however, as we shall see, his use of the principle is much closer to Maillard's dissertation than to Chasles' lists of propositions (see 6.2.3 for our discussion of Maillard). One crucial difference is to be noted however: as Schubert rewrites the equations obtained by Maillard on the back of a new understanding of the principle of correspondence, he does so with different semiotics, and symbols standing for numbers become symbols standing for conditions, or indeed modes of degeneration. One example will make this clearer.

Considering a system of point-pairs, whose elements are denoted p and q , Schubert denotes g their *Verbindungsstrahl*, and ε the condition that it be a coincidence, that is to say that p and q are infinitely near, but nonetheless lie on a fully determined ray g . Thus, ε also denotes the number of such coincidences, in a system of point-pairs. Schubert then considers a pencil of planes turning about some arbitrary fixed ray l . Note that l is not a condition-symbol, but, more classically, a symbol denoting a geometrical object. This is why Schubert does not use an ambiguous letter which could also stand for a condition related to a given ray, for instance. Schubert then forms the following correspondence between planes of this pencil: to each plane which contains p correspond the planes which contain q , and conversely¹⁴¹. The principle of correspondence, Schubert claims without further explanation, implies that there are $p + q$ planes which contain both p and q .

Indeed, to a plane containing p correspond a certain number β of planes, namely the number of planes containing q . Per virtue of the symbolism of conditions, this number is also written q . Conversely, to a plane containing q correspond p planes. Therefore, the number of coinciding planes, that is to say of planes who belong to their image by this correspondence (in whichever direction) is $p+q$ (per the principle of correspondence, which states that the number of coinciding elements in an (α, β) -correspondence is $\alpha + \beta$)¹⁴². This is none other than the number of planes in the plane-bundle which contain both p and q . Note that, while the principle is also valid for finite values of p and q (since the

¹³⁹As Kleiman notes ([Kleiman, 1979], p.9.), “there are relations of neither sort, [...], for example, there is Chasles' relation $\lambda = 2\mu - \nu$ ”. However, Schubert proposes no specific categorization for them.

¹⁴⁰Like the principle of conservation of number, the principle of correspondence is here presented as a geometrical form of the Fundamental Theorem of Algebra, see [Schubert, 1879b], p.42. On Chasles' principle of correspondence, see section 4.2.2.

¹⁴¹Since Chasles mostly used the principle of correspondence when studying planar figures, he only ever considered series of points on a line, and series of lines turning about a point. Considering pencils of planes turning about a ray is a rather natural extension of this principle.

¹⁴²Note that this is the number of planes containing two corresponding points, and not two arbitrary given points, which is why this number is not equal to the product pq (and, indeed, not of the same dimension).

proof relies on the properties of complex polynomial equations), the resulting formula is more general: its symbols stand for conditions, that is to say numbers that can be infinite. Schubert does not mention or resolve this apparent discrepancy; which is dissolved when one thinks of these symbols as ‘geometrical numbers’.

Now, Schubert continues, there are only two occurrences in the system which can give rise to one of the $p + q$ planes which exclusively contain both p and q . Either they correspond to a case where p and q are infinitely near one another (in which case, a plane containing p necessarily contains q , and vice versa); or their *Verbindungsstrahl* g lies in the plane. This last case is equivalent to the condition that g and l intersect, as l necessarily lies in such a plane, per construction. There are ε planes corresponding to point-pairs of the first kind, as the symbol for the condition ‘being a degenerate point-pair’ also stands for the number in the system of such degenerate figures; and there are g *Verbindungsstrahlen* which intersect a given straight line l , as the symbol g also stands for the condition that a ray intersects a given straight line. Thus, Schubert writes, $p + q = \varepsilon + g$; or, to present this result in guise of a proper coincidence formula,

$$\varepsilon = p + q - g$$

This version of the principle of correspondence is one of Schubert’s two main devices for the obtention of symbolic formulae prior to their insertion in series of algebraic computations. Here, one sees in full display the computational power allowed by the ambiguity of Schubert’s symbols, which alternatively stand for conditions, numbers, and geometrical figures. This allows for a recasting of Chasles’ principle of correspondence, which borrows largely from the use thereof developed already in Maillard’s 1871 dissertation. As we saw in the previous section, Maillard also used the principle of correspondence to obtain a sum of two numbers as a number of coincidences, which was then decomposed into a finite list of kinds of coincidences. In Maillard’s dissertation, however, the resulting formulae only involved symbols standing for finite numbers, such as orders of curves or Plücker’s numbers. In Schubert’s enumerative geometry, the same method yields symbolic relations between geometrical conditions, which can then enter algebraic computations to solve *Charakteristikenprobleme*. We will give an example of such computations in the last part of this section.

7.3.3 Hurwitz’s and Schubert’s proof of Chasles’ theorem

All these techniques were put to use in a paper written in 1876, in the immediate aftermath of the *Beiträge*, by Schubert and one of his most famous pupils, the young Adolf

Hurwitz¹⁴³. The latter was born in 1859 near Hildesheim, the city where Schubert taught between 1870 and 1876. Like his older brother Julius, Adolf Hurwitz excelled at his studies, and especially in mathematics: they both spent many a Sunday afternoon at Schubert's home for geometry lessons¹⁴⁴. In this paper written right before Schubert's departure for Hamburg, he and his pupil set out to provide yet another proof of Chasles' $\alpha\mu + \beta\nu$ theorem, this time relying on the new methods and notations of *abzählende Geometrie*. Through this paper, Schubert and Hurwitz were directly and explicitly replying to the doubts raised earlier this year by Halphen. However, for reasons discussed previously, Halphen had not yet been able to widely communicate his alternative theory, or the alleged causes for the falsity of Chasles' theorem. In fact, it seems that Hurwitz was the main author of the proof proper, while Schubert was responsible for the general framework and the creation of the methods used in it. Hurwitz would be named first author in the published version, and, in private letters, Schubert often referred to this proof as 'Hurwitz's proof'¹⁴⁵.

The context of this paper, unlike the *Beiträge* (and most of the *Kalkül*), is that of plane geometry; and the figure under study is, of course, the conic section. The first section of this paper is devoted to a brief reformulation of Chasles' theory of characteristics and of the $\alpha\mu + \beta\nu$ theorem in the language of Schubert's enumerative geometry. To that end, Hurwitz and Schubert consider a system of conics Σ_1 and its characteristics (μ, ν) , as well as a simple condition Z_1 . The latter is satisfied by (and thus defines) a system Σ_4 containing ∞^4 conics. Schubert and Hurwitz then define the characteristics of this system as $(\mu'^4, \mu'^3\nu', \mu'^2\nu'^2, \mu'\nu'^3, \nu'^4)$ (where μ'^4 is the number of conics satisfying Z_1 and passing through four given points, and so on). Remember that Schubert had defined, in the *Beiträge*, characteristics (or elementary numbers) of a system of curves as numbers deriving from the (arbitrarily chosen) collection of so-called elementary conditions (here, touching a given line and passing through a given point), with no restriction on the dimension of said system (see 7.2.3). Thus, in the case of the system Σ_4 of conics, they form the five numbers of conics in the system passing through a points and touching $4 - a$ lines, for a ranging from 0 to 4. The fact that these numbers are indeed 'characteristics',

¹⁴³ [Hurwitz & Schubert, 1876].

¹⁴⁴See [Oswald & Steuding, 2014] for more on the lives of the Hurwitz brothers and their later work on complex continued fractions. While both brothers were deemed promising by Schubert, the Hurwitz family was not well-off, and only Adolf was allowed to pursue a career in mathematics. This may be why Schubert elected to co-write an article with Adolf and not Julius. Adolf Hurwitz went on to study with Klein in München, and later with Kronecker and Weierstrass in Berlin. These choices are likely to have been suggested or influenced by Schubert, who was a good friend of Klein's and an admirer of the Berlin school of mathematics.

¹⁴⁵"Sie werden meine Note mit dem Beweise von Hurwitz in Händen haben", Cod Ms 5624 170, Letter Schubert to Halphen, dated December 4th 1876. Furthermore, Hurwitz is credited with the obtention of the proof in the paper, [Schubert, 1876a], p.506.

while suggested already by Halphen’s rewriting of Chasles’ theorem, is in all rigour only a consequence of the proof itself: the proof shows that these numbers allow for the writing of a *Produktensatz*, which in turn is the definition of characteristics that Schubert had put forth. This is of course not a logical fallacy, as to consider these numbers as characteristics before they are shown to be such has no bearing on the proof itself.

Chasles’ theorem is thus presented as stating that the number of conics belonging to both Σ_1 and Σ_4 is $\alpha\mu + \beta\nu$, where α and β depend only on Σ_4 (or, alternatively, on Z_1 , or even on μ' and ν'). In fact, Schubert and Hurwitz show that α and β can be computed as functions of the characteristics of Σ_1 , which they contrast with the “a posteriori, that is to say, experimental determination¹⁴⁶” of these two numbers. For Chasles, it was arguably not crucial to prove the theorem $\alpha\mu + \beta\nu$ because he had a method to systematically generate modules (or, equivalently, the coefficients α and β) of each possible condition – and a proof that the formula was generally valid was of no use to him. Schubert and Hurwitz prove, in fact, a real *Produktensatz*, whose form is not actually $\alpha\mu + \beta\nu$ but rather $P(\mu', \nu') \cdot \mu + Q(\mu', \nu') \cdot \nu$ where P and Q are two homogeneous polynomials of degree 4. In effect, what Schubert and Hurwitz are proving is a more precise version of Halphen’s reformulation of Chasles’ theorem (discussed in 7.1.1), which stated (in particular) that Z_1 is characterized by a homogeneous polynomial of degree 4 in the variables μ', ν' (provided that one follows Schubert in his extension of the meaning of the p and d). Equivalently, this means that every number formed by the intersection of Σ_1 with a system of level 1 can be expressed as a linear combination of the symbols of order four listed above.

True to Schubert’s organization of the enumerative geometry of a given figure, the authors move on to discuss the modes of degeneration of the specific figure at play. Conics in a fixed plane have a number of constants of 5. There are two degenerations of conics of number of constants equal to $5 - 1 = 4$, namely line-pairs ε and point-pairs δ . Here again, Schubert and Hurwitz take symbols to stand also for the number of such conics in a system. With these notations, they can rewrite results already obtained by Chasles, thus imbuing them with extra meaning: for instance, the number of degenerate conics of each kind in a system Σ_1 can now be written as

$$\begin{aligned} 2\mu - \nu &= \varepsilon \\ 2\nu - \mu &= \delta \end{aligned}$$

But these equations are now more than numbers of degenerate conics, as they express algebraic relations between condition-symbols. Furthermore, a third degeneration η arises when the two lines (resp. two points) of the pair coincide; this degenerate figure has 3 for

¹⁴⁶ [Schubert, 1876a], p.505.

its number of constants. For Schubert and Hurwitz, this degeneration is none other than the two-fold degeneration composed of the two simple degenerations ε and δ . Thus, from the equations above easily follows that $\eta = (2\mu - \nu)(2\nu - \mu)$. These two-fold degenerations would not appear in Schubert's and Hurwitz's proof, but merely serve toward the end of the paper as a way of explaining Halphen's doubts away: Chasles' formula, the authors write, is always true, provided all degenerate conics are counted amongst the solutions. If the formula is understood in a different manner, which excludes certain "solutions étrangères", then the occurrence of η degenerations may cause the formula to be sometimes inexact¹⁴⁷. However, it is wholly "unwarranted and unpractical" (*ungerechtfertigt und unzweckmässig*) to make such a demand. Zeuthen would identify this part as the main weakness of Schubert's and Hurwitz's proof, because he understood Halphen's theory as showing that η -degenerations depended on 4, and not 3, simple conditions (or, in other words, that they had to be viewed as simple degenerations, on the same level as the other two). One can see how hard it is to accommodate this idea with Schubert's understanding of what a figure and a condition are. We shall come back to the lingering disagreement between Halphen and Schubert in the next chapter.

Once this translation of the theory of characteristics into Schubert's symbolism is complete, Hurwitz presents his proof of Chasles' theorem itself. His strategy is to use the principle of correspondence in the way described in the previous section. Hurwitz forms a correspondence on a line, whose number of coinciding points determines the number of points through which go two conics of respectively Σ_1 and Σ_4 , whose four intersection points lie on a given conic. This number is then decomposed into a sum through a discussion of the various ways in which such coinciding points may arise. This yields an equation between sums of symbols, one of which will prove to be the one sought after.

To that end, Hurwitz fixes a straight line g and a point A on g , as well as a conic K in the plane. To the point A correspond μ conics of Σ_1 (that is to say, there are μ conics in Σ_1 passing through A), each of which intersects K at four points C, D, E, F . There are μ'^4 conics of Σ_4 which pass through all of these four points; and each of these conics intersects g at two points. We take these points to correspond to A ; and, in total, to the point A correspond $2\mu'^4\mu$ points B on g .

Conversely, to a point B on g correspond a certain number u of conics of Σ_4 which go through B and whose four intersection points with K are so that there is a conic in Σ_1 passing through them. Each of these conics intersect g at two points, thus totalling $2u$ points on g corresponding to B . In order to determine u , Hurwitz and Schubert elect to use the principle of special position¹⁴⁸. Supposing that the point B lies on the conic K

¹⁴⁷ [Hurwitz & Schubert, 1876], pp.515-517.

¹⁴⁸ While the principle of special position is described by Hurwitz as the "fastest" way to obtain $u = \mu'^4\mu$, they also show how to obtain this number using only the principle of correspondence, [Hurwitz & Schubert,

(thus specialising the locus of the data), there are μ conics of Σ_1 going through B , and these conics cross K at three other points. Through these three points and through B go μ'^4 conics of Σ_4 . Consequently, there are $\mu\mu'^4$ conics going through B and whose four intersections with K (among which is B) satisfy the description above. By the principle of special position, this number is preserved for all positions of B and K . Thus, to B correspond $2\mu'^4\mu$ points A , and the correspondence has a total of $4\mu'^4\mu$ coinciding points (AB) .

Such coincidences (*Coincidenzstellen*) (AB) are points on g through which go two conics (respectively in Σ_1 and Σ_4) so that the four intersections C, D, E, F of the conics lie on K . Such coincidences can arise for three distinct reasons, Hurwitz explains. The first kind of coincidences is simply the intersections of K and g : by such a point go μ conics of Σ_1 , each of which intersect K at four points (including (AB)) through which go μ'^4 conics of Σ_4 . Thus, the intersections of K and g yield $2\mu\mu'^4$ coincidences, because there are two intersections of K and g .

A second kind of coincidences is produced by the x conics common to both systems. Such a conic intersects g at two points; for each of which, the common conic in question itself (and only it) satisfies the requirement. There are $2x$ such coincidences, because each conic x intersects g twice.

Lastly¹⁴⁹, coincidences occur due to the degenerate conics ε in Σ_1 , that is to say conics formed by double-lines, of which there are $\varepsilon = 2\mu - \nu$ in Σ_1 . Such conics intersect g at one double point P , and intersect K at four points C, D, E, F , where C and D (resp. E and F) are infinitely close.

Referring to an unspecified result obtained by Chasles in his 1864-1867 communications¹⁵⁰, Hurwitz asserts that, in a system Σ_2 of characteristics $\mu'^2, \mu'\nu', \nu'^2$, the number of conics touching a given straight line at a given point is $\frac{1}{2} \cdot \mu'\nu'$. Consequently, Hurwitz continues, there are $\frac{1}{4} \cdot \mu'^2\nu'^2$ conics in Σ_4 which satisfy this condition twice, i.e. touch two given lines at two given points. In particular, this is the number of conics in Σ_4 which touch the ray (*Verbindungsstrahl*) joining C and D at the point (CD) , and which touch the ray joining E and F at the point (EF) . This means that there are $\mu'^4 - \frac{1}{4} \cdot \mu'^2\nu'^2$

1876], pp.508-509.

¹⁴⁹Schubert and Hurwitz assert that it is “obvious” (*ersichtlich*) that these are the only possible ways for coincidences to arise, [Hurwitz & Schubert, 1876], p.510.

¹⁵⁰Hurwitz also refers to [Schubert, 1876c], p.341, where an analogous result is stated but not proven. In this article, Schubert claims that the formula is easily obtained with the help of the principle of special position. An easy way to understand this formula, which perhaps is not what Schubert had in mind, is to show instead that $\mu'\nu' = 2z$, where z is the condition ‘touching a given straight line at a given point’. The symbol $\mu'\nu'$ expresses the condition that a conic in Σ_2 touches a given straight line g , and passes through a given point p . Per the principle of special position, one can take p to lie on g . The condition is then only satisfied by conics which are tangent to g at the point p (for a conic cannot have three contacts with a straight line), and in fact it is satisfied by all of them. However, each conic tangent to g at p is counted twice, for they pass twice by the point p , hence the factor 2, and Schubert’s formula.

conics in Σ_4 which pass through all four points C, D, E, F but are not tangent to (CD) or (EF) . However, a conic section which contains both pairs of infinitely close points, without being tangent to K at both points, must be an ε -degeneration¹⁵¹ (and, conversely, all ε -degenerations pass through these points without being tangent to the conic K). Such conics intersect g twice at point P , and therefore give rise to $2 \cdot (2\mu - \nu) \cdot (\mu'^4 - \frac{1}{4}\mu'^2\nu'^2)$ coincidences of the third type.

Hurwitz continues by equating the number of coinciding points to the sum of their causes, using the ambiguity of a notation which stands equally for numbers and conditions or figures. Thus, he writes:

$$4 \cdot \mu \cdot \mu'^4 = 2 \cdot \mu \cdot \mu'^4 + 2 \cdot x + 2 \cdot (2\mu - \nu) \cdot (\mu'^4 - \frac{1}{4}\mu'^2\nu'^2)$$

Rewriting this equation, Hurwitz shows that:

$$\begin{aligned} x &= \mu \cdot \mu'^4 - (2\mu - \nu) \cdot (\mu'^4 - \frac{1}{4}\mu'^2\nu'^2) \\ &= \mu \cdot (\mu'^4 - 2(\mu'^4 - \frac{1}{4}\mu'^2\nu'^2)) + \nu \cdot (\mu'^4 - \frac{1}{4}\mu'^2\nu'^2) \\ &= \alpha \cdot \mu + \beta \cdot \nu \end{aligned}$$

where α and β are homogeneous polynomial functions of degree 4 of μ' and ν' only. This concludes Hurwitz's proof of Chasles' theorem. In the following section of the paper, a similar proof is sketched for Cremona's $\alpha\mu + \beta\nu + \gamma\rho$ theorem, wherein, similarly, polynomials in μ', ν', ρ' are given to express the coefficients α, β, γ .

Note that a new notation has been introduced throughout this proof: Schubert and Hurwitz had introduced earlier a difference between two kinds of products. The juxtaposition (*Nebeneinanderstellung*) of condition-symbols remains in use to form the conjunctions of conditions relative to the same system (or the multiplication of the numbers of elements satisfying said conditions), but products between symbols relative to both systems are denoted with a dot (\cdot)¹⁵²:

Eine Verwechslung der symbolischen Potenzen und Produkte mit wirklichen ist nicht gut möglich. Doch haben wir zur Unterscheidung bei wirklichen Multiplikationen meist einen Punkt als Multiplikationszeichen gesetzt, bei symbolischen aber nicht.

This distinction would be preserved by Schubert in later texts, especially when writing

¹⁵¹By subtracting the second term, Hurwitz removes degenerate conics composed of pairs of tangent straight lines to (CD) and (EF) .

¹⁵²"It is not really possible to confuse symbolic products and powers with real ones. However, we have usually set a dot as a multiplication sign for real multiplications, but not for symbolic ones", [Hurwitz & Schubert, 1876], pp.504-505.

Produktensätze, in order to differentiate between the contributions of each system. Schubert indeed makes a point of insisting on the fact that, in such a formula, we have in each factor a product of two conditions of maximal dimension with respect to the system to which they apply. These dots, Schubert explains, “mean that the resulting numbers are to be multiplied¹⁵³”. In other words, this is a final formula, one in which one can just plug (finite) numbers of elements in a system to solve actual enumerative problems. This distinction between juxtaposition and (\cdot) -signs is extra-theoretical: it concerns the uses and status of the formula, but not its meaning or the rules it obeys.

In conclusion, Chasles’ formula has been transformed throughout this proof in at least two ways: α and β are now polynomials in the characteristics of the condition Z (that is to say: not just two numbers characterizing the condition, but given as expressions valid for all conditions), but also the theorem is recast as one formula part of more general type of formulae, namely *Produktensätze*. Were it not for the fact that he could compute these functions for α and β , and thereby turn Chasles’ formula into the sort of theorem he had identified as crucial to enumerative geometry, Schubert would have probably shown little interest for this theorem. Where Halphen and Clebsch had set out to prove Chasles’ formula in order to ground its certainty, Schubert was more interested in computing the coefficients appearing in this formula, and in turning it into a synthetic expression which contains the whole of enumerative geometry for conics.

7.4 An example: the enumerative geometry of the triangle

7.4.1 Constructing the triangle

Right after the publication of the *Kalkül* in late 1879, Schubert wrote a 60-page paper for the *Mathematische Annalen* on the enumerative geometry of the triangle, entitled “Anzahlgeometrische Behandlung des Dreiecks¹⁵⁴”. This is a good paper to conclude our presentation of Schubert’s geometry for several reasons. First, its object is a reasonably simple yet not entirely elementary figure, the study of which will not require advanced geometrical knowledge as would be the case for Schubert’s geometry of the cubic. Therefore, in this section, we set out to and are able to give all of the required details to understand Schubert’s derivation of a *Produktensatz*. Second, this paper exemplifies quite neatly the structure and the goals of enumerative geometry as a whole. Its organization matches, on a smaller scale, that of the *Kalkül*, thus allowing us to envision more precisely

¹⁵³ [Schubert, 1880a], p.173.

¹⁵⁴ [Schubert, 1880a].

what Schubert had in mind as the ideal form that the application of his method ought to take. Last but not least, the geometry of the triangle became one of the focal points of Schubert's polemical exchange with Halphen throughout the year 1879 (via private communications of these yet unpublished results), to which we shall turn in sections 8.2.1 and 8.2.2. Let us note in passing that the results discussed here have attracted some attention in the second-half of the 20th-century, most of which resulted in attempts to prove more rigorously Schubert's formulae as part of a solution of Hilbert's 15th problem¹⁵⁵. The structure of Schubert's paper, which we follow closely here, is highly reminiscent of that of the *Kalkül*, albeit with an exclusive focus on a single figure. First, Schubert defines the triangle as a figure in the plane, its fundamental elements and conditions, as well as its degenerations; all of which are associated with symbols. Some fundamental formulae between these symbols are obtained along the way thanks to the principle of conservation of number. Then, Schubert uses the principle of correspondence to obtain formulae between these symbols, in particular relating degeneration conditions and fundamental conditions. Then, over the course of several sections, Schubert solves the *Charakteristikenproblem* by computing the number of triangles common to two systems of complementary dimensions, relying on a minimal set of conditions. The rest of the paper, which we will not cover for the sake of brevity, tackles more delicate issues regarding specific kinds of triangles – for instance, triangles in mobile planes.

As we already mentioned, Schubert opens his paper by redefining triangles as a geometrical figure in terms of 'main elements'. More precisely, triangles are here defined as figures composed of three points a, b, c (called in what follows vertices (*Ecke*)), and three rays α, β, γ (called in what follows sides (*Seiten*); wherein the sides join the vertices so that a is not on α , b not on β , and c not on γ . With triangles as with point-pairs, this redefinition is crucial especially in as much as it allows for the determination of the number of constants of this figure (which is 6), as well as for the description of its degenerate forms (which will be discussed below). Schubert is here working on a given plane; that is to say the triangles and systems thereof described in this paper are all coplanar¹⁵⁶. In effect, this is a construction of the triangle which follows the 'constructive' definition of figures proposed by Schubert in the 1876 Beiträge, that is to say the definition of figures as 'a collection of point-loci and ray-loci linked together in various ways' (see 7.2.3).

Since the triangle is defined by six main elements, there are six fundamental conditions which a triangle can satisfy. These conditions, which Schubert here calls 'conditions of position' (*Lagebedingung*), are denoted by the same symbols as the main elements. For

¹⁵⁵See [Sample, 1954], [Tyrrell, 1959], [Roberts & Speiser, 1981], [Roberts & Speiser, 1984], [Roberts & Speiser, 1986], [Roberts & Speiser, 1987].

¹⁵⁶The sole exception being the very last section of the paper, wherein triangles are in a mobile plane, see [Schubert, 1880a], pp.205-212. We shall not cover this section here.

a triangle whose vertices (resp. sides) are denoted a, b, c (resp. α, β, γ), these symbols also represent the condition that its vertices lie on a given line (resp. that its sides go through a given point). These are obviously conditions of dimension 1. Strictly applying the methodology put forth in the *Kalkül*, Schubert first investigates the incidence formulae relative to these conditions. Since the only main elements involved in the generation of the triangle-figure, the only incidences which can be formed here are relative to a point and a ray. The following six incidence formulae can be formed¹⁵⁷:

$$\begin{array}{ll} a\beta = a^2 + \beta^2, & a\gamma = a^2 + \gamma^2 \\ b\gamma = b^2 + \gamma^2, & b\alpha = b^2 + \alpha^2 \\ c\alpha = c^2 + \alpha^2, & c\beta = c^2 + \beta^2 \end{array}$$

Instead of establishing or explaining them, Schubert merely refers to his 1879 book, and in particular to the first page of the section on incidence formulae. The first example of such a formula, which we discussed earlier in this section, was the following relation:

$$pg = p_g + g_e$$

where p means for a point to lie on a given plane, p_g for a point to lie on a given ray, and g_e for a line to lie in a given plane.

At first, it remains unclear at this point how this formula and one for triangles such as $a\beta = a^2 + \beta^2$ are related. Not only are they not identical (one has a sum of squares on its right-hand side, the other a sum of two conditions of dimension 2), but the former bears on spatial (fundamental) figures, while Schubert’s systems of triangles are all coplanar (except for the systems considered toward the end of the paper).

In fact, these formulae seem to be less of a corollary to Schubert’s general incidence formulae, and more of an outcome of a similar method of proof, that is to say directly applications of the principle of conservation of number. Furthermore, they will later play a role in the enumerative geometry of the triangle similar to the one played by the general incidence formulae in the architecture of Schubert’s *Kalkül*.

One possible proof of these formulae, which Schubert does not give but might have had in mind, is the following: $a\beta$ expresses the condition that a point a lies on a given line, and that β goes through a given point. By the principle of special position (that is to say, the second form of the principle of conservation of number), one can suppose

¹⁵⁷ Analogous formulae for $a\alpha, b\beta, c\gamma$ are not correct because of the condition satisfied by all triangles that b and c be on α (and so on for the other terms). An expression for $a\alpha$ necessarily involves degeneration conditions, which we shall introduce shortly.

that the given point lies on the given line. Then either a is the given point, in which case any position for the points b and c and for the rays α, β, γ will satisfy the condition $a\beta$, as the side β will go through a , therefore through the given point; or β is the given line, in which case any position for the points a, b, c and for the rays α, γ will satisfy the condition $a\beta$, as the point a will necessarily be on β , that is to say on the given line. For a to be a given point is represented by the symbol a^2 (as we are working in planar geometry, thus two lines determine one and only one point), and similarly for β to be a given ray is represented by the symbol β^2 ; hence the formula

$$a\beta = a^2 + \beta^2$$

Multiplying one of these incidence formulae by the appropriate vertex symbol yields a new formula, e.g.:

$$a^2\beta = a^3 + a\beta^2 = a\beta^2$$

as a^3 is an impossible condition, that is to say as there is no point a on three given lines (in planar geometry). The same can obviously be done for the other formulae.

7.4.2 The degenerations of a triangle

To the six aforementioned *Lagebedingungen*, Schubert then adds another family of conditions, namely “invariant conditions” (*invariante Bedingungen*), that is to say conditions which express the fact that a triangle be “degenerated” (*ausgeartet*). A triangle is said to be degenerate, Schubert explains, “if it satisfies the general definition of the triangle, but possesses vertices or sides which are not in general [position] with respect to each other, but infinitely close¹⁵⁸”. This matches once again the structure of the *Kalkül*, as degenerate triangles and the symbols attached to them give rise to what was called in 1879 “coincidence formulae”.

There are several modes of degeneration, which constitute conditions of different dimensions. To enumerate and classify them, Schubert considers triangles inscribed in or circumscribed by a curve. For instance, the three vertices of a triangle moving along a given curve, it may happen that all three points become aligned, thus giving rise to a degenerate triangle within a system. This is the degeneration ε , consisting in a single ray g on which lie the three sides α, β, γ , as well as the three vertices a, b, c (at possibly distinct points, of course). A dual degeneration τ is then introduced, consisting in a single point s at which lie all three vertices a, b, c , and through which go the three sides α, β, γ .

Continuing on his description of the degeneration of a triangle circumscribed by a

¹⁵⁸“Ausgeartet ist ein Dreieck, wenn es die allgemeine Definition des Dreiecks erfüllt, aber Ecken oder Seiten besitzt, die zu einander nicht allgemein, sondern unendlich nahe liegen”, [Schubert, 1880a], p.154.

curve, Schubert considers the case where two points become infinitely close to one another (for instance moving toward a double point of a curve). In this case, the three vertices of the triangle will necessarily be aligned. Thus, he introduces the degeneration ϑ_a , which consists in a ray g , on which lies one point s . The vertices b and c are infinitely near s , and a lies anywhere on g . The sides β and γ are infinitely near g , while α can be any ray that goes through s . In an analogous fashion, the degenerations ϑ_b and ϑ_c are introduced. These degenerations are self-dual¹⁵⁹.

Here again, the symbols ε , τ , ϑ_a , ϑ_b and ϑ_c stand equally for a degenerate figure and for the condition ‘to be degenerate in a certain manner’, as well as for the number of such degenerate triangles in a given system. These conditions are all of dimension 1, as can be seen for instance from the fact that the figure composed of a ray and three points on it has for number of constants $2+3 = 5$. In what follows, Schubert always uses s and g to denote the points and rays on which degenerations occur, as well as the degeneration conditions associated thereto. However, these symbols do not mean the same thing depending on the kind of degeneration. For instance, τs^2 denotes the condition that a triangle be a τ -degeneration, that is to say a point s at the intersection of three rays, and that said point s be given (or, what is the same, at the intersection of two given rays). Here, the meaning of the symbol s is informed by the presence to its left of the symbol τ . The symbol $\vartheta_a s$, on the other hand, denotes the condition that a triangle be a ϑ_a -degeneration for which the point s , that is to say the point to which the vertices b and c must be infinitely close, lies on a given ray. The symbol s here has a different meaning; in fact, it could be replaced by either b or c , as to demand that either of those points lie on a given ray would yield the same composed condition. However, using s will allow for symmetry to be preserved in Schubert’s formulae later in the article.

In fact, the list of degenerations of triangles does not stop here. While all possible first-level degenerations have been exhausted (as no other simple coincidence can be formed than those of two or three sides or vertices), there are higher-level degenerations which can be formed by specializing the former ones. For instance, in a ε -degeneration, one can also demand that the points b and c be both infinitely near a given point. This second-level degeneration is denoted ω_a by Schubert, alongside the analogous degeneration symbols ω_b and ω_c . This degeneration can also be understood in terms of triangles inscribed in a curve; for instance, three points moving on a curve, consider the case when two of these points approach a double point of the curve from different branches in such a manner that the third point always be aligned with them. Here, we have a ε -triangle further degenerating, as two of its points become infinitely close to one another. While

¹⁵⁹If we tried to form an expression equivalent to $a\alpha$, it would have to involve these degeneration symbols, as this composed condition is only satisfied by degenerate triangles.

Schubert does not give this kinematic interpretation, it may be useful to understand how ω_a degenerations differ from ϑ_a degenerations. In a ϑ_a -degeneration, one also has two points infinitely near a given point s , but any ray going through s could be taken as the side α . Of course, in an ε -degeneration whose two points b and c are collapsing into one, the side α is entirely determined, as it coincides with the other two sides β and γ . This also justifies Schubert's remark¹⁶⁰ that " ω_a cannot just be seen as a special ε , but also as a special ϑ_a ". Per duality, Schubert also introduces $\omega_\alpha, \omega_\beta, \omega_\gamma$ degenerations. In a ω_α -degeneration, the sides β and γ of a τ -degeneration are infinitely near a given ray g . For these degenerations, as well as those still to be listed, the symbols s and g will be used as discussed previously.

One last second-level, self-dual degeneration, denoted ψ , is then described in rather puzzling terms by Schubert: it is said to "consist in three vertices infinitely near a given point s , and in three sides infinitely near a given ray g which passes through s , but in such a way that the three vertices are generally not aligned, but rather are like three successive points on a curve, and in such a way that the three sides do not generally intersect at the same point¹⁶¹". This description may seem like a contradiction in terms, if degenerate triangles are interpreted as diagrams or sets of points; but this cannot be the case, as Schubert further asserts that such a degenerate triangle is not fully determined by the given ray g and by the given point s . In fact, the distinction Schubert makes here is clearer once again when understood as a distinction between modes of degeneration. This degeneration, which Schubert also calls "infinitely small triangle" (*unendlich kleines Dreieck*), arises when, in a ϑ_a -degeneration, the point a is moved infinitely close to b and c (that is to say, to the given point s). However, Schubert adds, this degeneration cannot be understood as a special form of ε or τ : if, for instance, three points of a ε -degeneration collapse into one, they do so whilst being aligned. When the points of a ϑ -degeneration collapse, they do so with a certain "curvature" (*Krümmung*), which, with the data provided by s and g , fully determine the degenerate triangle. Demanding that the points of a triangle have a certain curvature, Schubert explains, is equivalent to demanding that the radius of the circumcircle of the triangle be of a certain length.

While Schubert does not discuss explicitly the possible values of this curvature for a ψ -degeneration, his introduction of the two level-three degenerations, both of which can be understood as special forms of the ψ -degeneration, makes this remark clearer. First, Schubert introduces η -degenerations as ε -degenerations wherein all three vertices a, b, c lie infinitely close to a given point s . The dual degeneration, ξ , is obtained by considering a τ -degeneration wherein all three sides α, β, γ lie infinitely close to a given ray g . These

¹⁶⁰ [Schubert, 1880a], p.156.

¹⁶¹ [Schubert, 1880a], p.156.

degenerations are both infinitely small triangles, inasmuch as all three sides and all three rays lie infinitely close to one another. However, they differ from ψ because the way their elements collapse into one another is not general. In particular, the curvature of a triangle tending toward a ψ -degeneration vanishes, while that of a triangle tending toward an η -degeneration goes to infinity.

Of course, if this discussion of degenerate triangles sounds contrived, it may partially be due to the fact that there are several possible models for the geometry of the triangle, some of which present different singularities; that is to say that there are no intrinsic descriptions of the degenerate forms triangles may take, and that the latter must be constructed. At any rate, this is certainly how things would be perceived by ulterior readers of this article such as John Semple, who described the aim of Schubert’s classification of the degenerations of the triangle as a “desingularization of the triangle domain¹⁶²”. This means that, of the many ways in which the triangles of a plane can be envisaged as forming a manifold, Schubert is constructing one in which there are no singularities, so that for instance the function T , which to a triangle associates the radius of its circumcircle, be defined smoothly over the entire manifold. What is striking is that, for Semple, this is exactly analogous to what Van der Waerden had done to conic sections in his paper on Chasles’ theory of characteristics, wherein the $\alpha\mu + \beta\nu$ formula was given its first widely-accepted proof. And indeed, Semple’s understanding of Schubert’s geometry of the triangle exemplifies the transformation mathematics had undergone in the first decades of the twentieth-century in the same manner as Van der Waerden’s proof does.

This concludes Schubert’s enumeration and classification of degenerate triangles, as all possible coincidences of the main elements constituting the figure which is here called a triangle have been surveyed. However, Schubert ends this section by remarking that there are other degenerations which can arise through the consideration of the analytical equation of the triangle. By this remark, Schubert is reacting to criticisms expressed by Halphen through the course of their correspondence, which we discuss in the next chapter. It suffices to say for now that Halphen had produced counter-examples to Schubert’s enumerative geometry of the triangle of the same nature as those he had produced to Chasles’ $\alpha\mu + \beta\nu$ formula, by studying the infinitesimal analysis of the degeneration of a triangle. Schubert, in his paper, brushed away these modes of degeneration by excluding them *a priori* from the systems of triangles on which his formulae bore, so as to save

¹⁶² [Semple, 1954], p.80. See also pp.83-84, especially the section called “Schubert’s triangle variable”. Semple, a student of Baker at Cambridge, is also the co-author with Roth of a 1949 textbook in algebraic geometry wherein a long chapter is devoted to enumerative geometry, [Semple & Roth, 1949], pp.296-342. From this document, a glimpse into the profound transformation of enumerative geometry in the wake of Van der Waerden’s papers can be gained. On Baker and Semple, see [Barrow-Green & Gray, 2006], especially pp.342-349.

them from Halphen’s counter-examples¹⁶³. We will come back to this issue later (see section 8.2.4). For now, we turn to Schubert’s solution of the *Charakteristikenproblem* for triangles.

7.4.3 Coincidence formulae for triangles

The second section of the paper gives formulae between the condition symbols defined previously. These formulae are classified by degree. Consequently, Schubert begins with first-degree formulae, that is to say coincidence formulae between the symbols for conditions of dimension one, namely $a, b, c, \alpha, \beta, \gamma, \varepsilon, \tau, \vartheta_a, \vartheta_b, \vartheta_c$. Whilst producing these formulae, Schubert purposefully eschews mentions to the principle of correspondence, instead preferring to speak only of coincidences. And yet, it is difficult not to see in them direct applications of the principle of correspondence as he had used it in the *Kalkül*, and in particular in the section on coincidence formulae. Indeed, the very first coincidence formula on triangles produced by Schubert is the following:

$$b + c - \alpha = \tau + \vartheta_a$$

which he justifies by considering the point-pairs formed by the vertices b and c of the triangles in a system. Such point-pairs, Schubert explains, coincide only in τ -degenerations or ϑ_a -degenerations, as follows from the description of first-order degenerations given previously¹⁶⁴. However, this explanation seems lacking when not supplemented by the knowledge that coincidences can be viewed as fixed points of correspondences, thus justifying the $b + c$ term in this formula. The term α itself is not even mentioned in Schubert’s explanation. In fact, a proof for this formula, very similar to those Schubert routinely produced in the *Kalkül* (such as the one given in subsection 3.2 of this chapter), can easily be obtained.

Indeed, considering the system of point-pairs (b, c) deriving from a system of triangles, their *Verbindungsstrahl* is by definition α . The condition that b and c coincide whilst remaining on the ray α is indeed $\tau + \vartheta_a$, for the reasons given by Schubert. Now, considering a pencil of rays turning about some arbitrary point p , one can form the following correspondence: to each ray passing through b correspond the ray passing through c , and conversely. This is not a $(1, 1)$ -correspondence in general, as there might be several triangles in the system with the same vertex b . Since the symbols b and c also stand for the conditions that these vertices lie on a given ray, and by the principle of correspondence,

¹⁶³This is not unlike what Lakatos called the “monster-barring strategy”, in *Proofs and Refutations*, see [Lakatos, 2015], p.45.

¹⁶⁴Remember that the higher-order degenerations are special cases of these ones, and thus need not appear in this first-degree formula.

the number of coincidences in this correspondence $b + c$. Besides the degenerate cases mentioned previously, such a coincidence can only arise if the ray is α , that is to say if α goes through the given point p , which is a condition also denoted by the symbol α . Hence,

$$b + c = \alpha + \tau + \vartheta_a$$

which is obviously equivalent to the formula stated by Schubert.

Several simple operations on such formulae are allowed. For instance, by permuting the vertices and sides, analogous formulae can be obtained, such as

$$c + a - \beta = \tau + \vartheta_b$$

Furthermore, all these formulae can be transformed into their dual form, through a simple passage from Latin to Greek letters¹⁶⁵ (as well as few inversions of Greek letters, such as ε and τ). For instance:

$$\beta + \gamma - a = \varepsilon + \vartheta_\alpha$$

Finally, these formulae can be combined through simple algebraic operations. For instance, subtracting the first formula from the last one, Schubert obtains:

$$a + b + c - \alpha - \beta - \gamma = \tau - \varepsilon$$

Notice that throughout this section, Schubert constantly puts the *Lagebedingungen* on the left-hand side, and the degeneration conditions on the right-hand side. From these algebraic manipulations, Schubert gathers that it is not possible to express any of the degeneration-symbols solely in terms of fundamental conditions. However, τ and $\vartheta_a, \vartheta_b, \vartheta_c$ can all be expressed as a function of fundamental conditions and ε . Therefore, so can $\vartheta_\alpha, \vartheta_\beta, \vartheta_\gamma$ owing to one of the dual formulae.

To move to second-order formulae, Schubert employs two methods. He either forms coincidences of a higher order (for instance, coincidences in first-order degenerations) and uses the principle of correspondence, or multiplies first-order formulae. Furthermore, other elementary methods such as duality and linear combinations are obviously still required and used.

An example of this first method is the following¹⁶⁶: remember that ω_a -degenerations were described as ε -degenerations in which the vertices b and c coincide. Thus, from a system of ε -triangles, one can form a system of point-pairs (b, c) . Forming a corre-

¹⁶⁵Such uses of notations to emphasize duality in formulae was of course nothing new, see for instance [Chemla, 1989].

¹⁶⁶Once again here, Schubert does not write the proof entirely, nor does he mention the principle of correspondence as necessary to the obtention of this formula.

spondence between rays turning about a given point p such that to a ray containing p correspond the rays containing c , and conversely, coincidences between rays can only arise when b and c coincide (which, since all the triangles are already ε -degenerations, means that the triangle is actually a ω_a -degeneration), or when the side α goes through the given point p . Since the triangle is a ε -degeneration, all three sides α, β, γ coincide, so that this condition can be denoted by the symbol g . Thus, by the principle of correspondence, $\varepsilon b + \varepsilon c = \omega_a + \varepsilon g$, or, as Schubert writes it:

$$\omega_a = \varepsilon b + \varepsilon c - \varepsilon g$$

An example of the second method, with which Schubert is once again rather concise, is the following. Multiplying by c the very first formula $b + c - \alpha = \tau + \vartheta_a$, one obtains:

$$bc + c^2 - \alpha c = \tau c + \vartheta_a c$$

However, $\tau c = \tau s$, as, in a τ -triangle, all three vertices coincide. Similarly, $\vartheta_a c = \vartheta_a s$. Furthermore, $\alpha c = c^2 + \alpha^2$, per the incidence formulae given at the very beginning of the paper. Hence, we obtain the following formula stated without proof by Schubert:

$$bc = \alpha^2 + \tau s + \vartheta_a s$$

In the same manner, over the course of the next pages, Schubert goes on to produce some 47 formulae of order up to 5 (to which must even be added dual forms). The real import of this plethora of formulae only appears clearly in a subsequent “survey” (*Uebersicht*), wherein Schubert notes that what he has shown is in fact that, for each dimension, a certain subset of conditions suffices to express all others¹⁶⁷. For instance, the first-order formulae and our remarks regarding the expression of degeneration-symbols show that all conditions of dimension one can be expressed as (linear) algebraic functions of the seven following symbols:

$$a, b, c, \alpha, \beta, \gamma, \varepsilon$$

As Schubert notes, this is by no means a specificity of this very set of symbols. The proposition remains true, for instance, if ε is replaced by τ . What is important, however, is that 7 is a minimal number of symbols, that is to say that no smaller set of symbols can suffice to express all others via algebraic combinations. While Schubert does not explicitly prove this fact, it follows rather clearly from his formulae. The question remains, however, as to why these symbols are said to be able to capture ‘all conditions’, and not just those for which symbols were expressly created. The answer is that Schubert thinks of geometrical

¹⁶⁷ [Schubert, 1880a], pp.163-164.

conditions as composed of incidences and coincidences of fundamental objects, which are precisely what the symbols he defined capture. From the highly-structured ontology and architecture of his theory derives the meta-mathematical claim that indeed, all conditions on triangles are captured by these symbols.

For second-order formulae, the number of necessary symbols rises up to 17; then, 22 are required to characterize conditions of dimension 3. Per duality, the numbers for dimensions 4 and 5 are respectively 17 and 7 as well. Note that the duality applies also to the symbols composing these lists. For instance, conditions of dimensions 5 can all be expressed as (linear) algebraic functions of the following symbols:

$$\varepsilon b^2 c^2, \varepsilon a^2 c^2, \varepsilon a^2 b^2, \tau \beta^2 \gamma^2, \tau \alpha^2 \gamma^2, \tau \alpha^2 \beta^2, \psi s^2 g$$

which correspond to those of the list of symbols characterizing first-order formulae.

Die Gleichungen *zweiter Dimension* drücken alle derartigen zweifachen Bedingungen durch die folgenden 17 *Bedingungen* aus:

$$a^2, b^2, c^2, \alpha^2, \beta^2, \gamma^2, a\alpha, b\beta, c\gamma, \tau s, \varepsilon g, \\ \vartheta_a s, \vartheta_b s, \vartheta_c s, \vartheta_a g, \vartheta_b g, \vartheta_c g.$$

Die Gleichungen *dritter Dimension* zeigen, dass man alle derartigen, dreifachen Bedingungen als Functionen der folgenden 22 *Bedingungen* darstellen kann:

$$a^2 \beta, a^2 \gamma, b^2 \alpha, b^2 \gamma, c^2 \alpha, c^2 \beta, a b c, \alpha \beta \gamma, \\ \varepsilon a^2, \varepsilon b^2, \varepsilon c^2, \tau \alpha^2, \tau \beta^2, \tau \gamma^2, \varepsilon g^2, \tau s^2, \\ \vartheta_a s^2, \vartheta_b s^2, \vartheta_c s^2, \vartheta_a g^2, \vartheta_b g^2, \vartheta_c g^2.$$

Ebenso überzeugt man sich leicht, dass man jede derartige *viereckige* Bedingung durch die folgenden 17 *Bedingungen* ausdrücken kann:

$$b^2 c^2, c^2 a^2, a^2 b^2, \beta^2 \gamma^2, \gamma^2 \alpha^2, \alpha^2 \beta^2, \tau s^2 \alpha, \tau s^2 \beta, \tau s^2 \gamma, \\ \varepsilon g^2 a, \varepsilon g^2 b, \varepsilon g^2 c, \vartheta_a s^2 g, \vartheta_b s^2 g, \vartheta_c s^2 g, \psi s^2, \psi g^2.$$

[Schubert, 1880a], p.164.

7.4.4 A theory of characteristics for triangles

These lists effectively can serve as sets of characteristics for systems of triangles of each order, in the same way as the symbols μ, ν (or, alternatively, Zeuthen’s symbols ω, λ for the two degenerations of conics) served as characteristics for systems of conics of dimension 1. As an example, Schubert considers the systems of triangles inscribed in a given curve of order n , that is to say the systems of triangles whose vertices a, b, c all lie on this given curve. It is of course a system of level 3, in which the 17 characteristic conditions of dimension 3 are satisfied by finite numbers of triangles, all of which can be easily computed. For instance, $b^2 c = b^2 \alpha = a^2 \alpha = 0$, as the condition that b lies at the intersection of two given rays, whilst being on the given curve, is generally satisfied by no triangle; while $\beta^2 \gamma = n(n-1)^2$. Indeed, $\beta^2 \gamma$ represents the condition that the side β goes through two given points and that γ goes through one given point. By the principle of conservation of number, one can suppose that these three given points are all on the given

curve, from which it follows, according to Plücker's formula $n^* = n(n-1)$, that there are $n(n-1)(n-1)$ triangles satisfying this condition¹⁶⁸. By expressing the third-order condition $\alpha\beta\gamma$ as a function of some of these 17 symbols, and by plugging in the finite values obtained for the system of triangles inscribed in a curve, Schubert is then able to compute rather easily that there are $n(n-1)(2n-3)$ triangles inscribed in a curve of order n whose sides go through three given points.

Finally, with these formulae and minimal sets of symbols, Schubert is equipped to derive (*ableiten*) a series of *Produktsätze* for triangles¹⁶⁹, that is to say symbolical expressions for the number of triangles common to two systems of triangles of dimension i and $6-i$. These expressions are the intended equivalent to Chasles' $\alpha\mu + \beta\nu$ formula, which Schubert reads as expressing the number of conics common to two systems of dimension 1 and 4, characterized respectively by the symbols (μ, ν) and (α, β) (where α and β are actually functions in the characteristics of the second system, see 7.3.3). Of course, there are different such formulae for the different values of i , just like to Chasles' formula corresponded Cremona's $a\rho + b\sigma + c\tau$ formula for the number of conics common to two third-order systems, which are characterized by the three numbers ρ, σ, τ (where the same caveat applies).

In fact, Schubert begins by considering not triangles, but rather a system of points b of level i (i being either 0, 1, or 2), and a system of points of level $2-i$. The number of points common to both systems is either b^2, bb' or b'^2 depending on the value of i . Indeed, if $i = 0$, then the second system is just a finite set of points b' , and the number of points common to both systems is the sum of the multiplicities of the b' as points of the first system, that is to say the number of points b coinciding with a b' . This can be expressed as b'^2 , where b'^2 represents the condition for a point b to be given. Conversely, b^2 represents the number of points common to both systems when $i = 2$. Lastly, when $i = 1$, both systems are point-loci of dimension one, that is to say, curves of degree b and b' , since these symbols also stand for the condition that b be on a given straight line. Per Bézout's theorem, bb' therefore represents the condition that a point lie at the intersection of both loci. Thus, the following expression gives in general the number of intersections of two systems of points:

$$b^2 + bb' + b'^2$$

as in each possible case for the value of i , two of these factors will vanish. Alternatively, this expression can be understood as the general number of coincidences in a system of point-pairs (b, b') .

¹⁶⁸Note that Schubert does not prove this result, but merely states it. It seems difficult, however, to obtain this number without relying on the principle of conservation of number.

¹⁶⁹ [Schubert, 1880a], pp.167-183.

At this point, Schubert progressively complexifies the figure he is working with, working his way toward the triangle as previously defined. First, Schubert considers not systems of point-pairs (b, b') , but systems of pairs of rays α on which a point b lies. He then enumerates coincidences of such a pair, that is to say the number of elements belonging to two systems of ‘rays + points on the ray’ of respective levels i and $3 - i$. Here again, Schubert considers all possible values of i , forms the symbolic expression for the number of common elements in the two systems, and adds these expressions as all of them but one vanish for each value of i . To do so requires knowledge of the number of coincidences of the previous figures. For instance, for $i = 2$, Schubert considers two systems of ‘rays α + points b on the ray’ and ‘rays α' + points b' on the ray’ of respective levels 1 and 2, and finds the number of their common elements to be equal to $\alpha b \cdot b' + \alpha' \cdot b^2 - b' \cdot b^2$; whilst an analogous formula is obtained for $i = 1$, wherein the dashes on the letters are merely switched. For $i = 0$ and $i = 4$, the numbers of common elements are respectively $b'^2 \alpha'$ and $b^2 \alpha$, as is easily shown. Adding all these numbers, and factoring in terms which he can recognize thanks to his discussion of the simpler case of the point-pairs, Schubert thus finds that the number of coincidences of the system of pairs $([\alpha, b], [\alpha', b'])$ (my notation) is equal to

$$(b^2 + b \cdot b' + b'^2)(\alpha + \alpha' - b')$$

This is the number of pairs in which α and α' coincide, as well as b and b' .

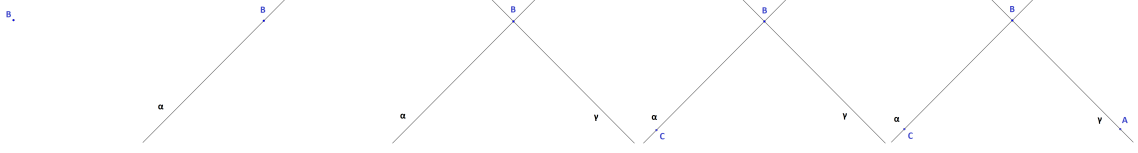
We shall not go into further technical detail regarding this computation, but it suffices to say that, adding further points and rays to the figure under consideration, Schubert eventually finds that the number of triangles common to two systems Σ and Σ' of level i and $6 - i$, with the usual notations, is none other than:

$$(b^2 + bb' + b'^2)(\alpha + \alpha' - b')(\gamma + \gamma' - b')(c + c' - \alpha')(a + a' - \gamma')$$

Notice that the product of the first two factors is none other than the previously-obtained number of coincidences for a simpler figure, just as the first factor of this previous number was that of coincidences of point-pairs.

However, this formula does not exactly compute triangles, as is clear from the fact that β does not appear in this formula. In fact, the way it was acquired was by constructing triangles in the following manner: first there was a point b , then Schubert added a ray α passing through b , then another ray γ also passing through b , then a point c on α , and finally a point a on γ (see fig. below). The number of coincidences of these successive figures was obtained by successive multiplications of factors taking into accounts the added elements. However, while the last figure reached by Schubert forms a triangle, its coincidences are not exactly those of a triangle. The number given above, in Schubert’s

words, counts “how often it happens, that b and b' , α and α' , γ and γ' , c and c' , a and a' coincide, counting therefore also these cases *in which furthermore β and β' do not coincide*¹⁷⁰”.



However, for two triangles to have a, b, c, α, γ in common, but not β , is only possible in some degenerate cases. More precisely, this case can only arise in two situations. Either a τ -degeneration τ in Σ and τ -degeneration τ' in Σ' are such that α and α' , γ and γ' coincide; or a ϑ_b -degeneration ϑ_b in Σ and a ϑ_b -degeneration ϑ_b in Σ' are such that g and g' , s and s' , b and b' coincide. The number of these pairs of degenerate triangles must be subtracted from the formula above to obtain a proper *Produktenform*. Now, the number of pairs of the first kind can be written as $\tau\tau'(s^2 + ss' + s'^2)(\alpha + \alpha' - s')(\gamma + \gamma' - s')$, which expresses the condition that both triangles be τ -degenerations, that their vertices (which are all collapsed infinitely close to two given points s, s') coincide, and that their sides α, α' (resp. γ, γ') coincide. A similar number can be given for the second kind of pairs of degenerations, so that Schubert can write the following expression for the number of triangles common to two systems Σ, Σ' of levels $i, 6 - i$:

$$\begin{aligned} X = & (b^2 + bb + b'^2)(\alpha + \alpha' - b')(\gamma + \gamma' - b')(c + c' - \alpha')(a + a' - \gamma') \\ & - \tau\tau'(s^2 + ss' + s'^2)(\alpha + \alpha' - s')(\gamma + \gamma' - s') \\ & - \vartheta_a\vartheta'_a(s^2 + ss' + s'^2)(g + g' - s')(b + b' - g') \end{aligned}$$

Such a formula, for Schubert, is not yet a *Produktsatz* (or a *Produktenformel*), but rather a *Stammformel* (‘primitive formula’) for all possible *Produktenformel*. Remember that a *Produktsatz*, for instance as defined in the 1876 *Beiträge* or in the *Kalkül* must be a sum of products of conditions pertaining to each system, that is to say a formula of the form¹⁷¹

$$b_1 \cdot e'_1 + b_2 \cdot e'_2 + \dots + b_m \cdot e'_m$$

This is not the case for the *Stammformel* given above. However, the latter is valid for all values of i , and more crucially, from it derive the *Produktsätze* for each of these values. As we have already explained, most of the terms of the *Stammformel* actually vanish for each value of i . More precisely, only the factors that have the appropriate numbers (that is to say, i and $6 - i$) of symbols with and without a dash subsist. Therefore, all that

¹⁷⁰ [Schubert, 1880a], p.171.

¹⁷¹ [Schubert, 1876a], pp.91-93 ; [Schubert, 1879b], pp.274-277.

remains to be done is to find out which terms remain each time, and rearrange them in the desired form. In fact, per duality, only three cases need to be examined, namely $i = 1, 2, 3$. Schubert thus devotes the next three sections of his paper to doing just that.

For the sake of brevity, in what follows, we shall focus on the case of two systems of triangles of respective levels one and five. To obtain this expression, it suffices to expand the general formula given previously, and to retain only the factors in which the total degree of symbols with dashes is 1, and the total degree of symbols without dashes is 5. Thus, the number of common triangles to these two systems, per the *Stammformel*, is:

$$\begin{aligned} x = & a' \cdot (b^2\alpha\gamma c) + b' \cdot (b\alpha\gamma ac - b^2\gamma ac - b^2\alpha ac) \\ & + c' \cdot (b^2a\gamma\alpha) + \alpha' \cdot (b^2\gamma ac - b^2\alpha\gamma a) \\ & + \gamma' \cdot (b^2\alpha ac - b^2\alpha\gamma c) - \tau' \cdot \tau s^2\alpha\gamma - \vartheta'_b \cdot \vartheta_b s^2gb \end{aligned}$$

where the \cdot sign is used in the same manner as in the 1876 proof of Chasles’ theorem.

In fact, Schubert goes on to manipulate this formula (for instance by expanding it, or substituting some of its terms using some of the formulae on conditions obtained previously) to make it symmetrical, and to conform it to the shape expected of a *Produktenformel*. After a few lines of relatively simple such computations, Schubert finally obtains the following *Produktenformel*:

$$\begin{aligned} x = & a' \cdot \varepsilon b^2 c^2 + b' \cdot \varepsilon c^2 a^2 + c' \cdot \varepsilon a^2 b^2 \\ & + \alpha' \cdot \tau \beta^2 \gamma^2 + \beta' \cdot \tau \gamma^2 \alpha^2 + \gamma' \cdot \tau \alpha^2 \beta^2 \\ & + d' \cdot \psi s^2 g \end{aligned}$$

Notice the symmetry (or duality) of this formula, as well as the fact that it has seven terms, that is to say the minimal numbers of conditions required to characterize first- and fifth-level systems.

With this last formula (and its equivalents for systems of other levels), the enumerative geometry of the triangle is complete – although Schubert goes on to give a few examples, to discuss further systems of infinitely small triangles, or even to generalize some of these results to triangles in space.

Conclusions

Schubert’s enumerative geometry, we have shown throughout this chapter, cannot be reduced to an application of Boolean logic to geometry, or to an intuitive yet unrigorous method for computing large numbers of geometrical figures. Rather, it is an intricate

symbolism which stems from various sources. The first one is Halphen's observation that modules can be meaningfully multiplied. Another one is that all geometrical figures can be formally constructed from fundamental elements, on which conditions and geometrical numbers can be defined and represented through symbols. A third and not lesser one is the influx of methods and results coming from the works of Chasles, Maillard, or Zeuthen, which Schubert systematically translated and incorporated into his own symbolism.

At the end of our presentation, one may still legitimately question the justifications for this symbolism, on at least two levels. The first level is that of the coherence and rigour of Schubert's computations. How can we know that these reasonings are sound and not leading to internal contradictions? In particular, what guarantees that juxtapositions of symbols and products of actual numbers of curves will work hand-in-hand when investigating any given figure, especially in the absence of theorems such as Chasles' $\alpha\mu + \beta\nu$ formula, which, at the very least, grounded Halphen's theorems. On another level, one may question the fact that Schubert's numbers do indeed capture a meaningful geometrical reality. As the case of triangles has made clear, for a systematic enumerative calculus to hold in general, and to result in the sort of theorems that Schubert pursues (namely *Produktensätze*), crucial choices must be made to include certain kinds of degenerations and to exclude others. But what, then, is being counted by Schubert's symbols? Even if the internal coherence of this theory can be proven, what supports the notion that its results hold any geometrical significance?

The answer to the first of these questions are, unfortunately, beyond the scope of the present thesis. In fact, those who attempted (and partially managed) to justify Schubert's inferential and definitional practices all had to resort to theoretical frameworks far removed from what is in the limits of our historical episode. The second question, however, was subject of many debates in the years following the publication of Schubert's book. It is to them that we turn in our next and final chapter.

From Truth to Significance: The Modernist Transformation of Enumerative Geometry

8.1 Chasles' formula at the heart of the modernist transformation of mathematics

8.1.1 A formula in flux

In the years following the publication of Chasles' theory of characteristics, many took up the central question it was designed to answer, namely the enumeration of conic sections in a plane satisfying five given conditions¹. Halphen or Schubert were but two of the many mathematicians who built off Chasles' work, either attempting to extend his methods to other figures, to translate his concepts into a language deemed more rigorous, or even to incorporate it into a broader theory. And yet, beyond this common question, a few notations borrowed from the theory of characteristics (such as the symbols μ and ν), and a few paradigmatic statements or axioms (such as the proposition that through any five given points always lies one and only one conic section²), there is little of Chasles' theory which survived into the works of its readers, be it mathematical style³, inferential practices, notations, or even identification of its general goal or value. This is true at the scale of individual readers, as we have shown in the previous two chapters; but not only so. Despite Chasles' and Schubert's attempts at providing a theoretical framework with

¹Of course, this was only part of Chasles' project, but concerns about the (theoretical) construction of these conics were quickly eschewed by subsequent readers of the theory of characteristics, as we have shown in chapter 6.

²This proposition was necessary for the sort of generality aimed at by these mathematicians, and yet its validity hinges upon a loosening of the criteria according to which a conic is judged to go through given points, as is made evident by the fact that this proposition is deemed false in modern-day textbooks; see for instance [Eiden, 2009], p.52.

³On the notion of mathematical style as a historiographical category, see [Rabouin, 2017].

Chapter 8. From Truth to Significance: The Modernist Transformation of Enumerative Geometry

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Vierter Abschnitt.

6. Die Bestimmung der Zahl der Curven, bei denen von den Schnittpunkten mit einer beliebigen Geraden zwei zusammenfallen, giebt:

$$4g - g + 6p + 2g + e_g$$

7. Die Bestimmung der Zahl der Curven, bei denen von den vier Tangenten aus einem beliebigen Punkte zwei zusammenfallen, giebt:

$$6g - g + 3f + 2 + e_g$$

8. Die Bestimmung der Zahl der Curven, bei denen zwei Wendetangenten zusammenfallen, giebt:

$$4f - 6p + 2v + 2g + e_g$$

9. Die Bestimmung der Zahl der Curven, bei denen zwei Wendepunkte zusammenfallen, giebt:

$$4v - 6s + 2x + 3y + e_g$$

10. Die Bestimmung der Zahl der Curven, bei denen zwei Schnittpunkte der Wendetangentendirektions zusammenfallen, giebt:

$$4u - 2f + 2g + p + e_g$$

11. Die Bestimmung der Zahl der Curven, bei denen die beiden Doppelpunktstangenten zusammenfallen, giebt:

$$2g - 2p + 2b + p + e_g$$

12. Die Bestimmung der Zahl der Curven, bei denen von den Tangenten aus einem beliebigen Punkte eine durch den Doppel punkt geht, giebt:

$$g + 4b - 3p + p + e_g$$

13. Die Bestimmung der Zahl der Curven, bei denen von den Schnittpunkten mit einer beliebigen Geraden einer auf einer Wendetangente liegt, giebt:

$$2v + 3f - 6p + 3v + e_g$$

14. Die Bestimmung der Zahl der Curven, bei denen von den Schnittpunkten mit einer beliebigen Geraden einer auf einer Wendetangente liegt, giebt:

$$2v + 3p - 6p + 6b + x + e_g$$

15. Die Bestimmung der Zahl der Curven, bei denen die Tangenten aus einem beliebigen Punkte eine einen Punkt u ent hält, giebt:

$$3g + 4u - 4x + 2f + p + e_g$$

Die Berechnung von Anzahlen durch Ausrechnen

16. Die Bestimmung der Zahl der Curven, bei denen Schnittpunkten mit einer beliebigen Geraden einer auf der punktgeraden s liegt, giebt:

$$s + 3s - 9p + v + e_g$$

17. Die Bestimmung der Zahl der Curven, bei de Strahl s mit einem Strahl f zusammenfällt, giebt:

$$8s + f - 3p + v + x + e_g$$

18. Die Bestimmung der Zahl der Curven, bei denen e aus einem beliebigen Punkte gezogenen Tangente der Best punkt mit dem dritten Schnittpunkte zusammenfällt, giebt:

$$g + 3v - 4p + f + p + e_g$$

19. Die Bestimmung der Zahl der Curven, bei denen Verbindungsgeraden eines beliebigen Punktes mit dem Dopp dritter Schnittpunkte zusammenfällt

$$b + v - p + f + e_g$$

20. Die Bestimmung der Zahl der Curven, bei denen e gente eines auf einer beliebigen Geraden erzeugten Schnitt mit einer der beiden andern von diesen Schnittpunkte aus Tangenten zusammenfällt, giebt:

$$2v + 3g - 6p + 2v + e_g$$

21. Die Bestimmung der Zahl der Curven, bei de Wendetangente zusammenfällt mit einer der beiden ande genten, die von dem Schnittpunkte der Wendetangente u beliebigen Geraden ausgehen, giebt:

$$2f + 3g - 6p + 2v + 2x + e_g$$

22. Die Bestimmung der Zahl der Curven, bei denen d andereno berührenden Tangenten, welche von einem Schni mit einer beliebigen Geraden ausgehen, zusammenfallen, gi

$$2v + 2g - 6p + 2b + 2x + e_g$$

23. Die Bestimmung der Zahl der Curven, bei de beiden, von dem Schnittpunkte einer Wendetangente mit e fectigen Geraden ausgehenden und von dieser Wendetange schiedenen Tangenten zusammenfallen, giebt:

$$2f + 6g - 6p + 6b + e_g$$

Wie die 23 Symbole e von dem oben definierten Anmerkung ungen abhängen, mögen die folgenden drei Beispiele vorle

caractéristiques des systèmes de coniques.

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groupes de termes de ϕ , et je retiens ceux pour lesquels a , acquiert sa valeur minima a . Je dis que le minimum de a , est l'ordre infimalité de ϕ .

Cela est évident s'il n'y a qu'un seul terme dont l'ordre soit égal à ce minimum a . S'il y a plusieurs termes, on doit prouver que la somme de leurs parties principales s'est pas nulle. On obtient ces parties principales en prenant pour les points e , g leurs positions limites répondant au point h , et pour les p leurs parties principales. Après suppression d'un facteur commun égal à l'indéfiniment petit principal élevé à la puissance a , il reste une fonction linéaire des coefficients de quelques termes de ϕ , et les coefficients de cette fonction ne sont pas nuls. Donc, en raisonnant comme précédemment ($n^o 22$), on voit que cette fonction ne peut être identiquement nulle, les coordonnées a , e , r , w étant des quantités linéaires. Donc a est l'ordre infimalité de ϕ .

Je pose

$$l = \min. de (nr + mr').$$

D'après (32), l'ordre de ϕ est $(pr + m3 + l)$. Je considère maintenant tous les points h analogues. Je désigne par l , la somme des nombres analogues à l et, comme aux $n^o 9$ et 10, par M et P les sommes des nombres m et p . J'ai ainsi pour le nombre total des zéros envisagés

$$(34) \quad B = eP + 3M + l.$$

On peut dire que les zéros de Ψ , indépendants du triangle uvw , forment deux groupes : les uns dépendent du mode de représentation du système, et leur nombre est eP ; les autres répondent aux coniques dégénérées, et leur nombre est $3M + l$.

24. Je m'occupe maintenant des autres zéros de la fonction Ψ , de ceux qui dépendent de a , e , r , w . Ils répondent tous à des coniques propres, pour chacune desquelles les a ne sont pas nuls. Ils sont donc indépendants du mode de représentation du système et correspondent à des coniques

1220. Galm.

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satisfaisait à la condition ϕ . Je vais démontrer que chacun d'eux est un zéro simple. Cela établi, il sera prouvé que le nombre de ces zéros est celui des coniques propres du système satisfaisant à la condition ϕ . Je déterminerais ensuite le nombre de ces zéros indirectement, comme la différence du nombre des infinis de la fonction et du nombre des zéros précédemment envisagés.

25. Les zéros dont il s'agit dépendent de a , e , r , w ; aucun d'eux ne correspond donc ni à un point multiple de (β) , ni à une conique stationnaire de (α) ($n^o 7$). Par suite, le point h auquel correspond un quelconque de ces zéros est un point simple et, en ce point, les dérivées des quantités a par rapport à l'axe de (β) ne sont pas nulles. J'emploie des accents pour marquer ces dérivées, et j'ai à prouver que la quantité

$$f = \sum_{i=1}^n \frac{\partial \Psi}{\partial a_i} a_i$$

n'est pas nulle au point h .

Fixons à volonté les points e et w , et attribuons a à parcourir une droite arbitraire, en sorte qu'il ne reste plus, de ce fait, qu'une variable que je continue à désigner par a . Faisons correspondre les coniques du système et les points a de manière que l'on ait constamment $\Psi = 0$. Il en résulte

$$(35) \quad f + \frac{\partial \Psi}{\partial a} a' = 0.$$

Si f est nul, l'une des quantités a' ou $\frac{\partial \Psi}{\partial a} a'$ l'est aussi. La seconde hypothèse conduisant à l'une ou à l'autre de ces deux conséquences : ou bien $\frac{\partial \Psi}{\partial a} a'$ est nulle identiquement ou bien cette quantité est nulle en vertu de $\Psi = 0$. Suivant la première, Ψ ne contient pas a . S'il en est ainsi, le raisonnement se répète sur e ou sur w , cette fois sans que cette conséquence soit possible, puisque Ψ contient au moins un des trois points. Suivant la seconde, Ψ est divisible par le carré d'une fonction entière de a ; par suite, la con-

[Schubert, 1879b]

[Halphen, 1878b]

clearly outlined notations, methods, and open problems for others to take up, no unified disciplinary matrix would structure a collective of enumerative geometers until the theory was incorporated into algebraic geometry in universities. In fact, this would only happen well into the 20th century⁴. The mismatch in mathematical practices appears most clearly when comparing any two pages borrowed from Halphen's memoirs and Schubert's *Kalkül*, so much that the sameness of the mathematical questions, results, and concepts at hand can be questioned⁵.

There seems to be, however, a much more glaring and pressing discrepancy between these theories that we need to address. Indeed, the very validity of Chasles' $\alpha\mu + \beta\nu$ formula remains undecided. Already in the controversy between Chasles and De Jonquières, presented in chapter 5, doubts were cast over the fact that this formula adequately solved the problem of the enumeration of conics, and the question was left unanswered as to why it was to be preferred to De Jonquières' simpler $\alpha\mu$ formula. De Jonquières claimed that his formula derived from perfect algebraic reasoning, and thus, was imbued with the corresponding kind of exactness, which he distinguished from Chasles' geometrical *esprit de vérification*. However, De Jonquières' formula quickly ceased to be discussed by geometers (in part because, on a technical level, it fails to abide by the principle of duality, and leads to many enumerations which are hard to accommodate with a general enumerative theory, but perhaps also for more contingent reasons, such as De Jonquières' military duties which prevented him from conducting research and publishing mathematical papers between 1866 and 1878). By contrast, Chasles' formula was, as we showed in chapter 6,

⁴The history of enumerative geometry as a scholarly discipline remains largely to be written. We shall give a few elements of this history in this chapter, but no systematic study thereof has been conducted.

⁵On the difficult historiographical question of the sameness of mathematical theorems across time and space, see [Goldstein, 1995].

the object of many proof attempts, and most importantly of Halphen's refutation.

And yet, despite Halphen's counter-examples and analytical explanation of the conditions of validity of Chasles' formula, the latter would remain central to Schubert's enumerative calculus. Indeed, Chasles' $\alpha\mu + \beta\nu$ formula was, alongside Bézout's theorem, one of the paradigmatic (and, historically, one of the first) examples of this kind of result which Schubert identified as a "Produktensatz", that is to say the kind of result to which the enumerative theory of a certain figure, properly conducted, ought to lead. But more profoundly, a symbolic play on the very expression $\alpha\mu + \beta\nu$ was the key factor that elicited the creation of Schubert's symbolism of conditions. Remember that, as we explained in section 7.1, Schubert had read a brief paper by Halphen as showing that, for five conditions of respective modules $\alpha_i\mu + \beta_i\nu$, the symbolical product

$$(\alpha_1\mu + \beta_1\nu) \times \dots \times (\alpha_5\mu + \beta_5\nu)$$

yielded the number of conics satisfying all five conditions, provided that all symbols $\mu^i\nu^{5-i}$ (that is to say, all symbols not involving any of the α_i 's or β_i 's) be interpreted as the numbers of conics satisfying the elementary conditions 'passing through a given point' (resp. 'touching a given straight line') i (resp. $5 - i$) times. Schubert went on to unify this symbolism by letting μ and ν denote said elementary conditions, and by introducing products of condition-symbols as their conjunctions. In every use of Schubert's symbolic calculus on conditions for conics, therefore, lies a hidden hypothesis. This hypothesis is weaker than that of the validity of Chasles' formula itself; it rather consists in the fact that all conditions involved in a specific symbolic computation have modules of the form $\alpha\mu + \beta\nu$. More largely, the validity of Chasles' formula is, strictly and logically speaking, not a hypothesis upon which Schubert's calculus hinges. Nonetheless, it appears clearly from its very architecture, and the purposes it is geared towards, that Schubert's *abzählende Geometrie* (at least in its original form) would lose much of its scientific value, were we to accept Halphen's claim that Chasles' formula is incorrect.

One thing must be made clear: it was not out of ignorance of Halphen's counter-examples that Schubert maintained the validity of Chasles' formula. In fact, the 1876 proof of said formula written by Schubert and Hurwitz explicitly mentions Halphen's counter-examples, and is presented as a response to them. Schubert even wrote a review of Halphen's memoirs for the *Jahrbuch* of the year 1878 (published in 1880), which he concluded with the following words⁶:

⁶"Therefore, there seem to be now in the theory of characteristics two possible directions for research. In the first one, the complete consideration of all systems and all conditions is essential. Then, one has to sacrifice the simplicity of representation of the number being sought, which is provided by the analogy with Bézout's theorem. In the second one, one sacrifices some systems for which the formulae become invalid, and which naturally would have to be precisely characterized; but one preserves the

Demnach scheinen jetzt in der Charakteristikentheorie zwei Untersuchungsrichtungen möglich zu sein. Bei der ersten ist die vollständige Berücksichtigung aller Systeme und aller Bedingungen wesentlich. Dann hat man die Einfachheit der Darstellung der gesuchten Zahl nach der Analogie des Bezout'schen Satzes zu opfern. Bei der zweiten opfert man einige Systeme, für welche die Formeln ungültig werden, und welche natürlich genau charakterisirt werden müssten; man bewahrt sich aber die einfache Darstellung der gesuchten Zahl als Summe von Producten. Nach des Referenten Ansicht sind beide Untersuchungsrichtungen theoretisch gleich berechtigt.

In later publications, such as the 1879 *Kalkül* and the 1880 paper on triangles, Schubert would also acknowledge the existence of Halphen's critique, and put the general limitation on the formulae derived in these texts that they must only be applied to systems containing no "Halphenesque degenerations"⁷. Schubert would not discuss how one is to know a priori whether a given system does or does not contain such degenerations, or even what their presence does to these formulae; despite having acknowledged the importance of this task in the review quoted above. Furthermore, whilst Schubert made such acknowledgements in passing in several texts, it is safe to say that Halphen's degenerations had no impact on his mathematical practice. These degenerations are never attributed symbols, nor do they affect Schubert's symbolic calculus in any way. Meanwhile, after the publication of his memoirs on the systems of conics, Halphen made very few public comments on Schubert's calculus or on Chasles' formula more generally, one notable exception being a brief note for the Société Mathématique de France on the enumerative geometry of the triangle. Beneath these timid public exchanges, however, Halphen and Schubert had a much more incisive dispute through an epistolary exchange, to which Zeuthen largely contributed, writing to and receiving letters from both at the same time⁸. We shall return to these public and private exchanges in section 8.2.

This undecidedness surrounding a formula at the heart of a well-known mathematical theory did not go unnoticed. By the late 1870s, the German mathematician Felix Klein was actively corresponding with Schubert, albeit mostly on different topics⁹. Their correspondence indicates that while he did not work on this question himself, Klein was

simple representation of the number being sought as a sum of products. According to the reviewer, both directions are theoretically equally justified", [Schubert, 1880c], p.430.

⁷Schubert's term is "Halphen'sche Ausartung", see [Schubert, 1880a], p.158. In the *Kalkül*, Schubert merely says Halphen's "doubts" (*Zweifel*) concerning Chasles' formula were published too late to be integrated to the book, see [Schubert, 1879b], p.344.

⁸Unfortunately, the sole extant documents are the letters received by Halphen from both Schubert and Zeuthen, except for a few excerpts of letters by Halphen to Zeuthen, which the latter communicated to Poincaré when the collected works of Halphen were being edited.

⁹Schubert's letters to Klein are preserved in the Universitätsbibliothek Göttingen; Cod. Ms. F. Klein 11, in particular Briefe n°782-795. I thank François Lê for drawing my attention to these sources.

aware of the lingering disagreement between Schubert and Halphen. Klein was not yet the influential professor and powerful organizer of mathematical research which he would become at Göttingen¹⁰; nonetheless, he already had access to a pool of promising students. In 1884, while still a professor at Leipzig¹¹, Klein steered one such student toward the study of enumerative geometry, and in particular the validity of Chasles' formula: this student was Eduard Study¹² (1862-1930). Despite his initial reluctance to work on this problem, Study obtained a new proof for Chasles' formula, and attempted to fully respond to Halphen's criticism thereof¹³. Crucially, Study put forth a new kind of argument: Halphen's counter-example did not refute Chasles' formula per se, but only one interpretation thereof – and not necessarily the most appropriate one. Shortly after the publication of Study's dissertation, Klein received a rather bitter letter by Zeuthen, by then an internationally recognized expert of enumerative methods¹⁴. In this letter, Zeuthen lamented Study's reluctance to discuss the matter with him, and rejected the claim that the problem had been solved once and for all. Halphen's untimely death in 1889 meant that his potential responses to Study's work were never formulated – and the meeting in Paris the two mathematicians had in 1886 had been rather unproductive¹⁵.

In 1890, therefore, with no end in sight to these disputes, Klein took it upon himself to write to Zeuthen once more to request a public and official response. Zeuthen replied with a letter which was immediately published in the *Mathematische Annalen*, which Klein then edited¹⁶. In this letter, Zeuthen reiterated his opinion that Study's work was based on a misunderstanding of the very problem Halphen had set out to solve, and that Halphen's theory of conics, as well as his refutation of Chasles' formula, still held. Study publicly rebuked Zeuthen's letter in *Mathematische Annalen* some two years later, only to trigger another attack by Zeuthen¹⁷ in the very same journal. This controversy in

¹⁰ [Rowe, 1989].

¹¹Klein was a professor in Leipzig between 1880 and 1886. There, he led an influential seminar, to which participated Adolf Hurwitz, Walter Dyck, or even Giuseppe Veronese, and where scientific exchange, in particular between France and Germany, was promoted; see [Rowe, 2018], pp.120-124. The presence at this seminar of Hurwitz, who had already studied under Klein in München, and for whom Klein would develop a great esteem ([Rowe, 2018], pp.172-175), may have also played a role in stimulating Klein's interest for enumerative problems.

¹²On Eduard Study's life, see [Hartwich, 2005], in particular chapters 3 and 5 for Study's dissertation under Klein and the later discussion with Zeuthen on related questions.

¹³This dissertation was defended on October 27th 1885 and printed by Teubner ([Study, 1885]), only to be immediately re-published in *Mathematische Annalen* via two papers ([Study, 1886b], [Study, 1886a]). There are only very slight changes between both versions, which we will discuss in the third section of this chapter.

¹⁴While we shall not insist on it in this chapter, the rise of international collaboration in mathematics, to which Klein and Hilbert were important actors, is an important factor for the history of enumerative geometry in the wake of these initial controversies.

¹⁵Study visited Paris alongside Hilbert, as part of a trip documented in [Reid, 1986], pp.20-27.

¹⁶ [Zeuthen, 1890].

¹⁷ [Study, 1892b], [Zeuthen, 1893].

the open was abruptly (and unsatisfactorily) cut short by the editorial board (and in particular Klein), as Lange explains, in his review of this exchange for the *Jahrbuch*¹⁸:

Nachdem wir so die Streitfrage erläutert haben, meinen wir, die Worte wiederholen zu sollen, durch welche die Redaction der Mathematischen Annalen die Discussion schloss: “Nachdem in der zwischen den Herren Study und Zeuthen schwebenden Streitfrage beide Autoren ihre Ansicht ausführlich dargelegt haben, kann die Redaction der Mathematischen Annalen von ihrem Standpunkte aus die Discussion um so mehr als abgeschlossen ansehen, da die beiderseitigen Ansichten in sachlicher Hinsicht nicht mehr so sehr differiren, — hat doch Hr. Study die Correctheit sämtlicher Entwicklungen von Halphen ausdrücklich zugestanden und andererseits Hr. Zeuthen wiederholt Study’s eigenen Standpunkt als einen möglichen anerkannt”.

How exactly are we to understand this coexistence of two ‘standpoints’ on the question of the validity of a formula? Klein’s editorial authority may have put an end to these debates for a while, but the epistemic unrest, which had begun with the controversies between Chasles and De Jonquières, Halphen and Schubert, and now Zeuthen and Study, was still very much alive. In fact, both Zeuthen’s and Study’s last contributions to this polemic were very much arguing for considerably more than the merits of their specific standpoints. Study, in 1892, concluded by asserting that Zeuthen “had succeeded neither in defending Halphen nor in attacking Study¹⁹”; while Zeuthen, in his 1893 response, maintained that “Study, who has saved the answer [that is to say, Chasles’ formula], must have altered the question²⁰”.

At the turn of the 20th century, a consensus would form in favor of the presentation given in texts such as Zeuthen’s entry on “enumerative methods” for Klein’s and Meyer’s *Encyklopädie*²¹, or in Corrado Segre’s teaching at the University of Torino²². For these

¹⁸“Having thus explained the question in dispute, we believe that we should repeat the words with which the editorial team of *Mathematische Annalen* closed the discussion: ‘Both authors having explained their views in detail on the question in dispute between MM. Study and Zeuthen, the editorial team of *Mathematische Annalen* can, from their point of view, consider the discussion all the more as closed, as the respective views of both sides no longer differ so much from a factual point of view, - since Mr. Study has expressly confirmed the correctness of all the developments of Halphen, and, on the other hand, Mr. Zeuthen repeatedly acknowledged that Study’s own position was possible”, [Lange, 1895], p.627. This is more or less a quotation of the footnote (in German) added by the editorial board at the end of the last letter (in French) written by Zeuthen for the journal. The reviewer, Ernst Lange, wrote a doctoral dissertation on quadrics under the supervision of Felix Klein (and Wilhelm Scheibner) in Leipzig in 1882.

¹⁹“Weder die Vertheidigung Halphens noch der Angriff gegen mich ist ihm gelungen”, [Study, 1892b], p.562.

²⁰“M. Study, qui a sauvé la réponse, doit avoir modifié la question”, [Zeuthen, 1893], p.542.

²¹ [Zeuthen, 1905], pp.291-293; 303-304. See also Pieri’s translation and alteration of the aforementioned paper, [Pieri, 1915], pp.305-306; 319-322.

²²See [Segre, 1890], pp.238-239.

geometers, Chasles' formula was incorrect, as it was limited by the frequent presence of Halphen's third degeneration for conics. The only (valid) proofs for this formula, such as Hurwitz's or Study's, were then thought to necessarily rely on the hidden assumption that they operate on systems free of such degenerations. However, over this period, the issues perceived by enumerative geometers to be the most crucial to address gradually shifted from Chasles' formula (and theory of characteristics) to the more fundamental principles constructed in its wake, such as that of the conservation of number, or that of correspondence, which also had been the subject of several disputes and reinterpretations. The perceived unrest pervading the budding field of enumerative geometry was such that Hilbert thought useful and necessary to include it into his well-known list of 23 problems presented in Paris before the 1900 International Congress of Mathematicians²³.

In a surprising turn of events, the series of papers which are usually considered to have first solved (at least partially) Hilbert's fifteenth problem would turn Chasles' $\alpha\mu + \beta\nu$ formula into a rather simple consequence of a new algebraico-topological framework for enumerative geometry at large. These are the papers published in the late 1920s by the Dutch geometer Baartel Van der Waerden, and the well-known series of papers called *Zur Algebraische Geometrie* which he published in *Mathematische Annalen* some ten years later²⁴. Even with the academic success met by Van der Waerden's foundational work, and its lasting influence over the concepts, methods, and axiomatic frameworks in use in algebraic geometry over the decades following his publications, and consequently the stabilization of the truth-value of Chasles' formula throughout the second half of the 20th century (coupled with a sharp decrease of its centrality to enumerative geometry), dissenting voices would still be heard again at times²⁵.

Modern-day mathematicians, following in the footsteps of Van der Waerden and many others, are well-equipped to understand the many resurgences of discussions over Chasles' $\alpha\mu + \beta\nu$ formula²⁶. In a nutshell, the profound problem underpinning these debates is that there are several ways to construct a space of conics in which to compute enumerative numbers. Starting from the variety of smooth conics L (which is defined unambiguously), the enumerative problem at hand here is to compute the number of intersections of various sub-varieties of L whose dimensions add up to that of L itself. To compute this number using the techniques of modern algebraic geometry, however, one must add boundary points to this base variety, so that we obtain a variety that is both projective and compact. This operation, called *compactification*, is neither unique nor trivial: as Eisenbud and

²³ [Gray, 2000], p.178.

²⁴ [Schappacher, 2007].

²⁵ Most notably, see [Casas-Alvero & Xambò-Descamps, 1986].

²⁶ The mathematical exposition in this paragraph is borrowed from [Casas-Alvero & Xambò-Descamps, 1986] and [Kleiman, 1976a]. I am also indebted to Patrick Popescu-Pampu for his help with the mathematical details in this section.

Harris put it in their course on algebraic geometry²⁷:

If we are lucky, the boundary points of the compactification still parametrize some sort of geometric object we understand. In such cases we can use this structure to solve geometric problems. But as we shall see, the boundary can also get in the way, even when it seems quite natural. In such cases, we might look for a “better” compactification... but just how to do this is a matter of art rather than of science.

Even in the case of the triangle, whose enumerative geometry we discussed in the previous chapter (see 7.4), there are several ways in which one can add infinitely small triangles to the variety of proper triangles (or, in the words of J. G. Semple, to ‘desingularize’ the domain of this figure, and to define a ‘variable triangle’ on which one can carry out enumerative procedures). Depending on this choice, the truth-value of Schubert’s *Produktsatz* for triangles discussed previously can vary. And, as Eisenbud and Harris pointedly noted, it always remains an open, extra-mathematical question to decide whether or not the chosen compactification yields results which are true to the original, geometrical intuition underlying the enumerative problem at hand.

Without going into too much detail here, one can still offer a potent ‘retrospective diagnosis’²⁸ of the debates surrounding Chasles’ $\alpha\mu + \beta\nu$ formula by saying that Study’s proof thereof operates on a certain variety, which Van der Waerden later identified and named ‘the variety of complete conics’ (a term still in use today). This variety can be defined as the closure W_1 in $\mathbb{P}_5 \times \mathbb{P}_5^*$ of the graph W_0 of the map $\alpha : \mathbb{P}_5 - \Delta \longrightarrow \mathbb{P}_5^* - \Delta^*$, where Δ represents the set of degenerate conics, and where α maps a (non-degenerate) conic locus to its conic envelope. W_0 can be interpreted as the (open) set of all non-degenerate (complete) conics. Halphen’s three modes of degeneration A, A^*, B for conics can be defined in the variety of complete conics; so that $W_0 = W_1 - (A \cup A^*)$ (as A and A^* are algebraic hypersurfaces of $\mathbb{P}_5 \times \mathbb{P}_5^*$, which meet transversally along B). However, this framework is not enough to form a counter-example to Chasles’ formula. Halphen’s next step in refuting said formula was, in this retrospective diagnosis, to demand that a solution (that is to say a complete conic C) to an enumerative problem X only be accepted if there exists some homography σ (that is to say a linear projective transformation of \mathbb{P}_2) such that C does not satisfy $\sigma(X)$. For the sake of simplicity, here, we can think of X as an algebraic equation $R(C, F) = 0$, invariant under projection, where C is the variable conic and F some given elements of the plane; as to define properly an enumerative problem would take us too far. For instance, if K is a contact problem, a solution C common to all

²⁷ [Eisenbud & Harris, 2016], p.290.

²⁸We borrow this term from the history of medicine, where it refers to “the employment of modern biomedical categories as a perfectly valid way to talk about disease in the past”; [Foxhall, 2014], p.356.

enumerative problems in the orbit of K is improper, because it has the required contacts regardless of the actual position of the given objects which it ought to touch. According to this diagnosis, Halphen's enumerative geometry is obtained by further restricting the space of solutions to an enumerative problem²⁹, and his memoirs successfully showed that such a restriction necessarily negates Chasles' formula.

Our purpose going forward will not be to assess the relative merits and drawbacks of this contemporary explanation of past debates, something which historians have rightly warned against for quite some time now. However, this detour has shown two things. The first is that the mathematical disagreement over Chasles' formula was a profound one; and one in which the technical aspects of the mathematics of the historical actors involved did limit what could or could not be said about the validity of this formula. Without the insights of early 20th-century topology and structural algebra, the distinctions required to carefully delineate the conditions of validity of Chasles' formula seem nigh impossible to clarify. Perhaps more important, however, is the fact that the very disciplinary identities of geometry embodied by the various collectives of mathematicians involved in the late 19th-century debates over Chasles' formula structured said debates in ways which made this contemporary (dis)solution of the problem unreachable. This is where lies the second import of this brief presentation of contemporary solutions: it emphasizes the profound transformation undergone by mathematical life at the turn of the century.

For Van der Waerden, these disputes of the past between Zeuthen and Study over Chasles' formula had little mathematical value, and bore merely on "the honor of Halphen's memory"³⁰. Besides its technical content, Van der Waerden's paper on the theory of characteristics brought new epistemic norms, a new understanding of the rules and purpose of mathematical activity, which made the many arguments of the aforementioned historical actors devoid of any interest. In fact, Van der Waerden could as well have agreed with the editors of *Mathematische Annalen* and declared Study's and Zeuthen's standpoints to be equally valid. The mathematical truth of Chasles' formula, for Van der Waerden, hinges crucially upon a certain arbitrary choice, namely that of a base variety, only after which the very question to which Chasles' formula is supposed to answer can be properly defined

²⁹In technical terms, Halphen blew up the variety of complete conics along the subvariety B . To blow up a variety X along a subvariety Y , here, means to construct a particular regular birational map $\pi : X^* \rightarrow X$ so that π is an isomorphism away from Y , and $\pi^{-1}(Y)$ is a hypersurface of X^* ; see [Harris, 1992], pp.80-86. In informal terms, π is obtained by replacing the subvariety Y by the tangent directions jutting outward from it. Typically, this is used in modern algebraic geometry to resolve the singularities of an algebraic variety defined over an algebraically closed field of characteristic zero, by blowing up iterably suitable smooth subvarieties singular loci; see [Kollár, 2007], ch.1; and [Fulton, 2008], pp.82-87.

³⁰"Auf die scharfe Polemik zwischen Study und Zeuthen, die sich an die hier geschilderte Study'sche Auffassung geknüpft hat, brauchen wir hier nicht einzugehen, da sich weniger die Sache selbst betraf als die Frage nach dem Werte anderer Untersuchungen von Halphen und die Ehre seines Andenkens", [Van der Waerden, 1938], p.646.

– so much that even elementary concepts such as that of a “number of solutions” (that is to say, number of intersections of systems of complementary dimensions) are given a formal definition through his general framework for algebraic geometry.

This is not to say that Halphen, Schubert, Zeuthen, or Study failed to acknowledge that the mathematical truth of a proposition depends on the proper definition and delineation of the concepts involved in its expression. On the contrary, this final chapter shows that the disputes surrounding Chasles’ formula between 1876 and 1893, that is to say between the publication of Halphen’s counter-examples and Zeuthen’s final letter to Klein and Study, were as much a technical mathematical exchange as a debate about the very identity of mathematics as a discipline and cultural practice. As such, I argue, they encapsulated epistemic tensions and divides at the very core of what has been described in recent decades as the ‘modernist transformation of mathematics’ at the turn of the 20th century.

8.1.2 Mehrten’s thesis and the notion of mathematical modernism

In his 1990 landmark study *Moderne–Sprache–Mathematik*, German historian Herbert Mehrtens argued that mathematics as a scientific discipline and as a cultural practice underwent a profound transformation in the last decades of the 19th century. This transformation was not merely a fast accumulation of theorems, concepts, or proofs, but a transformation of the very rules of mathematical activity – be them semiotic³¹, epistemic, or collective rules. Arguing against a more traditional narrative focusing on the so-called *Grundlagenkrise*, that is to say the rise of competing foundational programmes³² which arose in the wake of the discovery of logical paradoxes (such as Russell’s paradox which undermined Frege’s logical analysis of numbers), Mehrtens proposed to view this historical episode as “the upheaval of the concepts of truth, meaning, object, and existence in mathematics”. In other words, for Mehrtens, foundational disputes were but one facet on a more radical change in modes of objectivity, regimes of truth, semiotic practices, and the very cultural identity of mathematics. This latter identity went under particular strain as the idea arose that mathematics was a “pure language of strictly regulated denominations”, that is to say a discourse which is about nothing other than itself³³. This change,

³¹Mehrtens is adopting in this book a Foucauldian approach, especially in his treatment of mathematics as a discursive formation, which we shall not attempt to summarize here, in the interest of brevity. On the influence of Foucault on the historiography and philosophy of mathematics, see [Rabouin, 2015].

³²Especially those known as the “Big Three”, namely logicism, intuitionism, and formalism. See [Shapiro, 2000], pp.107-200.

³³“[Ich] gebe eine neue Interpretation der sogenannten ‘Grundlagenkrise’ der Mathematik, die nicht Krise der ‘Grundlagen’ war, sondern die Erschütterung der Begriffe von Wahrheit, Sinn, Gegenstand, Existenz in der Mathematik. Diese Begriffe bezeichnen weniger epistemologische Grundfesten der Wis-

Mehrtens argue, brought about another in the politics of (mathematical) discourse, and in the relations of the mathematicians to their textual output and its meaning.

The ideal-type of modern mathematician, for Mehrtens, was best described as a “free mathematician” and as a “creator³⁴”; that is a mathematician who has free reign over the domain of their discourse, and whose professional identity is defined by its autonomy within the bounds of a certain language. Examples of such ‘modern mathematicians’ include George Cantor and David Hilbert, to quote but a few. The semiotics of Hilbert’s famous *Grundlagen der Geometrie* illustrated Mehrtens’ characterization of the modernist turn: in it, symbols are used with no a-priori reference to objects existing outside the discourse (whether it be physical objects, a model of some phenomenal field, or even abstract objects conceived prior to the utterance of mathematical speech). Furthermore, to this semiotic practice is tied a specific understanding of the mathematician’s task, laid out explicitly by Hilbert in this same book: “to bring out as clearly as possible the meaning of the different groups of axioms and the scope of the conclusions to be derived from the individual axioms³⁵”.

The emergence of this ideal-type was not that of an hegemonic model for mathematical activity: in fact, a key feature of Mehrtens’ narrative is that the so-called ‘moderns’ are always defined by contrast with another emerging ideal-type, namely that of the ‘counter-moderns’ (*Gegenmoderne*) (such as Felix Klein or Henri Poincaré). Counter-moderns, according to Mehrtens, pushed back against the perceived dangers of the ‘arbitrary will of the creator³⁶’. Counter-moderns wanted to secure epistemic stability by inscribing mathematical concepts and truth in an external system of references, for instance relying on ‘intuition’ (*Anschauung*) or conventions born from experience. This is not to say that mathematics lost its autonomy, and they certainly did not advocate for experimental methods in mathematics, but rather that the meaning of mathematical discourse (and therefore its certainty) derived from such extra-textual realm³⁷. Moderns, of course,

senschaft als die großen Orientierungsmarken, an denen sich das Selbstverständnis einer Wissenschaft, ihre Identität bestimmt. [...] Der Diskurs der Mathematik um die reine Sprache strikt geregelten Bezeichnens als Teil kultureller Produktion und die in ihm hergestellten Identitäten bedürfen einer theoretischen Erörterung”, [Mehrtens, 1990], p.8.

³⁴“Der Meister der Moderne dagegen bestimmt sich als ‘freier Mathematiker’, als ‘Schöpfer’”, [Mehrtens, 1990], p.10.

³⁵“Die vorliegende Untersuchung ist ein neuer Versuch, für die Geometrie ein vollständiges und möglichst einfaches System von Axiomen aufzustellen und aus denselben die wichtigsten geometrischen Sätze in der Weise abzuleiten, daß dabei die Bedeutung der verschiedenen Axiomgruppen und die Tragweite der aus den einzelnen Axiomen zu ziehenden Folgerungen möglichst klar zu Tage tritt”, [Hilbert, 1899], p.1. For a more careful interpretation of Hilbert’s understanding of the role of axioms, which does not fully fit with Mehrtens’ categories, see [Detlefsen, 1986].

³⁶“Die Gegenmoderne wirft [dem Moderne] ‘Schöpferwillkür’”, [Mehrtens, 1990], p.10.

³⁷“Die andere Variante Gegenmoderne [...] bemüht die ‘Gabe’ der ‘Anschauung’. [...] Eine Mathematik, die ‘in der Anschauung wurzelt’, muss ihrer ‘Natur’ gemäss für sie Sinn haben. [...] Es geht nicht um nützliche oder anwendbare Mathematik, nicht um konkrete Zusammenarbeit mit Technikern oder

refused such saving grace external to the mathematical discourse itself. Counter-moderns, in Mehrtens' narrative, are not viewed as resisting some ineluctable changes, but rather actively constructing a counter-model, another possible mathematical discipline in the wake of a radical upheaval of its norms and objects. Such a counter-model is illustrated for instance by Poincaré's well-known review of Hilbert's aforementioned opus, which ended with the following words³⁸:

Le point de vue logique paraît seul l'intéresser. Étant donnée une suite de propositions, il constate que toutes se déduisent logiquement de la première. Quel est le fondement de cette première proposition, quelle en est l'origine psychologique, il ne s'en occupe pas. [...] Son œuvre est donc incomplète; mais ce n'est pas une critique que je lui adresse. Incomplet, il faut bien se résigner à l'être.

Mehrtens draws the portraits of two models for the cultural status of the mathematician, whose clash was at the heart of what he described as the conflicted rise of modern mathematics. As such, this transformation was rife with political implications, as the very nature of the social and political authority of mathematics, or even its relation to the real it had served to model for decades, were under strain.

8.1.3 At the junction of cultural and technical histories of mathematics

In the years following the publication of *Moderne-Sprache-Mathematik*, the concept of mathematical modernism has been adopted and adapted by various authors, of which we shall survey a few in what follows. The uptake of Mehrtens' categories, however, has more often than not been paired with a criticism thereof.

Moritz Epple, through a study of the re-emergence of the divide between analytic and synthetic geometry at the onset of the 20th century – in particular, in Klein's *Vorlesungen*

Naturwissenschaftlern, sondern um einen übergreifenden 'Sinn', der die Mathematik autonom lässt, aber den Zusammenhang sichert. Die Moderne verzichtet auf solche Sicherung durch einen eingeschriebenen Sinn und vertraut auf den funktionierenden Wissenschaftsbetrieb, in dem die Sprache Mathematik über Ausbildung und Anwendung schon ihren Weg zur Wirklichkeit finden wird", [Mehrtens, 1990], pp.13-14. In Mehrtens' account, these oppositions have overt political connotations, as the counter-moderns are presented as the forerunners of an epistemology of mathematics developed by proponents of the national-socialist ideology in the 1930's. This important aspect of Mehrtens' book is left out of this presentation, as it falls out of the scope of the present study.

³⁸"The logical point of view alone appears to interest him. Being given a sequence of propositions, he finds that all follow logically from the first. With the foundation of this first proposition, with its psychological origin, he does not concern himself [...] His work is then incomplete; but this is not a criticism which I make against him. Incomplete one must indeed resign one's self to be", [Poincaré, 1902], p.272, cited in [Gray, 2008], p.188.

über höhere Geometrie (1926) and Hilbert's *Grundlagen der Geometrie* –, has attempted to correlate Mehrtens' categories (and his description of the semiotics of the textual outputs of the moderns and the counter-moderns) to differences in mathematical styles³⁹. In order to do so, he mobilized the Wittgensteinian concept of *language-games*, seeking a characterization of modernist mathematics internal to the mathematics of historical actors, and not only to social-historical factors. A couple of years later, applying this mode of analysis to his research on the history of knot theory⁴⁰, Eppe put forth a strategy for the close study of actual research activities, criticizing Mehrtens' focus on meta-mathematical and programmatic texts.

More recently, Jeremy Gray has provided in his 2008 volume entitled *Plato's Ghost* an extensive survey of the mathematical and philosophical shifts of this period, covering a large array of subjects and authors⁴¹ (from which enumerative geometry is nonetheless absent). Drawing explicitly on Mehrtens' work, Gray nonetheless notes that it suffers from too exclusive a focus on Germany over the whole of Europe, and on programmatic or philosophical texts over actual mathematical practices. This makes Mehrtens' work useful, yet insufficient for informing a precise understanding of the multi-national, cross-cultural, decade-spanning modernist transformation of mathematics. Gathering decades of historical research (conducted by himself and many others) on the history of various mathematical disciplines over this period, Gray has then characterized modernist mathematics as “as an autonomous body of ideas, having little or no outward reference, [...] maintaining a complicated—indeed, anxious—rather than a naïve relationship with the day-to-day world⁴²”. The counter-moderns, according to Gray's analysis, would combat this anxiety by grounding mathematical truths into appeals to intuition, or some transcendent order. Gray's account sheds light on many previously unexplored facets of this transformation, such as changes in the way mathematics was popularized during this episode, or the interplay between the history of modern mathematics and psychology⁴³.

What this literature most importantly shows is that the changes described by Mehrtens are tied to the many profound transformations of the body of mathematical knowledge⁴⁴ itself: the reappraisal of non-Euclidean geometries (for instance by means of a budding

³⁹ [Eppe, 1997].

⁴⁰ [Eppe, 1999], especially pp.199-265.

⁴¹ [Gray, 2008].

⁴² [Gray, 2008], p.1. On anxiety and mathematical knowledge, see [Gray, 2004].

⁴³ [Gray, 2008], respectively pp.346-365; 388-405.

⁴⁴We borrow this term from [Corry, 2004], where the ‘body of knowledge’ is defined as including “statements that are answers to questions related to the subject matter of the discipline, [...], theories, facts, methods, open problems”, in opposition to the ‘image of knowledge’, which “includes claims which express knowledge about the discipline *qua* discipline [...], serves a guiding principles, or selectors”, p.3.

group theory)⁴⁵ or the development of set-theory⁴⁶ are but two examples of theories which not only played important and well-documented roles in the renewal of the epistemology of mathematics at the turn of century, but were integral part of a drastic expansion of mathematical knowledge, at a rate perceived as far greater than had ever been the case.

Another line of critique of the history of mathematical modernism, forcefully argued by Leo Corry⁴⁷, lies in the thesis that the worth of the concept of modernism in the historiography of mathematics also hinges upon its ability to incorporate broader, contemporary cultural changes, whilst still retaining a clear view of the specificity of mathematics as a cognitive system. The very name of this transformation suggests comparison to transformations in the arts, many of which have been tentatively put forth, with contrasted levels of success⁴⁸. According to Corry, it remains necessary to “[show] (if possible) that the processes that led to modernism in general and in mathematics are similar and have common cultural roots”.

Of course, Mehrtens had already attempted to tie his categories to broader cultural and scientific transformations. In his book, for instance, the fact that Freud’s psychoanalysis undermined the Self is put in parallel to the semiotic practice present in modern mathematical texts whereby objects appear from the text itself, without requiring a Self to perceive or conceive them⁴⁹; elsewhere, disenchanted mathematics after the First World War is compared to Schönberg’s atonal music on the grounds that “one of the main features of modern music is its detachment from the narrative character of its language”, and that “like [modern] mathematics, this music is turned towards its syntax⁵⁰”. However, Corry argues, these points of comparison are too coarsely sketched, and remain far removed from actual cultural or scientific practices, be it in the mathematics, the natural sciences, or the arts. Furthermore, while Gray’s more recent attempt at constructing a large-scale history of mathematical modernism remedies several of the shortcomings of Mehrtens’ book, and elaborates more precisely on certain common roots of mathematical

⁴⁵While Bolyai and Lobachevskii published their work on non-Euclidean geometry in as early as the 1830s, it only became a real topic for discussion among French mathematicians in the late 1860s, see [Voelke, 2005].

⁴⁶See [Ferreirós, 2007], where it is argued that the notion of set was not merely brought forth in order to secure rigorous grounding for analysis (let alone mathematics at large), but actually grew out of pressing, technical needs of algebraists, geometers, or arithmeticians well removed from foundational or philosophical concerns proper.

⁴⁷ [Corry, Forthcoming].

⁴⁸For instance, [Everdell, 1997], [Albright, 1997], [Engehardt, 2018]. None of these works, however, are the works of specialists in the history of mathematics, for which they rely mostly on somewhat outdated secondary literature.

⁴⁹ [Mehrtens, 1990], p.10. The paradigmatic example of this practice, for Mehrtens, is Hilbert’s *Grundlagen der Geometrie*, wherein the reference and meaning of geometrical terms such as points or lines arise from the discourse itself, and not from anterior intuitions of space on the part of the reader.

⁵⁰“Ein Zug der modernen Musik ist die Ablösung vom narrative Charakter ihre Sprache. [...] Diese Musik wendet sich wie die Mathematik, ihrer Syntax zu.”, [Mehrtens, 1990], p.561.

and cultural modernism (for instance in his study of the history of the psychology of mathematics), it suggests no systematic answer to the question of the cultural rooting of the modernist transformation of mathematics at large.

Our purpose in the rest of this chapter is of course not to venture such a systematic answer. More modestly, we set out to show that the controversies over the validity of Chasles' formula between Schubert, Halphen, Zeuthen, and Study can serve as an informative case-study to illustrate the notion that the shaping of mathematical modernism is not reducible to one all-encompassing transformation of human knowledge and art, but rather is a composite phenomenon, separable from neither cultural history at large, nor the technical history of mathematical knowledge⁵¹.

Indeed, these controversies span the decades and the locales most crucial to the conflicted emergence of modern mathematics⁵². Furthermore, they were never understood by their protagonists as being primarily a foundational or philosophical issue, but rather a discussion of technical, albeit important results. As such, the choice of this formula as a focal point for a new perspective on the modernist transformation of mathematics eschews the shortcomings of Mehrtens' approach identified in the paragraphs above.

More crucially, the actors involved in these disputes all embody very different cultural figures of the mathematician: they belong to different socio-professional categories, place their work under different epistemic ideals, and interacted with different cultural institutions. This was already made apparent in the two previous chapters. Halphen, we have shown, was a career military-man, for whom mathematics was a non-professional activity which nonetheless took up a lot of his time and resources, dominated by the military ideals of rigour, preparedness, and precision. Meanwhile, Schubert was a *Gymnasium* teacher, who in his mathematical practice accorded great importance to the crafting of notations and the simplicity of results.

Thus, this chapter sets out to show another way to tie the modernist transformation of mathematics to several late 19th-century cultural trends and ruptures, by framing the

⁵¹It might be useful to point here to what this chapter is not, namely a global, 'social history' of enumerative geometry, even on a limited time frame. Of our account were left out several authors which did engage to various degrees with the issues we discuss here, such as the Göttingen-educated geometer Heinrich Krey, who wrote several papers on closely related issues while a professor at the University of Freiburg (see [Krey, 1885]); or Mario Pieri, a student of Segre who occupied positions in the universities in Turin, Catania, and Parma (see [Marchisotto & Smith, 2007], p.376. A forthcoming volume in this series will shed more light on Pieri's contribution to enumerative geometry). A systematic (and perhaps quantitative) survey of the authors involved in the emergence of enumerative geometry as a discipline, for instance relying on databases such as that provided by the *Jahrbuch*, entirely remains to be undertaken. Our purpose in this chapter is much more local, and our interests more epistemological than sociological.

⁵²Modernism in mathematics is usually measured against the backdrop of a consensus-etalon in 1880, see [Gray, 2008], p.112. Beyond Paris and Göttingen, traditionnally the focus of these historical narratives, the circulation of Chasles' formula encompasses Hamburg, Copenhagen, Leipzig, and even later on would include major mathematical centers in Italy, Holland, and England.

debates over Chasles' formula as the confrontation of successive mathematical selves⁵³, that is to say epistemic ideals which can be found in the highly-normative descriptions of proper mathematical practice – past or present – produced by the key actors of the historical episode previously sketched. To each of these accounts are associated different epistemic virtues, as well as textual practices, which in turn give rise to different ontologies and regimes of truth. These are then all situated differently on the quadrants drawn by the two axes along which the modernist transformation of mathematics was structured by Gray, namely the absence of outward reference for mathematical discourse, and the growing anxiety amongst practitioners after the emergence of new standards of rigor. These selves, it will be shown, were all shaped against the decisive backdrop of various cultural trends and intellectual debates beyond mathematics. Conversely, we show how the mathematical practice – be it textual practice, identification of the epistemic tasks to fulfill, or choices in techniques of proof – of each of these authors is shaped and structured by said epistemic ideals. In so doing, we set out to construct a case-study for the modernist transformation of a specific subject at the junction of cultural and technical histories of mathematics.

In the rest of this chapter, we focus on two moments of the decade-long discussions over the validity of Chasles' formula. First, we present the epistolary exchange between Schubert, Halphen, and Zeuthen, and the controversy between Schubert and Halphen over the validity and generality of formulae such as $\alpha\mu + \beta\nu$. In a second section, we describe Study's dissertation with broad strokes, before turning to his public polemic with Zeuthen. In both cases, we identify and contrast epistemic ideals embodied by these authors and their texts, whereby a fresh outlook on the permanence of disagreement over the status of Chasles' formula can be gained.

⁵³Here, we borrow and adapt the notion of 'scientific selves' from [Daston & Galison, 2007], especially pp.35-50; 216-233; 367-371. The history of objectivity as envisioned in this book is not the history of conceptualizations and philosophical accounts of objectivity, but rather the history of the epistemic virtues that regulated and enabled said objectivity. Such virtues were not only preached, but also practiced and embodied by various means such as note-taking, self-erasing, attentive observation etc. Daston and Galison's ambition was to put forth a mesoscopic, *longue-durée* history of scientific objectivity across disciplinary borders. The scope of this chapter, by contrast, is resolutely micro-historical and local; what is imported from *Objectivity* is an analytical framework rather than a collection of ready-made 'scientific selves'. Nonetheless, the selves identified in this chapter are not to be equated with moral biographies: behind each of them lie broader characters, which for the purpose of brevity we locate only in certain individuals. For a similar 'local approach' to scientific selves, see [Paul, 2016].

8.2 Halphen versus Schubert: The (un)naturalness of mathematical concepts

8.2.1 The epistolary exchange between Halphen, Schubert, and Zeuthen

At the Paris Bibliothèque de l'Institut de France is preserved a rather large Halphen Nachlass, which includes drafts, notes, manuscripts, and letters from several dozens of mathematicians, from all over Europe, and on a wide variety of mathematical subjects⁵⁴. In what follows, we shall focus on the letters written by Schubert and Zeuthen, as they not only concern to a large extent Halphen's work on the $\alpha\mu + \beta\nu$ formula, but are the only correspondences to do so in a prolonged and detailed manner⁵⁵. Among the mathematicians who wrote to Halphen about this formula, we can also mention De Jonquières, Klein, Lindemann. All of these letters, with the remarkable exception of three letters by Schubert, are in the folder Ms 5624 (which gathers Halphen's correspondence⁵⁶). The three outliers are in the folder Ms 5621, which gathers scientific notes pertaining to Halphen's work on the theory of characteristics as well as the notes of the editorial committee in charge of publishing Halphen's collected works. Thus, it is possible that these three letters had been singled out by Poincaré (who was part of said editorial committee, alongside Camille Jordan and Émile Picard) for inclusion in Halphen's *Oeuvres*, but were ultimately left out. This is a reasonable guess because these three letters are the most substantial of Schubert's, and because some notes in Poincaré's hand, in the very same folder, indicate that an initial project was to include several of Halphen's letters to Zeuthen in the fourth volume of Halphen's *Oeuvres*. In the end, only a few excerpts were printed⁵⁷.

Zeuthen's correspondence with Halphen spans over a decade; including more than 30 letters from December 1875 to August 1886. At various points, this correspondence became very dense: 6 very long letters were written by Zeuthen during the month of November 1879 alone, for instance. By contrast, entire years go by without almost any letter, for instance the 10 months preceding Zeuthen's marriage in October 1879. While this correspondence revolves mostly around mathematical issues (and in particular, questions of algebraic geometry and the theory of characteristics), it also includes discussions of an epistemological nature (for instance on the relation between computing and reason-

⁵⁴I have been kindly granted access to several of Halphen's personal documents by his descendants. My special thanks go to Bernard, Maurice, and Hélène Lyon-Caen.

⁵⁵Several of these letters are reproduced in appendixes B and C.

⁵⁶The letters are all numbered. In what follows, we shall give the numbers of said letters in footnotes, as well as dates.

⁵⁷[Jordan *et al.*, 1924], pp.628-637. With two brief cards sent to Study in 1886, these are the only letters by Halphen we have had access to.

ing), exchanges of practical information (who published what and where, postal addresses and news of the professional lives of various mathematicians etc.), and more intimate and personal exchanges (such as Zeuthen's announcement of the birth of his first child in October 1880). Zeuthen wrote in French all throughout these discussions, a language in which he was rather confident, but with which he nonetheless needed help from Halphen in order to adequately translate certain terms from German or Danish. The tone of these letters becomes noticeably more cordial with time, and Zeuthen befriending Halphen, being at the receiving end of the latter's secret (and often negative) opinions regarding other mathematicians, or even just exchanging pleasantries.

Zeuthen's very first extant letter to Halphen⁵⁸, in fact, was a negative reply to the latter's request that the deadline for submitting an entry for the mathematical prize of the Danish Royal Academy be extended⁵⁹. As Zeuthen explained to Halphen, the commission for this prize had to wait first to see if any entry worthy of being awarded the prize had been submitted in time. Unfortunately for Halphen, such was the case; and Zeuthen, in his letter, described the work of an unnamed author who had made "very fortunate applications of what he calls 'Halphen's symbolic multiplication'", and regretted that Halphen had not been able to challenge for the prize on time⁶⁰. Of course, this unnamed author is none other than Schubert, who indeed went on to win a gold medal for his work on spatial cubics.

A few months later, Halphen asked Zeuthen to provide him with the name and address of this prize-winner⁶¹, and then began the correspondence between Halphen and Schubert.

⁵⁸In this letter, it is mentioned that Zeuthen had met Halphen in person in Paris in the past. It remains unclear whether previous letters had been exchanged between the two men; at any rate, none is preserved in the Bibliothèque de l'Institut.

⁵⁹The question to be answered had been published in January 1875; it bore on "the extension of the theory of characteristics to systems of geometrical figures composed by the points and osculating planes of skew cubics, and the determination of the characteristics of their elementary systems", [Zeuthen, 1877], p.169.

⁶⁰"Il fallait attendre premièrement si au terme déjà fixé il y avait des réponses à la questions [sic], et, lorsqu'il y en avait une, il fallait l'étudier et y appliquer beaucoup de preuves numériques pour voir si elle était digne de la prix [sic]. Elle s'en est montré très-digne, selon moi et je suppose que les autres membres du comité seront du même avis, et alors une prorogation est impossible. L'auteur fait des applications très-heureuses de ce qu'il appelle 'la multiplication symbolique de M. Halphen', et il vous cite aussi quant à la détermination des droites de l'espace dont il a besoin pour trouver les courbes singulières. Vous voyez donc que vous n'êtes pas absolument absent du concours; mais, certes, j'aurais préféré de beaucoup, si vous auriez pu nous faire l'honneur d'y prendre part d'une manière plus directe. La révision de deux mémoires, dont l'un pourrait servir à éclairer les points difficiles de l'autre, m'aurait été, peut-être, plus facile que celle d'une seule", Letter Ms 5624 218, Zeuthen to Halphen, dated December 12th 1875. Zeuthen, in the rest of this letter, evokes the loss by Halphen of his first child as what prevented him from writing an entry before the deadline.

⁶¹"Pour le moment je vous écris seulement à la hâte pour vous fournir l'adresse que vous demandez. M.Schubert vient d'obtenir une place à Hambourg en 'Oberlehrer' à un lycée. Il m'a indiqué l'adresse suivante: Baumeisterstr. 1, Hambourg, mais comme Hildesheim n'est pas une grande ville, il m'étonne qu'on n'en a pas envoyé votre lettre à Hambourg", Letter Ms 5624 219, Zeuthen to Halphen, dated May 13th 1876. Various geometrical diagrams (planes, rays..) are penciled in at the end of this letter, probably

In fact, Schubert's first letter to Halphen, dated May 18th 1876, is a reply to a lost letter by Halphen⁶². Overall, Schubert would write 15 letters and 3 postal cards to Halphen, between May 1876 and February 1880. Here again, this correspondence is concentrated over specific periods; in particular, 9 of these 18 documents were written between November 1879 and February 1880. Just as for Zeuthen's letters, this corresponds to the eruption of a disagreement between Schubert and Halphen, and of a technical dispute on the enumerative geometry of the triangle.

There ensued two intertwined epistolary exchanges: Halphen mentioned his exchanges with Schubert to Zeuthen at several points, either to vent frustration at what he perceived to be a failure to understand his mathematics on Schubert's part, or even to ask that Zeuthen intervene in their dispute. In parallel, Schubert lamented to Zeuthen (in letters we do not possess, but which Zeuthen mentions occasionally) the deterioration of his relationship with Halphen. From the evolution of this variable epistolary triangle, we shall present two focal moments, at which letters were exchanged at maximum frequency. The first of these two moments is the second half of the year 1876; the second one the period between November 1879 and February 1880.

Schubert's initial salvo of 6 letters to Halphen, from May 18th 1876 to December 4th 1876, served various purposes. A *Gymnasium Oberlehrer* in Hamburg, Schubert had a hard time accessing recently published mathematical journals and books, especially foreign ones. Schubert's perceived isolation had more to do with the lack of any established mathematical institutions in Hamburg than his position as a high-school teacher. Such positions were in fact rather desirable, for a variety of reasons⁶³. In July 1876, for instance, Schubert would explain to Halphen that he had recently turned down an offer to replace Ernst Schröder as a 'ordinary professor' at the *Polytechnikum* in Darmstadt, citing the low wages of university staff in Germany as the main factor for his decision⁶⁴. There would be no university in Hamburg until 1919, and therefore no scientific library where to freely

by Halphen.

⁶²Schubert writes to Halphen in German (despite being able to read French), albeit with a different handwriting (an uneven *Kursivschrift*) than that which he uses to communicate with other German mathematicians (that is to say, *Kurrentschrift*).

⁶³"[The profession of *Gymnasium Oberlehrer*] had been created in the early nineteenth century with the reform of Prussia's secondary school system. It had rapidly replaced the Lutheran pastorate as the principal threshold of social mobility into the so-called *Bildungsbürgertum*, that influential and self-conscious element of Germany's middle class that owed its social standing not to wealth and commerce, but to its quasi-monopoly of elite education and the access to the professions and to the civil service that followed. From the *Oberlehrer* threshold, upwardly mobile members of this middle class launched their sons into careers in law, medicine, the higher civil service, and the university professoriate", in [Turner, 1994], p.35.

⁶⁴"Vor einigen Wochen hatte ich einen Ruf als ord. Prof an das Polytechn. zu Därmstadt an Stelle von E.Schröder erhalten, ihn aber wegen der Kargheit des dortigen Budgets ablehnen müssen. Die Professoren in Deutschland werden noch gar zu schlecht bezahlt, mit wenigen Ausnahmen", Letter Ms 5624 167, Schubert to Halphen, dated July 12th 1876

access such documents. The ‘Mathematische Gesellschaft in Hamburg’, despite being one of the oldest in all of Europe, was at a low ebb. In fact, Schubert would play an active role in its revival a few years later, leading a commission investigating the collection of mathematical works in the local municipal library, pushing for the creation of a journal attached to the society⁶⁵, and actively publishing in it, until personal differences with other members led him to quit the Gesellschaft in 1910, a year before his passing⁶⁶. In 1876, however, without the support of such an institution, the access to recent mathematical research was an every-day problem for Schubert.

To directly order scientific journals and books, as Schubert repeatedly makes clear in his correspondence, was extremely costly. In one of his earliest letters to Halphen, Schubert mentions having bought directly the second volume of the *Bulletin de la Société Mathématique de France*, for 18 Reichsmark⁶⁷; a hefty price at a time when the average annual wage was below 1000 Reichsmark⁶⁸. Throughout his initial exchanges with Halphen, a Parisian mathematician well-established at the budding Société Mathématique de France, Schubert makes this difficulty explicit, and seeks ways to have publications relevant for his own work directly sent to him, free of charge. Beyond financial concerns, Schubert used his connection with Halphen to obtain news from the profession, and to maintain his knowledge of the research being conducted in Paris on enumerative geometry, by mathematicians such as Louis Saltel or Georges Fouret⁶⁹.

It comes as no surprise, therefore, that as early as July 1876, Schubert consulted Halphen about the conditions for joining the Société Mathématique de France as a foreign member. To join this society would mean receiving its *Bulletin* for free, have a venue to communicate one’s research to French mathematicians (by sending notes to be read and discussed during the Society’s bi-weekly meetings, which Schubert did several times, even if said notes were never published in the *Bulletin*), and to remain updated with regard to the state of the art of geometry in Paris. Halphen happily obliged: on November 14th 1876, Schubert was presented (in abstentia) by Halphen and Jordan before the Society;

⁶⁵The first volume of the *Mitteilungen der Mathematischen Gesellschaft in Hamburg* would be published in 1881.

⁶⁶ [Bureau & Renschuch, 1966], pp.11-12.

⁶⁷“Ich habe mir jetzt den II. Band des Bull. wegen Ihrer, Saltel’s u. Fouret’s Abhandlungen buchhändlerisch zukommen lassen, zu dem enormen Preise von 18 Reichsmark = $22\frac{1}{2}$ francs”, Letter Ms 5624 168, Schubert to Halphen, dated August 12th 1876. Taking inflation into account, this value roughly translates to around 92.5€ in 2015; per [Edvinsson, 2016]. The *Bulletin* was a particularly expensive journal according to Schubert, who explains in his letters that he could afford *Comptes-Rendus de l’Académie des Sciences*, and that the equally heavy price of the *Mathematische Annalen* was counter-balanced by the fact that the profit made there was then forwarded to the young orphan children left behind by Clebsch after his death at a young age.

⁶⁸ [Sommariva & Tulli, 1987], p.18.

⁶⁹We briefly discussed the contributions of Fouret and Saltel in 6.1.3.

two weeks later, he was unanimously elected as a member thereof⁷⁰. In the months following this election, the Société would fail to properly send Schubert his certificate of membership as well as the new issues of the *Bulletin*; which he insistently asked Halphen to remedy. These requests, as well as the many enquiries Schubert made to Halphen between his initial request in July, and the election itself in November, bespeak the paramount importance this affiliation had for the isolated German mathematician. Zeuthen, by comparison, only became a member of the Société Mathématique de France in 1881, despite having had prolonged interactions with French geometers since his visit in Paris in 1863; a sign that such affiliations were of no crucial importance to a well-established professor, whose institutional position enabled easy access to scientific literature from a variety of European countries. Tellingly, he never asks that Halphen sends him notes or articles, but merely that the latter tell him when and where such publications may appear.

On the mathematical front as well, the questions raised in both correspondences during this period and the manner in which they are raised differ. In his first few letters, Schubert expresses his happiness when seeing that Halphen is also attempting to find other *Produktensätze*, enquires about the details of certain proofs or theorems, communicates Hurwitz's methods relying on the principle of correspondence. In particular, Schubert does not react strongly to Halphen's announcement in November of a counter-example to the $\alpha\mu + \beta\nu$ formula, despite it occurring at almost the same time as Hurwitz's obtention of a new proof for this very formula⁷¹. Upon receiving Halphen's brief 1876 note for the *C.R.A.S.* (in which a counter-example is sketched, and the general conditions of applicability of Chasles' theorem are described), Schubert writes⁷²:

Auf Ihre Note gegen $\alpha\mu + \beta\nu$ war ich schon durch Zeuthen, und durch Klein gle-

⁷⁰ [SMF, 1877], pp.71-72.

⁷¹Recall that, in Hurwitz's and Schubert's joint paper, a mention of Halphen's counter-example is given.

⁷²"I had already been made aware of your note against $\alpha\mu + \beta\nu$ by Zeuthen and by Klein, immediately after its appearance. But before I could obtain the volume in question from the local, very cumbersome municipal library, I received your kind consignment last Tuesday. The same evening I announced to Stern a note for the *Göttingen Nachrichten*, which I wrote on Wednesday and Thursday, and which he presented to the Göttingen Society on Saturday. Perhaps out of a certain reverence toward Chasles' theorem, I have tried in this note, through the publication of the proof of $\alpha\mu + \beta\nu$ which Hurwitz and I found, to take up the cudgel [eine Lanze zu brechen] – perhaps the last one – for this interesting theorem. You will perhaps find it adventurous to publish the proof of a theorem which is attacked in its general validity. But my main concern was the love of science. I believe that through my note, even if it contains the same errors, the matter will be brought to the fore [aufs Tapet bringen] even more, attracting even more interested parties and collaborators who may shed light on the matter from even different points of view. So I published the proof and some remarks about the meaning [Sinn] of the theorem, prompted by your attack, even at the risk that you prove me in your detailed paper that I was totally wrong, which I will be able to bear well. On this I found my trust that you will not take my defense of the theorem as the beginning of a personal polemic against you", Letter Ms 5624 169, Schubert to Halphen, dated November 5th 1876.

ich nach ihren Erscheinen aufmerksam gemacht. Ehe ich mir aber das betreff. Heft aus der hiesigen, sehr schwerfälligen Stadt-Bibliothek verschaffen konnte, bekam ich am vorrigen Dienstag Ihre gütige Zusendung. Am denselben Abend noch annoncirte ich Stern eine Note für die Gött. Nachr., die ich Mittwoch und Donnerstag abfasste, und die er Sonnabend der Göttinger Societät vorgelegt hat. Vielleicht aus einer gewissen Pietät gegen Chasles's Satz, habe ich in dieser Note durch Publikation des von mir und Hurwitz gefundenen Beweises von $\alpha\mu + \beta\nu$ eine Lanze – vielleicht die letzte – für diesen interessanten Satz zu brechen versucht. Sie werden es vielleicht abenteuerlich finden, den Beweis eines Satzes zu publiciren, der eben in seiner Allgemeingültigkeit angegriffen ist. Die Hauptsache war mir aber die Liebe zur Wissenschaft. Ich glaube, dass durch meine Note, auch wenn dieselbe Irrthüme enthält, die Sache noch mehr aufs Tapet gebracht wird, noch mehr Interessenten und Mitarbeiter wirbt, die vielleicht von noch andern Standpunkten die Sache beleuchten. So publicirte ich den Beweis und einige Bemerkungen über den Sinn des Satzes, veranlasst durch Ihren Angriff, selbst auf die Gefahr hin, dass Sie durch Ihre ausführliche Abhandlung mir beweisen, dass ich mich total geirrt habe, was ich gut werde, tragen können. Dazu kam mein begründetes Vertrauen darauf, dass Sie meine Vertheidigung des Satzes nicht als Eröffnung einer persönlichen Polemik gegen Sie auffassen werden.

The note mentioned here by Schubert is none other than that which would become the paper co-authored by Hurwitz, discussed in the previous chapter. Schubert was, as we know, not entirely convinced by Halphen's doubts (which, it is worth recalling, had not yet been published in the guise of a full-fledged theory, but merely through brief notes for the *Comptes-Rendus de l'Académie des Sciences*). Indeed, Hurwitz and Schubert, in their joint paper, had included at the last minute a paragraph explaining why the authors felt that the validity of Chasles' theorem, once properly understood, remained general⁷³. Similarly, in a letter to Halphen written just after the publication of this paper, Schubert expressed his continued belief that the theorem was true⁷⁴. The correspondence between

⁷³“Unter diesen Umständen wollen wir, noch immer von den Allgemeingültigkeit des richtig verstandenen Satzes überzeugt, nicht länger zögern, unsern geometrischen Beweis desselben zu veröffentlichen.”, [Hurwitz & Schubert, 1876], pp.503-504.

⁷⁴“Sie werden aus der Göttingen Note ersehen haben, warum mich Ihre Beispiele noch nicht von der Unrichtigkeit des $\alpha\mu + \beta\nu$ überzeugt haben, und ich fasste meine Note hauptsächlich deshalb ab, um die Sache ordentlich, auch in Deutschland, zur Sprache zu bringen, damit schliesslich aus dem Widerstreit der Meinungen die Wahrheit Frieden stiftend sich erlebe. Darum thut es mir leid, dass Sie meiner Vertheidigung des $\alpha\mu + \beta\nu$ in Ihrer ausführlich Abh. keine Widerlegung folgen lassen können. Über die Sache selbst kann ich deshalb noch nicht sprechen, weil ich Ihre Note noch nicht habe”, Letter Ms 5624 170, Schubert to Halphen, dated December 4th 1876.

Schubert and Halphen halted after this unfruitful exchange⁷⁵, only to resume sporadically over the course of the following two years.

In parallel, however, Halphen was having a very different conversation with Zeuthen. Quite tellingly, Schubert was only made aware of Halphen's doubts against the $\alpha\mu + \beta\nu$ formula several months after Zeuthen. As one of the letters printed in his collected works shows, Halphen had indeed written to Zeuthen as early as July 29th 1876 that he was convinced that this formula was false⁷⁶. Between August and October 1876, Halphen and Zeuthen wrote three times to one another, discussing at length the nature of the former's refutation of Chasles' formula. While Zeuthen was eager to accept Halphen's refutation (after raising a few doubts, promptly answered), the two mathematicians nonetheless had a different understanding of the nature of this refutation. As we shall discuss in the rest of this section, Halphen thought it meant that Chasles' formula was less general than his own, while Zeuthen suggested viewing them as different viewpoints upon the same question, each with their relative strengths and weaknesses. At any rate, Zeuthen and Halphen quickly agreed on the fact that Schubert was wasting his time trying to preserve the formula, and that he did so out of a relative misunderstanding of the subject at hand⁷⁷:

Et l'‘aventure’ de M.Schubert ! Il ne me l'a pas racontée encore, de façons [sic] que j'en juge seulement par ce que vous m'en écrivez. Trouvant ordinairement assez de bon sens à ce que fait M.Schubert, j'essaie de me l'expliquer. La seule conjecture qui me semble probable, c'est qu'il veut compter au nombre des $\alpha\mu + \beta\nu$ solutions les solutions singulières. J'ai parlé de ce point de vue dans ma réponse à votre première communication sur cette matière ; mais je vous ai convenu que ce point de vue n'est pas à défendre au moment où l'on introduit plus d'une de ces conditions qui conduisent à des nombres de solutions propres qui ne sont pas exprimables par $\alpha\mu + \beta\nu$. M.Schubert aurait donc tort aussi dans le cas où ma conjecture serait juste ; mais nous verrons ce qu'il va dire. En tout cas, il est hardi de publier quelques chose en cette

⁷⁵In December 1876, Schubert sent a note on this very question to be read before the Société Mathématique de France, which Halphen enabled; after which he and Lindemann made some remarks. However, this was not published in the *Bulletin* afterwards. See [SMF, 1877], p.72.

⁷⁶“Je ne doute plus de l'inexactitude de $\alpha\mu + \beta\nu$ ”, [Jordan *et al.*, 1924], p.629.

⁷⁷“And M. Schubert's ‘adventure’! He did not tell me about it yet, so that I can only judge it based on what you tell me. Finding ordinarily what M. Schubert does to be rather sensible, I struggle to explain it to myself. The only conjecture which seems likely to me, is that he wants to count singular solutions amongst the $\alpha\mu + \beta\nu$ solutions. I spoke about this point of view in my response to your first communication on the matter ; but I conceded in my response that this point of view is not to be defended as soon as one introduces more than one of these conditions which lead to numbers of proper solutions which are not expressible by $\alpha\mu + \beta\nu$. M. Schubert would thus be wrong in the case where my conjecture is correct as well ; but we shall see what he has to say. At any rate, it is brave to publish anything on the matter before the publication of your new theory”, Letter Ms 5624 223, Zeuthen to Halphen, dated October 19th 1876.

matière avant l'apparition de votre nouvelle théorie.

Zeuthen did not immediately react via a letter to Hurwitz's and Schubert's proof of correctness of the formula in question. In fact, after the letter quoted above, he did not write to Halphen until December 1877. At this point, however, Zeuthen was ever more convinced that the question had been solved, and that all that was left to do was to wait for the publication of Halphen's memoirs, which, as we mentioned in the previous chapter, had been considerably delayed⁷⁸. Despite the perceived misguidedness of this pursuit of a proof for Chasles' formula Zeuthen wrote a long review of Schubert's *Beiträge* (as well as an earlier paper on systems of second-order surfaces) for the *Bulletin des sciences mathématiques et astronomiques*⁷⁹. While Zeuthen's reviews contain neither praise nor criticism for Schubert's work (except for a few mentions of the 'important formulae' it contains), they display a remarkable level of detail and familiarity with it. In a similar fashion, Zeuthen's 1880 review of Schubert's *Kalkül*, published in the same journal, would only contain a rather neutral exposition of the methods and results contained in the book, but merely a veiled reference to the disagreement with Halphen over the $\alpha\mu + \beta\nu$ formula⁸⁰.

By the end of the year 1876, therefore, both conversations paused, with Zeuthen and Schubert having different appreciations of the situation. None of them, however, had yet access to Halphen's theory for conic sections. Although Zeuthen had asked for a few details regarding the status of Halphen's third degeneration for conics, it would take him at least two more years to begin to properly understand the refutation of $\alpha\mu + \beta\nu$, as his ulterior letters to Halphen show. In September 1878, for instance, Zeuthen would ask Halphen technical details on the status of the ratio $\frac{m}{n}$ in his classification of the modes of degeneration for conics in a system⁸¹. Schubert's correspondence with

⁷⁸“Mais j'oublie de vous demander où devient votre mémoire sur les caractéristiques. Depuis avril je l'ai cherché en vain dans le Journal de Mathématiques. [...] Il y a longtemps que je n'ai eu des nouvelles de M.Schubert. [...] J'espère qu'il n'est pas sur le théorème inexact de $\alpha\mu + \beta\nu$ ”, Letter Ms 5624 225, Zeuthen to Halphen, dated December 1st 1877.

⁷⁹ [Zeuthen, 1877]. Note that, at this point, and in keeping with Schubert's own opinion, Zeuthen translates ‘abzählende Geometrie’ by ‘Géométrie numérique’. In fact, Zeuthen had asked Halphen in a letter written on November 5th 1879 what the suitable French term was; it is likely that Halphen replied with the term which Schubert himself had suggested in a letter written on May 21th 1876.

⁸⁰ [Zeuthen, 1880].

⁸¹“C'était bien aimable d'ajouter à la série de vos autres obligeances celle de suppléer, dans une lettre, à mon défaut de présence d'esprit lorsqu'il s'agirait de saisir des idées mathématiques exposées oralement. En lisant votre lettre, je vois que vos raisonnements sont assez simples pour devoir être saisis tout de suite ; mais en même temps j'y trouve aussi des remarques sur lesquelles j'étais bien aise de pouvoir réfléchir chez moi. J'avais besoin par exemple d'une réflexion pour m'assurer du fait, dont vous faites usage, et qui me paraissait plausible, seulement, à la première lecture, que l'ordre m d'une conique dégénérée est égal à son degré de multiplicité dans la formule $2\mu - \nu$ (Vous ne définissez expressément dans votre lettre que le rapport $\frac{m}{n}$; mais je suppose que, pour définir explicitement m ou n , vous faites, dans le voisinage d'une conique singulière, le système ou la ‘branche’ du système dont il s'agit, dépendre d'une manière rationnelle d'un paramètre variable qui peut être zéro pour la conique singulière : m est alors le double de

Halphen, like Zeuthen's, was rather limited throughout the years 1877 and 1878; however, unlike the Dane, his occasionnal letters do not concern Halphen's theory, but rather a variety of other common interests⁸². Nonetheless, Schubert was made aware of the publications of Halphen's theory in the *Mathematische Annalen*, of which he wrote the aforementioned review for the *Jahrbuch*⁸³ (which also mentions Halphen's then-upcoming memoirs in the *Proceedings of the London Mathematical Society* and in the *Journal de l'École Polytechnique*). In the *Kalkül*, as we discussed in the previous chapter, he would only include a brief note mentioning Halphen's counter-examples, and explain that he received Halphen's memoirs too late to be properly included in the book.

8.2.2 A dispute made public

It is only in November 1879 that this three-way epistolary exchange became active once more. Schubert's *Kalkül* was about to be published, and his enumerative geometry for triangles was already at an advanced stage of development⁸⁴; Halphen's full-fledged alternative theory for the enumerative geometry of conics had passed in the hands of both Schubert and Zeuthen; and the latter had been reading attentively the papers of the other two, turning them into a course on enumerative methods he had just begun teaching at the University of Copenhagen⁸⁵. This confluence made for a situation rife for conflict: Schubert was persevering on his own line of research, which he deemed equally worthy of pursuit as Halphen's. In turn, Halphen was losing patience with what he considered

l'ordre du segment infiniment petit intercepté sur une droite quelconque par la conique correspondante à une valeur infiniment petite du 1^e ordre du paramètre)", Letter Ms 5624 228, Zeuthen to Halphen, dated September 5th 1878. For the definition of this ratio, see section 6.2.3 of this dissertation.

⁸²In particular, Schubert communicates his results on the degenerations of cubics to Halphen, and enquires of Halphen on the philology of perfect numbers communicated at the SMF by Carvallo.

⁸³ [Schubert, 1880c].

⁸⁴In a letter written to Halphen this very month, Schubert mentions having already found the 7, 17, and 22 symbols from which the *Produktsätze* presented in the previous chapter derive.

⁸⁵The content of this course is described by Zeuthen in a subsequent letter in the following terms: "Vous exprimez, un peu ironiquement – non pas envers moi mais envers l'objet de mon cours – le désir de venir suivre ce cours. Je vous réponds que vous n'y gagneriez pas grand chose. La plupart des résultats, ~~du moins~~ excepté ceux qui servent seulement d'exemples d'applications, vous sont bien connus (formules de Plücker, formules analogues dans l'espace, théorie des caractéristiques, y compris détermination des courbes qui satisfont à des conditions données avec l'indication expresse qu'il ne faut pas croire que leurs nombres aient toujours l'expression $\alpha\mu + \beta\nu$ etc.). Les méthodes sont celles dont on doit les germes à Poncelet, Steiner et Chasles (pour le principe de correspondance bien plus que la germe à Chasles) et qui sont développées et fertilisées et appliquées par De Jonquières et Cremona, puis MM. Halphen et Zeuthen et – je l'avoue – aussi par M.Schubert – bref les méthodes dont je me sers dans mes recherches personnelles. Vers la fin du cours je pense donner une esquisse ~~du~~ ~~calc~~, illustrée par des exemples, du calcul symbolique introduit par vous, et dont le développement que M.Schubert ne semble peut-être trop exécuté pour ce que cette forme contient encore, mais qui ~~contient~~ a beaucoup de bon (exemple : formule VII de la page 32 dont la traduction géométrique est un théorème, facile à déduire, mais non pas immédiatement évident). J'espère donc qu'en venant suivre mon cours vous trouverez les mots que je sers aux jeunes gens assez sains et du moins innocents, quant même vous n'y trouveriez pas trop de gout et de force", Letter Ms 5624 232, Zeuthen to Halphen, dated November 14th 1879.

to be a misunderstanding of their position on Schubert's part. On November 14th 1879, Zeuthen would express a similar feeling to Halphen in unambiguous terms⁸⁶:

J'ai encore quelques remarques à ajouter sur $\alpha\mu + \beta\nu$ et notre ami commun à Hambourg, ou plutôt, je n'ai pas ~~besoin de~~ beaucoup à ajouter ; car ~~à cet égard~~ je ne le comprends pas M.Schubert à cet égard. Après sa première publication de la démonstration de Hurwitz – qui a je crois des ressemblances notables avec votre ancienne démonstration – je lui ai écrit sur votre découverte. Sa réponse me faisait croire de l'avoir persuadé de la nécessité d'exclure dans une théorie complète les solutions $\frac{0}{0}$ provenant de la troisième dégénération, c'est-à-dire de l'inexactitude, prouvée par vous, de la formule $\alpha\mu + \beta\nu$. Et à présent il vient la démontrer de nouveau ! même sans s'entourer de réserves et de définitions précises qui serviraient à l'explication de ce qu'il semble regarder comme un fait, que la formule pourrait être en même temps vraie et fausse ; car je ~~suis persuadé~~ ne crois pas qu'il ~~ne~~ veut pas nier votre découverte. Sans doute il ne la comprend pas du tout ; au cas contraire il ne nommerait pas au même instant les objections insignifiantes de M.Saltel, et il aurait la prudence de chercher – **grâce à l'aide qu'offre [sic] vos premiers exemples**⁸⁷ – la faute de sa démonstration, qui n'est pas extrêmement difficile à trouver. Il me semble à cet égard très naïf ; mais tant pis pour lui ; vous n'y perdez rien. Du reste je comprends très bien que vous souhaitez à cette occasion faire comprendre mieux votre découverte, quant même vous auriez besoin de faire usage de répétitions, qui est la mère de la science.

Things would not improve from there, and in particular the relationship between Halphen and Schubert would quickly deteriorate. But a week later, Zeuthen wrote: “certainly, I was wrong in calling M. Schubert our common friend; but I am the common friend

⁸⁶“I still have remarks to add about $\alpha\mu + \beta\nu$ and our common friend in Hamburg, or rather, I need not add much ; for, in this regard, I do not understand M. Schubert. After his first publication of Hurwitz's proof – which I believe has notable similarities with your old one – I wrote to him about your discovery. His response led me to believe I had persuaded him of the necessity to exclude from a complete theory the solutions $\frac{0}{0}$ which come from the third degeneration, that is to say of the inexactitude, proven by you, of the $\alpha\mu + \beta\nu$ formula. And now he just proved it again ! even without surrounding himself with precautions and precise definitions which would serve to explain that which he seems to view as fact, that the formula is at the same time true and false ; for I do not believe he wants to deny your discovery. There is no doubt that he does not understand it at all ; otherwise he would not mention at the same time Saltel's insignificant objections, and he would have the caution to search - with the help of your examples - the flaw in his proof, which is not extremely hard to find. He seems to be very naive in this regard ; so much the worse for him ; you shall lose nothing from it. At any rate, I understand very well that you wish on this occasion to have your discovery better understood, even if you should have to use repetitions, which are the mother of science”, Letter Ms 5624 232, Zeuthen to Halphen, dated November 14th 1879.

⁸⁷This section in bold is circled by Zeuthen in his letter.

of yours and Schubert's⁸⁸". This coincides with the resuming of Schubert's epistolary exchange with Halphen, on November 15th. Whilst working on the enumerative geometry of triangles, Schubert had noticed degenerations similar to those discussed by Halphen in his memoirs on conics. Recall, indeed, that amongst the symbols for degenerations of triangles presented in the previous chapter, there was one for the case where the two elementary degenerations occurred simultaneously, that is to say when all three vertices collapsed on one point and all three sides on one straight line (see 7.4.2). However, Schubert sought to accommodate such degenerations with the kinds of results he thought central to enumerative geometry, namely *Produktensätze*⁸⁹.

This did not go well with Halphen: it showed that while Schubert had on the surface accepted his refutation via the B -degeneration for conics, he had not understood the underlying argument that general enumerative formulae, such as Chasles' $\alpha\mu + \beta\nu$, counted figures which were no real solutions to the enumerative problems at hand. With triangles as with conics, Halphen was convinced that such general formulae were impossible. Thus, upon receiving Schubert's letter (and his note on triangles), Halphen immediately wrote to Zeuthen, describing the situation in no uncertain terms: "At first sight, I knew I had false theorems before my very eyes⁹⁰". In the rest of his letter, Halphen described Schubert's notations for conditions on triangle, his *Produktensatz*, and constructed a counter-example to it – which he sent to Schubert as well.

Halphen considers two systems of triangle Σ, Σ' , of respective levels 5 and 1 (in fact, Halphen calls Σ' a system and Σ a condition). Σ' is defined as the collection of triangles of which two vertices b and c are given and fixed, and whose third vertex a is on a given curve C of order m , such that C passes through b and that bc is tangent to C at b . Note that the system Σ' as defined by Halphen contains a degeneration determined by two infinitesimal quantities, namely the triangle whose three sides are infinitely close to the (given) straight line bc , such that a is infinitely close to b . While he does not make this part of his construction explicit, Halphen needs it in order to ensure that his system will always contain fewer solutions than the numbers yielded by Schubert's formulae.

Denoting μ the multiplicity of b as a point of C (μ being possibly equal to 1), the left factors of every term in Schubert's *Produktensatz* for triangles can be easily computed⁹¹.

⁸⁸"Certainement j'avais tort en appelant M.Schubert notre ami commun ; mais moi je suis l'ami commun de vous et M.Schubert", Letter Ms 5624 233, Zeuthen to Halphen, dated November 22nd 1879.

⁸⁹In fact, Schubert also sent a first draft of his paper on triangles directly to Chasles, hoping to have it published in the *Comptes-Rendus de l'Académie des Sciences*. Unbeknownst to Schubert, an aging Chasles had passed this note along to Halphen, who most likely had reviewed it negatively. Chasles never replied to this sending, and the note was never published, two things which Schubert, ironically, lamented in subsequent letters to Halphen.

⁹⁰"Du premier coup, je savais donc avoir sous les yeux des théorèmes faux", [Jordan *et al.*, 1924], p.632.

⁹¹The values of $a', b', c', \alpha', \beta', \gamma'$ are directly given by Halphen; they are (respectively) $m, 0, 0, 0, m, m - \mu$. It is obvious that $b = c = 0$, as b and c are given and fixed. Similarly, α is given as the straight line

Thus, for any condition Z (or, in other words, any system Σ defined by a simple condition), Schubert's *Produktensatz* can be rewritten to express the number of triangles common to Σ and Σ' (that is to say the number of triangles in Σ' satisfying Z) as

$$x(\Sigma, \Sigma') = Am - B\mu$$

where A and B are two polynomial expressions in the characteristics of the system Σ .

Such a formula, Halphen argued, cannot be generally true for all conditions Σ ; and he would prove so by considering one specific condition which puts the lie to the formula. Given three points $\omega, \omega', \omega''$, there is exactly one conic Ω passing through these three points and touching the straight line bc at point b - a point and a line which, remember, are given and fixed. The condition defining Σ is that a lies on this conic⁹².

For a triangle common to both systems, b, c, α are given, and the possible vertices a are given by the intersections of C (which is given) and Ω (which is uniquely determined). However, Halphen explains, "the number of these intersections does not depend solely on m and μ , but also on the number of branches of C tangent to bc at point b , and even on the orders of their contacts with this straight line⁹³". This is where Schubert would perhaps conclude that this number is none other than $2m - \mu$, that is to say the number of intersections given by Bézout's theorem, minus the number of these intersections which in fact are due to b being a multiple point of C . However, for Halphen, among the μ branches of C jutting outward from point b (per definition of the multiplicity of a point), not all count equally toward the solution of the enumerative problem of computing $x(\Sigma, \Sigma')$. Among these branches, Halphen continues, there are μ_1 branches which are not tangent to bc , μ_2 branches which have contacts with bc at an order lower than 1 (and ν_2 denotes the sum of these orders), and μ_3 branches which have contacts with bc at an order higher than 1. Naturally, $\mu = \mu_1 + \mu_2 + \mu_3$, and

$$x(\Sigma, \Sigma') = 2m - \mu - \nu_2 - \mu_3$$

From there on, Halphen can easily consider specific values for μ such that his direct computation of $x(\Sigma, \Sigma')$ fails to match the result yielded by the alleged general formula above. This is because, Halphen shows, m and μ are not sufficient to characterize the

joining b and c , thus $\alpha = 0$. Given a straight line in the plane, it intersects C at exactly m points. Thus, $a = m$, as for a to be on a given straight line, it has to coincide with one of these intersections. The other numbers can be obtained in similar fashion. To compute ε' , Halphen considers a specific condition Z , namely that a triangle be inscribed in a given conic B_1 which passes through three given points. Halphen directly computes how many triangles in Σ' satisfy Z , and from there, derives that $\varepsilon' = m - \mu$.

⁹²Halphen frames his condition through a converse statement, but this makes it clearer that it is indeed a simple condition and that it bears on the triangle abc itself.

⁹³ [Jordan *et al.*, 1924], p.631.

contacts which can arise in his example, and thus the adequation of a triangle as a solution to this particular enumerative problem. This example, for Halphen, has a double merit: not only does it refute Schubert's formula, but it also suggests that one can always form conditions which require further numbers to be taken into account, and therefore that no easy fix to such *Produkstensätze* is to be hoped. For triangles as for conics, Halphen contended that there was no finite, compact formula that would solve all problems – unless the requisit was added that the systems involved contain no degenerations defined by more than one infinitely small quantity⁹⁴.

On November 21st 1879, Halphen presented before the Société Mathématique de France his critical remarks against Schubert's *Kalkül*, and in particular its sixth chapter on the theory of characteristics and *Produkstensätze*⁹⁵. In this communication, which was later printed in the *Bulletin*, Halphen gave several counter-examples to Schubert's formulae for a variety of other figures (such as the figure composed of n points on one straight line), all relying on similar constructions to those described above. These remarks were also sent directly to Schubert.

A back and forth exchange ensued between Halphen and Schubert, with the latter attempting each time to save his formulae and to explain this situation to which both referred as “ $(\Sigma, \Sigma') = \{1, 2\}$ ”. Schubert sent various notes in response to be read before the Société Mathématique de France, including substantial draft papers in French. Meanwhile, Schubert would lament the degradation of his relationship with Halphen in letters to Zeuthen, upon whom it befell to try and accommodate both mathematicians. While Halphen attempted to convince Schubert that the latter's position was untenable, he also asked Zeuthen to take a stand on the matter, and to do so publicly. Zeuthen declined, and instead manoeuvred in order to convince Schubert to publicly retract some of his claims. While Zeuthen was firmly on Halphen's side when it came to criticizing Schubert's attitude and mathematical skills, he nonetheless had a different understanding of the nature of the disagreement. For Zeuthen, Halphen's results were not more ‘general’ than Schubert's, but rather more ‘absolute’.

On January 16th 1880, Halphen finally agreed to have two of Schubert's latest short notes read before the Société and published in the *Bulletin*⁹⁶. In these notes, Schubert conceded that he had misunderstood Halphen's theory of conics, and that the formulae published in §38 of the *Kalkül* were erroneous. This paragraph, the second in the chapter on the *Charakteristikenprobleme*, was concerned with the theory of characteristics for

⁹⁴“Pour toute figure, il existe une formule $\alpha\mu + \beta\nu + \gamma\rho + \dots$ d'un nombre déterminé de termes donnant le nombre de figures qui satisfont à une condition et font partie d'un système si ce système ne contient que des dégénérescences caractérisées par un seul infiniment petit”, [Jordan *et al.*, 1924], p.635.

⁹⁵ [Halphen, 1880].

⁹⁶ [Schubert, 1880d], [Schubert, 1880b].

conic sections; it contained an adaptation of Hurwitz's proof of the $\alpha\mu + \beta\nu$ formula⁹⁷. Schubert, before the entire Société, agreed that these formulae were true only in systems free of Halphen's third degenerations. Of his other formulae, Schubert only agreed to the proposition that Halphen's counter-examples point to cases implicitly proscribed in his book. This is the same line of defense he had maintained in his correspondence, in one of his longest letters, filed amongst Halphen's mathematical papers⁹⁸:

Die Differenz ist also mehr eine sprachliche geworden. [...] Denn über die Nicht-Anwendbarkeit meiner Formeln in gewissen Fällen sind wir nie uneinig gewesen. Nun Sie sagten, die Formeln sind ungenau, weil sie in jenen Fällen nicht passen, ich sagte, die Formeln sind dann sogar sinnlos, und die Nicht Anwendbarkeit ist im Zusammenhange meines Buches selbstverständlich.

Indeed, that Halphen's constructions constitute proper counter-examples to Schubert's theory can be disputed. For starters, Halphen's systems are defined in ways that seem rather remote from Schubert's geometrical practice. Furthermore, at the core of the discrepancy revealed by Halphen between the formula and the direct computation lies the delicate status as solutions of certain degenerate triangles, so that this counter-example can in no way be construed as an *experimentum crucis*; a test which conclusively shows in full the falsity of the formula. In fact, Schubert did not at first accept this refutation: the fault that these examples revealed, he argued in his response to Halphen's letter, lay "not in the formulae, in the misunderstanding of their content⁹⁹". More precisely, his argument would be that his *Produktenformeln* only apply to systems of a given level, and that the degenerations through which Halphen disputed the generality of these formulae were of a higher-order than permitted by these formulae. Discussing one of Halphen's examples bearing on figures Γ constituted by three points on a line, Schubert attempted to borrow from Halphen's own vocabulary, by admitting that a better job could have been done to give a priori the conditions of applicability of his formulae; whilst refusing to reject them as being outright false¹⁰⁰:

⁹⁷ [Schubert, 1879b], pp.284-288.

⁹⁸"The difference has therefore become more of a linguistic one. [...] Thus, we have never disagreed about the non-applicability of my formulae in certain cases. You said that the formulae are inexact because they do not work in certain cases, I said that the formulae are even meaningless then, and that the inapplicability is self-evident in the context of my book", Letter Ms 5621, Schubert to Halphen, dated December ? 1879.

⁹⁹"Freilich lassen sich Beispiele erfinden, bei denen man meine Dreieck-Formeln ohne Weiteres nicht anwenden kann. Daran sind aber die Formeln nicht schuld, sondern das Missverständniss ihres Inhalts", Letter Ms 5624 174, Schubert to Halphen, dated November 23rd 1879.

¹⁰⁰"In my Book, I have always assumed that a system of ∞^i straight lines g , on which lie three points p_1, p_2, p_3 , contains ∞^{i-1} straight lines g characterized by the reunion of two of these points, and that such a system contains only ∞^{i-2} straight lines g characterized by the reunion of all three points. This is why these formulae cannot only be applied either to cases where a system of ∞^i figures Γ contains the

Dans mon Livre, j'ai toujours supposé qu'un système de ∞^i droites g , possédant trois points p_1, p_2, p_3 , contienne ∞^{i-1} droites g caractérisées par la réunion de *deux* de ces points, et qu'un tel système ne contienne que ∞^{i-2} droites g caractérisées par la réunion de tous les *trois* points. C'est pourquoi les formules [en question] ne peuvent se rapporter ni aux cas où un système de ∞^i figures Γ contient la dégénérescence constituée par la réunion de plus de deux points, ni aux cas analogues. Je vois maintenant qu'il aurait été *utile* d'ajouter cette restriction à la *formule expressément*, parce que cette formule, ayant encore des coefficients indéterminés, ne fait pas connaître les cas exclus par elle-même.

It remains unclear how much Schubert's partial concession had to do with the perceived need to maintain a network in France, if only to obtain news of the profession and journals sent to him for free. Once Schubert's notes were published, his correspondence with Halphen almost immediately stopped. Schubert's subsequent engagement with foreign mathematicians would shift quite quickly from France to the United States. Zeuthen's correspondence with Halphen lasted for a little bit longer, revolving around other themes. By the end of 1881, it would also come to a halt, only to be rekindled in 1886 after the publication of Study's dissertation. For all intents and purposes, the debates over Chasles' $\alpha\mu + \beta\nu$ formula – and the very possibility of *Produktensätze* at large – between the three men were over by February 1880.

This was a multi-faceted discussion, one in which technical and mathematical arguments were intertwined with discussions of a more philosophical nature. Through their disagreement over the generality, applicability, or truth of formulae such as $\alpha\mu + \beta\nu$, Halphen, Schubert, and Zeuthen were discussing what the rules and goals of mathematical life were and ought to be. Are mathematicians rigorous explorers who, armed with their expert knowledge and their analytical tools, retrace the borders of the applicability of a formula? Or are there free creators of symbols and concepts, bound only by the law of non-contradiction, and motivated by a quest for unitary knowledge? Such were the broader questions which, in the rest of this section, we shall argue lay behind the dispute over Chasles' formula.

8.2.3 Halphen's analytical campaign

We opened this chapter by remarking the vast differences in mathematical styles present in Halphen's and Schubert's mathematical papers dealing with similar questions and objects.

degeneration constituted by the reunion of more than two points, or analogous cases. I see now that it would have been useful to add expressly this restriction to the formula, which, still having indeterminate coefficients, does not show by itself what the excluded cases are", [Schubert, 1880d], p.60.

Another dimension to this contrast must be added, in that what both mathematicians understood to be the norms of their scientific practice differed widely as well.

Halphen viewed his task as a mathematician to be that of an expert analyst, both in the mathematical and general sense of the term. Amongst those who had taken up Chasles' theory of characteristics between 1864 and the early 1880's in a prolonged manner, he was the only one with a real mastery over the new methods and concepts of the algebraic analysis of curves: Clebsch did not live long enough to pursue his line of enquiry, and neither Lindemann nor Cayley attempted to engage with Halphen's results on this matter. And yet, for Halphen, this analytical mastery was necessary to successfully reframe the problems tackled by De Jonquières and Chasles, as Halphen made explicit in his 1885 *Notice* as part of his application to the Paris Académie¹⁰¹:

Combien d'incertitudes, de tâtonnements, de fautes mêmes, vite corrigées, a-t-on pu voir dans les essais de ce siècle sur la Géométrie générale, qui se mêle à la *théorie des fonctions algébriques* ! Cette dernière est, elle-même, fort nouvelle. Il fallait l'attendre et adapter à ses découvertes les formes de la Géométrie.

But analysis was not just a collection of mathematical tools, for Halphen. It was a form of discipline, a normative and regulative ideal for his mathematical practice. This ideal was characterized by certain epistemic virtues such as rigour, precision, thoroughness; its purpose was to dissipate the illusions of intuition, and to lay bare the inconsistency of mathematical concepts grounded in imagination rather than true reasoning.

Indeed, in a letter to Zeuthen at the peak of his discontent with Schubert, Halphen wrote¹⁰²

De toutes les raisons qui militent contre les théorèmes prétendus *généraux*, la meilleure est celle-ci : les arguments dont on peut les couvrir disparaissent quand les deux êtres (C) , (Σ) sont définis chacun par plus d'une équation.

¹⁰¹“How much uncertainty, fumbling, how many mistakes even, soon to be corrected, were seen in this century's attempts at a general Geometry, which mingles with the theory of algebraic functions! The latter itself is rather new. One had to wait for it, and to it adapt the forms of Geometry”, [Halphen, 1885], p.14.

¹⁰²“Of all the reasons one can enlist against the allegedly general theorems, the best is the following: the arguments with which they can be covered disappear when the two beings (C) , (Σ) [N.d.T.: (C) , (Σ) here refer to a figure solution of a problem, and a system of such figures] are each defined by more than one equation. In these circumstances, we must abandon intuition and come back to Analysis. By this term I mean true reasoning ; I demand no equation, of course. M.Schubert absolutely wants to change nature to accomodate it to his formulas. We deal with a problem that admits one solution: One! Are you joking? The formula yields two: therefore there are two! Do you know how I replied? I took the question to be a particular case of another, wherein the formula yields 5, and then of another, wherein the same formula gives one”, Letter Halphen to Zeuthen, dated December 7th 1879, [Jordan *et al.*, 1924], p.637.

Dans ces circonstances il faut abandonner l'*intuition* et revenir à l'analyse. J'entends par là le raisonnement véritable ; je n'exige pas d'équation, bien entendu.

M. Schubert veut absolument changer la nature pour l'accommoder à ses formules. Nous traitons un problème qui a une solution : *Une ! plaisez-vous ? La formule en donne deux : Donc il y en a deux !* Savez-vous comment j'ai répondu ? J'ai pris la question comme cas particulier d'une autre, où la formule donne 5, et ensuite d'une autre, où la même formule donne 1....

What Halphen's analysis of the theory of characteristics had shown was that there was no such thing as a system of characteristics (μ, ν) in general, just like his work on algebraic curves had shown that there was no such thing as a skew curve of order m in general. Systems of figures, Halphen had shown, are characterized by an infinite series of numbers unless certain modes of degeneration, depending on two or more infinitesimal quantities, were discounted; similarly, families of curves can only be meaningfully characterized by two numbers, and not just one (for instance, order and genus). For Halphen, to maintain otherwise and to write formulae bearing on systems (μ, ν) of conics was equivalent "to reasoning on what does not exist¹⁰³". Such reasonings can only produce the appearance of generality: formulae such as De Jonquières' $\alpha\mu$ or Chasles' $\alpha\mu + \beta\nu$, because they bear on ill-defined objects, are bound to be procrustean. Either one admits that they suffer limitations in their applicability, and thus one must agree with Halphen that they are imperfectly general; or one follows Schubert in twisting geometrical facts to accommodate ill-gotten formulae.

Thus, Halphen's practice of generality is one that consists primarily in the construction of analytical tools for the expression of perfectly precise translations of the notions that, theretofore, had remained implicitly defined by geometers. Like Clebsch, Halphen sought the general equations for all of the geometrical notions used by Chasles in his initial presentation of the theory of characteristics: notions such as that of a system, of a degenerate conic, of a condition, of a solution etc. Clebsch's analysis, however, was incomplete to Halphen's eyes. Looking back at his work in his *Notice*, Halphen made this clear as he discussed the novelties of his own contributions with respect to the works of Clebsch¹⁰⁴:

¹⁰³"Pas plus qu'il n'y a de courbe gauche générale de degré m , il n'y a de système général de coniques de caractéristiques μ . On ne peut raisonner sur ce qui n'existe pas", Letter Halphen to Zeuthen, dated December 7th 1879, [Jordan *et al.*, 1924], p.637. Halphen would later use similar terms in his memoir on skew curves: "dans la Géométrie plane, un nombre unique, le *degré* permet de caractériser une famille de courbes; tous les membres d'une même famille sont cas particuliers d'un seul et même être, bien défini par une équation, la courbe générale du degré considéré. Dans la Géométrie de l'espace, cette définition des courbes d'un même degré fait défaut. On reconnaît même, dès l'abord, que ces courbes forment plusieurs *familles*, entièrement distinctes entre elles", [Halphen, 1882], p.1.

¹⁰⁴"I immediately noticed that I still had to make precise a notion which had until then remained vague,

Pour dissiper les doutes, il fallait, je m'en aperçus aussitôt, préciser une notion qui, jusque-là, était restée vague, celle de l'*indépendance* entre le système de coniques, d'une part, et la condition supplémentaire qu'on impose, d'autre part, aux coniques de ce système. Souvent M. Chasles avait négligé de la mentionner ; mais chacun la restituait sans peine. Dans chaque exemple, en effet, rien n'est plus simple ; mais, dans la théorie générale, on ne voit pas d'abord comme préciser cette indépendance.

The flaw of Clebsch's proof, for Halphen, resided in the fact that his concept of "mobile solution" (*bewegliche*) had been made insufficiently precise and rigorous. Of course, Halphen found even worse deficiencies in the proofs of Lindemann or Hurwitz. Even the reasonings and proofs present in Schubert's papers and books, despite being supported largely by a machinery of abstract symbols, were cast as relying unduly on intuition – or even imagination – by Halphen. To speak of a system of characteristics (μ, ν) is already to let imagination suggest an object upon which no sound reasoning can be carried out. The tools of mathematical analysis, on the contrary, when expertly used, dispel the illusion that a general truth can be said about such objects; they serve as instruments of precision in the face of the rampant vagueness that prevailed throughout geometrical discourse¹⁰⁵.

The mathematical practice which befits this figure of the mathematician is one which attaches great importance to counter-examples. We saw in section 6.4 how Halphen not only began his second foray into the characteristics by communicating counter-examples to the Académie des Sciences¹⁰⁶, but repeatedly claimed that he was in possession of a method to generate counter-examples at will to any supposedly general formula that Schubert could throw at him. Counter-examples were of great importance to Halphen, not least because they reveal that the generality of a formula such as $\alpha\mu + \beta\nu$ is illusory, that the analysis of its domain of validity had been improperly carried out, and that further hypotheses had to be added to make its proof valid. Generality, for Halphen, was hard-won, through an expertly-conducted campaign to root out counter-examples, false intuitions and other pervasive causes of errors¹⁰⁷. In this sense, Halphen embodies the rise of 'mathematical anxiety' which came to characterize large parts of mathematical life toward the

namely that of the independence of, on the one hand, the system of conics, and on the other hand, the extra condition that is imposed on the conics of this system. Often M. Chasles had neglected to mention it, but everyone restored it effortlessly. In each example, indeed, nothing is simpler. In the general theory, however, it is not clear at first how to make this independence precise.", [Halphen, 1885], pp.9-10

¹⁰⁵In fact, Halphen uses similar vocabulary to frame his research on skew curves: "dès le début de mes travaux sur la Géométrie algébrique, j'avais compris la nécessité de préciser les notions, alors extrêmement vagues, qu'on possédait sur les courbes gauches", [Halphen, 1885], p.23.

¹⁰⁶In fact, Halphen had first presented these counter-examples at the *Congrès de l'Association française* in Clermont-Ferrand, on September 4th 1876, [Halphen, 1885], p.10.

¹⁰⁷This passage can be compared to that between the first two regimes of objectivity discussed in [Daston & Galison, 2007], namely 'truth-to-nature' and 'mechanical objectivity'.

turn of the century¹⁰⁸. These worries may seem overly familiar to modern-day readers, trained by textbooks which carefully delineate the hypotheses without which a theorem may be refuted through specific counter-examples. Countless mathematical books list counter-examples to well-known statements whose validity hinges upon the verification of every one of their hypothesis, bar none¹⁰⁹. And yet, this must not obscure the fact that Halphen's posture *qua* mathematician here is at odds with that of most of his interlocutors regarding the theory of characteristics. Contrast it, for instance, with Chasles' creed in Nature's tendency to provide simple and direct paths toward mathematical truth, described in the first chapter of this dissertation. No anxiety, no epistemic worry was to be detected in the epistemology of the so-called 'Prince of geometers': the simplicity of a method was a sure sign that it rested on general principles, and conversely. In this sense, the very long lists of propositions produced by Chasles between 1864 and 1876 relying on the principle of correspondence in order to (among other things) compute modules of certain conditions were a much surer sign of the worth of the theory of characteristics than any alleged proof of the $\alpha\mu + \beta\nu$ formula could be - proofs for which Chasles had little interest. The practice of generality which puts at its core the discussion of possible counter-examples, and the search for theorems completely safe from them, stands in stark contrast with the 'generic' practice of generality found in Chasles' geometry¹¹⁰. It is not surprising, thus, that Halphen would be compared to Abel in Jordan's obituary for the *Journal de Mathématiques Pures et Appliquées*: like Abel, he had first attempted to solve a problem - that of finding a formula for the number of conics satisfying five conditions - only to prove it was impossible¹¹¹.

Halphen was a career military-man, an artillery officer who had taken part in the 1870 defeat against Prussia, but also in the effort to modernize the French army in its wake. Until his death at a young age, he maintained both his mathematical and military activities; a dual occupation which was soon erected to a somewhat heroic status by his biographers. The idea that Halphen died of overwork, extremely busy at the forefront of the renewal of both French military and scientific power, is present in many accounts of his life written around the turn of the century. For instance, Brioschi opened his note on

¹⁰⁸"This mounting disquiet about so many aspects of mathematics is seldom discussed as a widespread part of mathematics life after 1850, although the individual cases of it are of course discussed in essays and books devoted to the relevant particular parts of the field. The disquiet contrasts with the dominant image of the nineteenth century in mathematics, the standard model of widespread innovation and success", [Gray, 2004], p.27.

¹⁰⁹To mention only two, written for different audiences, concerning different parts of mathematics, and in different countries, see [Steen & Seebach Jr., 1970], [Hauchecorne, 1995]. Counter-examples were also famously presented as playing an essential role in mathematical practice in [Lakatos, 2015].

¹¹⁰We borrow this term from Hawkins' discussion of the "generic reasoning" present in Hesse's analytical geometry, see [Hawkins, 1988], p.44.

¹¹¹ [Jordan, 1889], p.347. In fact, Abel, like Cauchy, spoke rather of rules and exceptions than of counter-examples, see [Sørensen, 2005].

Halphen's last communication by mentioning "the last months of his laborious life¹¹²"; while Jordan, in his obituary, depicted Halphen as "devoting his days to his professional duties, and his nights to his scientific works¹¹³". This portrait of Halphen as a man of labor was not merely the fabrication of biographers: it was at least partly due to Halphen's own self-styling. For an epigraph to his entry for the 1882 Steiner prize of the Berlin Akademie der Wissenschaften, he had borrowed a verse by Lucanus, famously quoted by Montaigne in his essay *De l'oisiveté*: "Variam semper dant otia mentem¹¹⁴". He would reproduce this very motto in his own *Notice* as part of his application for the Académie des sciences¹¹⁵, after framing the bulk of his mathematical research as a largely unified labor, guided by an ideal of rigor, and motivated by the need to secure sound foundations for algebraic geometry. Here as well, this could not contrast more with the effortless combination of truths that Chasles' ideal geometer was to experience.

This labour, for Halphen's colleagues, was characteristic of a certain turn of mind, of the sort of mathematician he was. Jordan, Hermite, Picard, and many others all present Halphen as someone who "dug profoundly into every subject he touched on and never left anything unfinished¹¹⁶", who had a "scrupulous conscience¹¹⁷", and "exhausted the consequences of known notions, striving to bring to the fore in the solution to each question the real elements upon which it depends¹¹⁸".

This characterization of Halphen as a hard-working, thorough, meticulous, expert-mathematician reflected epistemic virtues which also characterized the ideal war-strategist in the minds of his contemporaries. In fact, Halphen himself occasionally expressed his epistemic worries regarding the lack of rigor of his predecessors in passages rife with military overtones. For instance, in his *Notice*, Halphen depicted the shortcomings of past attempts at a theory of characteristics as a failure to properly prepare for a quasi-military campaign¹¹⁹:

¹¹² [Brioschi, 1889], p.520. Halphen was well known amongst Italian mathematicians, as is shown by two letters from Guccia to Jordan quoted in [Brechenmacher, 2016a], pp.79-82.

¹¹³ [Jordan, 1889], p.346.

¹¹⁴ [Anonymous, 1882], p.735. Lucanus' verse has been translated into "Idleness always produces a wavering mind", [Asso, 2010], p.89.

¹¹⁵ [Halphen, 1885], p.42.

¹¹⁶"Fidèle à son habitude constante de creuser tous les sujets qu'il touchait et de ne rien laisser d'inachevé", [Jordan, 1889], p.347.

¹¹⁷"On remarque [dans le mémoire de Halphen sur les courbes gauches algébriques], avec le génie de l'invention, le don si précieux de la clarté et une conscience scrupuleuse qui ne laisse jamais rien d'incomplet et d'inachevé dans les sujets qu'il traite", [Hermite, 1889], p.994.

¹¹⁸"Il semble que l'on puisse aujourd'hui distinguer, chez les mathématiciens, deux tendances d'esprit différentes. Les uns se préoccupent principalement d'élargir le champ des notions connues [...]. Les autres préfèrent rester, pour l'approfondir d'avantage, dans le domaine de notions mieux élaborées ; ils veulent en épuiser les conséquences, et s'efforcent de mettre en évidence dans la solution de chaque question les véritables éléments dont elle dépend", [Picard, 1890], p.489. In the following pages, it is made explicit that, for Picard, Halphen belongs to this second category.

¹¹⁹"This theory, which led to so many controversies, seems today to be fixed. But, one must admit,

Cette théorie, qui a donné lieu à tant de controverses, semble aujourd’hui fixée ; mais, on l’avouera, elle a subi un sort bien étrange ! Où trouver la cause de ces vicissitudes ? Trop d’imagination, peut-être, avait entraîné prématurément les géomètres dans une campagne mal préparée..

These military accents were later echoed by Halphen’s colleagues. Hermite, in his *Allocation* given at the Paris Académie after Halphen’s death, used a similar vocabulary¹²⁰:

Halphen, Faïdherbe, après tant d’autres, ont été fidèles à la double mission de l’École Polytechnique et ont continué ses glorieuses traditions. N’y a-t-il pas effectivement, dans les habitudes de l’intelligence, dans cette nature particulière que crée l’enseignement de notre grande École, une liaison normale, une concordance avec les qualités du soldat ? Une rigoureuse discipline de l’esprit prépare aux devoirs militaires, et l’on ne peut douter que les études mathématiques contribuent à former cette faculté d’abstraction indispensable au chef pour se faire une représentation intérieure, une image de l’action par laquelle il se dirige, en oubliant le danger, dans le tumulte et l’obscurité du combat.

Tellingly, this very passage would also be quoted by Poincaré in his own *Notice* on Halphen¹²¹.

In these regards, Halphen is emblematic of a larger trend in public discourses on science in France after the 1870 defeat to Prussia. As has been well-documented in recent decades, this defeat was largely attributed to France’s scientific unpreparedness, and of the lack of faith in (the usefulness of) science by the French population¹²². However dubious this explanation may seem today, it nonetheless proved to be a strong impetus for the creation of new scientific societies, among which the Société Mathématique de France¹²³:

The proliferation of calls for new structures for research and its dissemination after 1870 bears witness to the unprecedented will for change and the conviction that something could be done. The resulting innovations had two

what a strange fate it’s had! Where to find the source of these vicissitudes? Too much imagination, perhaps, prematurely led geometers into an ill-prepared campaign”, [Halphen, 1885], p.14.

¹²⁰“Halphen, Faïdherbe, after so many others, have been faithful to the double mission of the École Polytechnique, and have continued its glorious traditions. Isn’t there indeed, in the habits of intelligence, in this particular nature which the teaching of our great School creates, a normal link, a concordance with the soldier’s qualities? A rigorous discipline of the mind prepares one for military duties, and doubtlessly mathematical studies contribute to form this faculty of abstraction which proves indispensable to the chief who needs to form an interior representation, an image of the action by which he leads himself, forgetting danger, into the tumult and obscurity of combat”, [Hermite, 1889], p.995.

¹²¹ [Poincaré, 1890], p.138.

¹²² [Fox, 2012], p.234.

¹²³ [Fox, 2012], pp.236-237. On the history of the Société, see also [Gispert, 1991].

recurring characteristics. First, they served, almost without exception, to assert the primacy of specialized disciplinary expertise among the criteria for professional advancement. And secondly, they broke with the notion of a single elite of science defined by membership of the Académie and the near-oracular authority that academicians enjoyed. In both respects, the Société mathématique de France, founded in 1872, was a typical new departure. Its priority was service to an open but informed public defined by its mathematical competence rather than by academic seniority or a position in a particular institution.

Despite Chasles' instrumental role in the creation of this Société, and in the selection of topics under discussion during its first meetings, his authority as académicien quickly waned there – which is why he mainly published in the *Comptes-Rendus* and not in the *Bulletin*. For Chasles, expertise was not an epistemic virtue: in a well-grounded geometrical theory, he thought, anyone could add to the edifice, provided he was in possession of some fundamental principles. For Halphen, on the contrary, the mastery of the modern theory of algebraic functions was very much needed in order to properly contribute. It is this very expertise which he found lacking in Schubert's work, for instance. Insufficiently equipped, Schubert reasoned on things which did not exist, and he did not properly understand the meaning of the formulae he had aligned in his book. This is, at least, what Halphen thought he had uncovered via his counter-examples.

Halphen was thus very much the embodiment of a specific scientific persona, that is to say “a cultural identity that simultaneously shapes the individual in body and mind and creates a collective with a shared and recognizable physiognomy, [which lies] intermediate between the individual biography and the social institution¹²⁴”. To such a persona correspond specific epistemic virtues (expertise, rigour, thoroughness..) and practices¹²⁵ (the search for counter-examples, domains of validity..). Such cultural identities have many uses: they unify collectives of practitioners around stable sets of epistemic norms and rules, help the identification of research problems and criteria for solutions converge quickly. However, their downside is that they make communication from and to the outside world more difficult. The failure for Schubert and Halphen to come to any meaningful agreement regarding the discrepancy in their results can largely be explained by the fact that Schubert did not identify as an ‘analyst’. Schubert's normative ideal for mathematical life was a different one altogether; and Halphen's accusations of ‘having changed

¹²⁴ [Daston & Sibum, 2003], p.2. The ‘personae’ studied in the rest of this special issue range from that of the *Naturforscher* who, in the 1820s and 1830s, “drew upon tavern camaraderie” and folk music traditions to forge a unified, scientific identity; to that of the modern theoretical physicist, who like Oppenheimer, is simultaneously “theorist, teacher, administrator, and advisor”, see p.5.

¹²⁵This framework is adapted from [Daston & Galison, 2007], p.371.

nature to accommodate his formulae’ could not have swayed him.

8.2.4 Schubert’s cultural mathematics

Like Chasles’, the moral economy of Schubert’s scientific life revolved around virtues such as simplicity and unity. However, these terms meant very different things for both mathematicians. To understand these claims, it is important to first situate Schubert at the intersection of various contemporary intellectual and philosophical debates, to which we now turn. We shall argue that Schubert’s enumerative geometry can be advantageously read against the backdrop of his own philological and philosophical interests, thus casting his mathematical work, as well as his refusal to really engage with Halphen’s criticism, under a new light. In the wake of his 1879 book, and after a decade of intense work on enumerative geometry, Schubert began publishing in a wider range of journals, authoring articles and books on a wider range of subjects, until illness brought his scientific output to a halt in 1905. Among these stand out works on the philology and ethnography of numbers, elementary textbooks on algebra, and philosophical essays for a popular audience¹²⁶, which we now discuss in turn.

While numbers had been central to Schubert’s mathematical practice throughout his foray into enumerative geometry, as we showed in the previous chapter, toward the late 1880s he began approaching them from a philological and an ethnographical perspective. This venture would become a serious scientific endeavour, as Schubert quickly came to publish in the second edition of the *Anleitung zu wissenschaftlichen Beobachtungen auf Reisen* (*Guides to scientific observations on travels*) of the famous German explorer Georg von Neumayer¹²⁷ in 1888, but also to participate to congresses of German philologists¹²⁸. In fact, Schubert’s interest in philology predates his first publications on the topic, as some of his letters to Halphen show – however, even then, this interest mostly revolved on the philology of numbers and arithmetic.

Schubert’s first publication on the ethnography of numbers, a booklet published in 1887 under the title *Zählen und Zahl*, purported to give a “kulturgeschichtliche studie” of numbers and number-words. This study consists mostly in a sketch of the developmental stages through which the formations of number-words (“Zahlwortbildung”) and number-signs (“Zahlzeichenbildung”) ought to pass (see fig. below). For Schubert, “the system of numbers that we take for self-explanatory in our childhood is not something that can be

¹²⁶Schubert also wrote several books and papers on recreational mathematics, which we will not discuss here.

¹²⁷ [Schubert, 1888]. On the creation and uses of these guides, see [Monteath, 2018].

¹²⁸In [Schubert, 1905], p.5, Schubert mentions attending such a congress in 1905 for instance. The history of German philology has received increasing attention in the last decades, see for instance [Benes, 2008].

taken for self-explanatory, but rather the highest offshoot of a cultural-historical process that began when man became man, when he began to speak and write¹²⁹. By retracing the manners in which various peoples devised various ways to write and represent numbers, relying on the evidence brought by (mostly German) explorers throughout the past decades from Alexander von Humboldt to Karl von den Steinen, Schubert sets out to describe the emergence of the positional notation as a cultural process. Indeed, Schubert systematically ties signs and other textual practices to the cultural (and ecological) landscape surrounding the peoples who devised them. Religions and mythologies, but also neighbouring seas or mountains, are viewed as possible factors in the development of said number-words and number-signs. In a particularly striking passage, Schubert writes¹³⁰:

Bereits die älteste Literatur der Brahmanen, die Vedas, enthalten viele Beispiele, welche die Liebe der Inder zu übertrieben grossen Zahlen verrathen. Da ist die Rede von einem König, der seinen Reichthum zu hunderttausend Billionen Edelsteinen angiebt, von einem Affenfürsten, der seinen Feinden 10 000 Sextillionen Affen im Kampfe gegenüberstellen kann. Und in buddhistischer Zeit liest man von 24 000 Billionen Gottheiten und von 600 000 Millionen Söhnen Buddha's. [...] Auch die Griechen waren zu sehr Freunde der Natürlichen und Wahren, als das sie derartige Uebertreibungen lieben konnten Homer lässt im fünften Buche der Iliade den verwundeten Ares wie 9- oder 10 000 Männer schreien. Ein Inder würde einen Kriegsgott, der nur wie 10 000 Männer schreien kann, für lungenkrank gehalten haben.

Schubert goes on to explain why certain peoples ('*Volk*') have a need and desire for large numbers, which in turn leads them to devise ways of conveniently writing words for large numbers. To craft a word for the number ten thousand, the Greeks created the new word *μυριοι* (myrias), because they couldn't reasonably foresee a real need for many more such words. The Indians, on the contrary, yearned for ever larger numbers, and so devised a

¹²⁹"Das Ziffersystem, das wir in unserer Kindheit als etwas Selbstverständliches in uns aufnehmen, ist nichts Selbstverständliches, sondern die höchste Sprosse eines kulturgeschichtlichen Prozesses, der seinen Anfang nahm, als der Mensch zum Menschen wurde, als er nämlich anfang, zu sprechen und zu schreiben", [Schubert, 1887], p.1.

¹³⁰"Already in the oldest literature of the Brahmins, the Vedas, there are many examples that betray the love of the Indians for excessively large numbers. There is talk of a king who advanced his wealth to a hundred thousand trillion jewels, of a Monkey Prince who could confront his enemies with 10,000 sextillion monkeys in battle. And in Buddhist times one read of 24,000 trillion deities and of the 600,000 million sons of Buddha. [...] The Greeks were too friendly to the natural and the true, to love such exaggerations. Homer lets a wounded Ares scream like 9- or 10,000 men in the fifth book of the Iliad. In India, a god of war who could only scream like 10,000 men, would be considered asthmatic"; [Schubert, 1887], pp.14-15. Later in the book, Schubert insists: "Kein anderes Volk als das der Inder war dazu prädestinirt, die Null zu erfinden. Phantastisch denkend und dabei dem Formalismus huldigend, im Besitz des konsequenten aller Zahlwort-Systeme, und ausserdem übertrieben grosse Zahlen leidenschaftlich liebend, waren gerade die Inder von der Natur dazu angelegt" (see pp.31-32).

way to express them using number-words which already existed, not unlike contemporary English does with the juxtaposition of the words ‘ten’ and ‘thousand’. Such systems of number-words are ultimately classified by Schubert on a scale which goes from ‘Natürliche Zahlreichen’ (numbers being represented by collections of points or other tokens) to the ‘Prinzip des Stellenwerthes’, which corresponds to our modern way of writing the so-called Arabic numerals¹³¹.

Entwicklung der Differdschrift.

Prinzipien.	Beispiele.	Bemerkungen.																									
1) Natürliche Zahlreichen:	8 = '::::' oder IIII III oder OOOOOOOO (astetisch).	Asteten bis zur Zahl 20; Griechen und Römer in sehr alter Zeit bis 10 (Büchel, Dominus, Varian, Kerkhof).																									
2) Abbildendes Prinzip:	1887 = MDCCCLXXXVII (römisch).	Asteten; Phönizier; Römer; Grie- chen auch, aber nur bis 400 v. Chr.; Gebäude bis etwa 150 v. Chr.																									
3) Alpha- betisches Prinzip:	1887 = αααααααααα (griechisch); es be- deutet nämlich: α = 1, α = 1000, ω = 800, π = 80, ζ = 7.	Griechen von 400 v. Chr. an; Ge- bräuch von etwa 150 v. Chr. an; Ägypter, Kopten, Armenier, Perser.																									
4) Multiplika- tives Prin- zip:	1887 = 1 M 8 C 8 X 7; wo M, C, X 1008 = 1 M 8, bedeuten die Ein- 1080 = 1 M 8 X, fenzahlen tau- 180 = 1 C 8 X; sen. hundert. sehn bedeu- ten.	Chinesen ganz feinsinnig. Vermischt mit anderen Prinzipien auch sonst. z. B. Römer bei größeren Zahlen: 11 885 = XI MDCCCLXXXV.																									
5) Marken- Prinzip:	1887 = $\frac{1}{1000} \frac{8}{100} \frac{8}{10} \frac{7}{1}$, 1008 = $\frac{1}{1000} \frac{8}{100} \frac{8}{10} \frac{7}{1}$, 1080 = $\frac{1}{1000} \frac{8}{100} \frac{8}{10} \frac{7}{1}$, 180 = $\frac{1}{1000} \frac{8}{100} \frac{8}{10} \frac{7}{1}$.	Konsequenz nirgend. Die Schrift erheben bei den Zahlen von 400 an über Einer zu Hunderten, Tausenden u. s. w. durch Punkte oder Striche; Griechen bei den Zahlen von 1000 bis 10 000; Römer zumellen, auf einer aus- gegrabenen Tafel ist 11 885 600 aus- gedrückt durch $\overline{\text{X}}\overline{\text{L}}\overline{\text{X}}\overline{\text{X}}\overline{\text{X}}\overline{\text{V}}\overline{\text{D}}\overline{\text{C}}$.																									
6) Columnen- Prinzip:	<table> <tr><th></th><th>M</th><th>C</th><th>X</th><th>I</th></tr> <tr><td>1887 =</td><td>1</td><td>8</td><td>8</td><td>7</td></tr> <tr><td>1008 =</td><td>1</td><td></td><td></td><td>8</td></tr> <tr><td>1080 =</td><td>1</td><td></td><td>8</td><td></td></tr> <tr><td>180 =</td><td>1</td><td>8</td><td></td><td></td></tr> </table>		M	C	X	I	1887 =	1	8	8	7	1008 =	1			8	1080 =	1		8		180 =	1	8			In Europa im früheren Mittelalter. (Rechnen auf dem Abacus, Rechen- brett.)
	M	C	X	I																							
1887 =	1	8	8	7																							
1008 =	1			8																							
1080 =	1		8																								
180 =	1	8																									
7) Prinzip des Stellen- werthes: (In Verbindung mit dem Ge- bäude, auch das Nicht zu bezeichnen.)	Der Werth der vorstehen Ziffer wird berücksichtigt, der der vor- stehen verzeichneten u. s. w. Ein Ziffern (alpha, Beta) fügt die Stelle einer nicht vor- stehenden Zifferzahl aus. Mit den zehn Ziffern 1, 2, 3, 4, 5, 6, 7, 8, 9 und 0 läßt sich jede auch so große Zahl schreiben. Wohin die Entwicklung, Ideal erreicht.	Von indischen Brahmanen im 4. Jahrhundert n. Chr. erfunden, um 800 zu den Arabern gelangt (Alchwarizmi, latinisiert: Algo- ritmus), durch Spanien um 1000 den westeuropäischen Völkern bekannt gemacht. Kampf der Algorithmen gegen die Abzählen bis 1200. Sieg der Null unter Bernardo von Pisa. Umwälzende Veränderungen der Ziffernschrift aus der Zeit des Mittelalters. Jetzt von allen Kulturvölkern ge- kannt und geübt.																									

(620)

Truf. von C. F. Richter in Hamburg.

(520)

Druck von J. F. Richter in Hamburg.

[Schubert, 1887], p.36.

Schubert's booklet was by no means an oddity: on the contrary, by invoking the very category of ‘Kulturgeschichte’, it was intended to tie into a larger German historical and philological tradition, to which was most famously associated Moritz Cantor, but which actually goes back to Arthur Arneth. Arneth, a professor of mathematics at the Heidelberg Lyceum, also “viewed the abstraction process leading to mathematical content as being conditioned by cultural factors¹³²”. While Schubert's booklet was not intended to make a positive contribution to the philology or ethnography of numeral systems, it displays a

¹³¹This interest in the practices of denoting and writing down numbers reflects Schubert's own didactical and pedagogical concerns, exemplified in several of his publications on enumerative geometry. Indeed, Schubert constantly discussed his choices of notations, and made them so that as to make certain regularities as apparent as possible; see section 7.2.2

¹³²[Smadja, 2015], p.266. Moritz Cantor had a controversy with Zeuthen over the historiography of the Newton-Leibniz discovery of the differential calculus, discussed in [Sigurdsson, 1992]. The author contrasts Zeuthen's “elitism and pragmatic platonism” to Cantor's culturalist historiography.

real knowledge of state-of-the-art research in this field, and illustrates the wide-reaching aspects of his reflexion on numbers and symbols.

To this Arnethian inspiration, Schubert combined the cosmopolitan and Humboldtian perspective on the development of mathematics which he borrowed from Hermann Hankel¹³³. Indeed, the variety of locally- and culturally-rooted mathematics – that is to say, all the ways of writing numerals devised by peoples all over the world – was, for Schubert, to blend into an ultimately universal science, which surmounts national and regional characteristics. In a series of philosophical essays on mathematics for the newly-created journal *The Monist*, Schubert had expounded a proto-formalist philosophy of mathematics for a lay audience in which he built on his study of the ethnography of numbers¹³⁴. From his study of ‘primitive’ systems of numerations, and his understanding of the developmental stages of the path to ideal number systems, Schubert attempted to derive a philosophical account of what numbers are, as well as what strings of symbols of numbers and operations represent. “Counting a group of things”, Schubert first proposed, “is to regard the things as the same in kind and to associate ordinally, accurately, and singly with them other things. In writing, we associate with the things to be counted simple signs, like points, strokes, or circles¹³⁵”. Philological and ethnographical studies paint before our eyes the original mathematician as a crafter of signs, words, and symbols, who progressively emancipates their science from the local cultural and ecological landscape it originated from. Once such emancipation has been achieved, the mathematician’s numbers are pure cultural creation¹³⁶:

Observation of the world of actual facts, as revealed to us by our senses, can naturally lead us only to positive whole numbers, such only, and no others, being results of actual counting. All other kinds of numbers are nothing but artificial inventions of mathematicians.

How, then, are we to know how to operate on these unnatural numbers? Schubert’s solution to this question, while not completely unoriginal, borrows extensively from Hankel’s work on systems of numbers, and in particular on his principle of permanence. Schubert renamed it the “principle of no exception”, and summarized it as follows¹³⁷:

¹³³Hankel was another German mathematician at the crossroads of philology, mathematics, and the history of mathematics, see [Smadja, 2015], pp.301-307.

¹³⁴ [Schubert, 1891], [Schubert, 1892], [Schubert, 1893], [Schubert, 1894c], [Schubert, 1894b], [Schubert, 1896a], and [Schubert, 1896b]. On the role of *The Monist* in the diffusion of popular science and in particular of non-Euclidean geometry in the U.S., see [Lorenat, forthcoming].

¹³⁵ [Schubert, 1894c], p.397.

¹³⁶ [Schubert, 1894c], p.402.

¹³⁷ [Schubert, 1894b], p.567. This principle derives from George Peacock’s famous ‘principle of equivalent forms’, whose shaping had also hinged upon his author’s lasting interest in natural history. The mathematical and cultural context of Peacock’s and Hankel’s principles, however, are very different, see [Richards, 1987], [Lambert, 2013].

In the construction of arithmetic every combination of two previously defined numbers by a sign for a previously defined operation (plus, minus, times, etc.) shall be invested with meaning, even where the original definition of the operation used excludes such a combination ; and the meaning imparted is to be such that the combination considered shall obey the same formula of definition as a combination having from the outset a signification, so that the old laws of reckoning shall still hold good and may still be applied to.

Crucial for both Schubert's and Hankel's understandings of what systems of numbers are, is the latter's proof of the theorem that there can't possibly be any extension of the system of complex numbers which preserves basic algebraic laws, such as commutativity. Hamilton's quaternions, for instance, are an extension of complex numbers in which the order of multiplication matters. For Schubert, this shows that "the building up of arithmetic is thus completed", and that this science has reached absolute perfection -because it derives from a single, "monistic principle"¹³⁸.

In fact, this recourse to Hankel's 'principle of permanence' had already been used by Schubert in a pedagogical context. For most of his life, Schubert worked as a *Gymnasium* teacher. From 1876 to 1908, he was *ordentlicher Oberlehrer* at the *Gelehrtenschule des Johanneums* in Hamburg. While in Hamburg, Schubert also lectured two to four hours a week at the *Akademisches Gymnasium*, to a mixed audience composed largely of fellow *Gymnasium* teachers. Between 1883 and 1908, Schubert published some twelve textbooks and exercise books for high-school students, making pedagogical and didactical texts a significant component of his scientific output. The so-called "Sammlung Schubert", a series of textbooks published by Göschenverlag (a company which would be bought by De Gruyter in 1919) proved to be popular with teachers all over Germany¹³⁹. It is in one of the first of these textbooks that Schubert first explicitly presented his philosophy of numbers and algebra relying on Hankel's 'principle of permanence'¹⁴⁰. On Klein's command, Schubert wrote the very first chapter of the first book of Klein's and Meyer's *Encyklopädie*, on the foundations of arithmetic¹⁴¹. This chapter, which relied heavily on Schubert's understanding of Hankel's principle of permanence, elicited harsh criticism from Frege and Peano¹⁴².

¹³⁸ [Schubert, 1894b], pp.578-579.

¹³⁹ A clear view of Schubert's authorial career is still very much lacking. According to Burau's recension of Schubert's publications, reproduced in [Schubert, 1979], pp.18-21, there were 6 volumes in the *Sammlung Schubert*, with topics ranging from elementary arithmetic and algebra to combinatorics and logarithms. On these texts, see also [Remmert & Schneider, 2010]; pp.89-93.

¹⁴⁰ [Schubert, 1885], p.27.

¹⁴¹ [Schubert, 1898].

¹⁴² Hankel's principle of permanence, as well as Schubert's adaptation thereof and the opposition to it by Peano and Frege, are discussed in [Detlefsen, 2005].

The connection between Schubert's philosophical views and geometrical practice would appear most clearly in Schubert's rebuttal of the spiritualist theses of Johann Zöllner (1834-1882). A German astrophysicist, Zöllner argued toward the end of his life that the mathematics of four-dimensional spaces and its physical interpretation form a rational and scientific basis for spiritualism, that is to say the study of the spirits of the dead¹⁴³. Rejecting any attempt to use pure mathematics to naturalize such phenomena, Schubert insisted on the purely artificial character of the numbers the mathematician freely crafts in the course of their work. Dimensions are one such example of artificial numbers¹⁴⁴:

Is it permissible to extend the notion of space by the introduction of point-aggregates of more than three dimensions? [...] In mathematics, in fact, the extension of any notion is admissible, provided such extension does not lead to contradictions with itself or with results which are well established. Whether such extensions are necessary, justifiable, or important for the advancement of science is a different question. It must be admitted, therefore, that the mathematician is justified in the extension of the notion of space as a point-aggregate of three dimensions, and in the introduction of space or point-aggregates of more than three dimensions, and in the employment of them as means of research.

Here, Schubert's theses echo once more those of Hankel's, who had famously claimed that "number is no longer an object, a substance which exists outside the thinking subject and the objects giving rise to it, an independent principle, as it was for instance for the Pythagoreans. [...] Only that counts as impossible for the mathematician which is logically impossible, i.e. that which contradicts itself¹⁴⁵". For Schubert, the mathematician wields symbols and concepts with no intrinsic relation with natural objects whatsoever. The sole rules of such an activity are that it should preserve past discoveries, and introduce no new contradiction. This is not to say that anything goes: mathematics, for Schubert, is

¹⁴³ [Sawicki, 2016], pp.299-310. Zöllner's theses were discussed by a range of philosophers, from Nietzsche to Helmholtz, and even caused a scandal in Leipzig in the early 1870s.

¹⁴⁴ [Schubert, 1893], p.410.

¹⁴⁵"Ein Ding, eine Substanz, die selbstständig ausserhalb des denkenden Subjectes und der sie veranlassenden Objecte existirte, ein selbstständiges Princip, wie etwa bei den Pythagoreern, ist die Zahl heute nicht mehr. Die Frage von der Existenz kann daher nur auf das denkende Subject oder die gedachten Objecte, deren Beziehungen die Zahlen darstellen, bezogen werden. Als unmöglich gilt dem Mathematiker streng genommen nur das, was logisch unmöglich ist, d. h. sich selbst widerspricht", [Hankel, 1867], p.6, quoted and translated in [Epple, 2003], pp.293-294. The similarities between Schubert's and Hankel's mathematical practices deserve to be explored further. Like Schubert, Hankel in his *Vorlesungen über die complexen Zahlen* insists in many instances on the role of content-free signs (*Zeichen*), which can be used to refer to spatial objects such as points, rays, and planes, and manipulated using the formal rules of arithmetic. For these reasons, Hankel seems a much better candidate than Poncelet to understand the genesis of the principle of conversation of number.

always located on a path of progression, of which the end goal is the “[unification] under a high point of view of theories heretofore regarded as different¹⁴⁶”.

To view Schubert as a philosopher of mathematics is bound to lead to disappointments: his writings do not have the finesse and argumentative solidity to withstand assaults from the likes of Frege, who harshly dismantled his view of number in an ironic review¹⁴⁷. There is, however, much to gain from reading Schubert’s texts as depicting a regulative ideal of mathematical activity, one that already ruled his geometrical research. Indeed, Schubert’s enumerative geometry, as the name suggests, is a science of the numbers of geometry. In fact, we saw that he used expressions such as ‘geometrical numbers’ to refer to some of the symbols of his *Kalkül*. Such geometrical numbers, like dimension-numbers, are purely human creations; and their very definition rests on the postulation of a principle of conservation as well. They are bound by no rule other than internal coherence: in fact, the first of these numbers, that is to say the numbers of ‘main elements’ satisfying fundamental conditions, had been said by Schubert to be axiomatic. Lastly, the use of these numbers was aimed toward the obtention of unitary knowledge by way of *Produktensätze*, which gather in one formula the solution to all enumerative problems pertaining to a certain figure.

We can now understand why Halphen’s criticism failed to elicit a strong reaction from Schubert. Halphen accused Schubert of altering Nature, but this accusation could not sway the German geometer, for whom mathematicians were free to craft symbols and numbers as they saw fit, as long as no contradictions were thus introduced, in the hope of finding a path to a unitary formulation of the solution to a geometrical problem¹⁴⁸. The rhetorical recourse to Nature, whether in the form of Chasles’ account of geometrical practice as the search for fundamental properties from which theories can be effortlessly derived, or of Halphen’s description of the Analyst using his expert training and tools to track the traps and counter-examples which lay in our imprecise intuition of Geometry, was ultimately meaningless for Schubert. A clash of geometers who shared little understanding of what mathematical activity consists in and what its goals are, the Halphen-Schubert controversy was one in which constructive dialogue nigh impossible.

¹⁴⁶ [Schubert, 1896a], p.301.

¹⁴⁷ Frege’s review was first published in Jena in 1899, see [Frege, 1984], pp.249-272.

¹⁴⁸ Some have even likened Schubert’s mathematics to “the expression inside the realm of pure mathematics of the mindset of the contemporary industrialisation”, see [Schappacher, 2007], §2.

8.3 Zeuthen versus Study: A matter of perspective?

8.3.1 Study's 1885 *Habilitationsschrift*: Chasles' formula vindicated?

The public discussion of Chasles' theory of characteristics and of the $\alpha\mu + \beta\nu$ formula paused in the wake of the Schubert-Halphen controversy. Schubert went on to publish several other papers in the vein of his enumerative *Kalkül* (for instance generalizing the enumerative geometry of conics and quadrics in n -dimensional space¹⁴⁹), showing little interest in adapting his methods and results to Halphen's criticisms. Halphen had moved on to other research interests, from algebraic geometry to differential invariants and equations. Zeuthen had not taken part publicly in any of these discussions, and he published on topics in algebraic geometry which did not directly concern enumerative methods. Nevertheless, he made frequent use of the principle of correspondence in its generalized forms. And yet, the state of the theory of characteristics did not satisfy Felix Klein, who had maintained a friendly and professional relationship with Schubert, frequently corresponding and publishing several of his papers in the *Mathematische Annalen*. Klein was ideally placed to know that the conflict hadn't been fully resolved; and so he decided to direct one of his promising students in Leipzig toward writing an *Habilitationsschrift* on the difficult terrain of enumerative geometry. This student, as mentioned earlier in this chapter, was none other than Eduard Study¹⁵⁰.

Upon receiving his assignment in July 1884, Study admitted that he knew very little about enumerative geometry, and wondered whether he would be able to find anything new to contribute¹⁵¹. A month into his study of Schubert's enumerative geometry, Study felt even more helpless: the principle of conservation of the number as employed by Schubert seemed to him ill-founded, and the algebraic work required to rigorously ground its usage far beyond what he was able to do. In a letter to Klein, Study confessed that he did not have the first idea on how to lift the difficulties he had faced in reading Schubert's

¹⁴⁹ [Schubert, 1886], [Schubert, 1894a].

¹⁵⁰ Study has been the subject of little interest in recent years, with one notable exception being [Hartwich, 2005], on which we shall rely heavily in the rest of this chapter. A short analysis of Study's philosophy of mathematics and space is provided in [Gray, 2008], pp.293-296.

¹⁵¹ "Ihre Themenstellung verstehe ich zwar noch nicht ganz – aber Sie haben wohl die Freundlichkeit, mir die Punkte, die Sie besonders behandelt wuenschen, naeher zu bezeichnen – auch kenne ich ja die abzaehlende Geometrie noch viel zu wenig. Ob ich freilich Bemerkungen finden werde, die sich nicht Jedem, der die Sache studirt, von selbst darbieten, weiss ich nicht. Ich ergreife aber mit Freuden die Gelegenheit, meinen Gesichtskreis in dieser Hinsicht zu erweitern – es ist fuer den Geometer gewiss noethig, sich mit diesem Calcul vertraut zu machen und eine Meinung darueber zu bilden.", Nachlass Klein, Letter Study to Klein 1221, dated July 24th 1884, quoted in [Hartwich, 2005], p.53. Study was described, in many contemporary accounts, as eccentric and opiniated. In particular, his correspondence with Klein often borders on confrontational.

book¹⁵². As a general algebraic treatment of Schubert’s enumerative geometry seemed out of reach, Study decided to focus on Chasles’ $\alpha\mu + \beta\nu$ formula. Here as well, at first, he thought the subject to be too slippery: in Study’s words, “the problem lacked an exact formulation¹⁵³”. To find such a formulation, and thereby to rigorously prove Chasles’ theorem, such was the task to which Study devoted the following months and his *Habilitationsschrift*¹⁵⁴.

In July 1885, Klein deemed Study’s work to be worth the *Habilitation*. Study’s dissertation was published, first independently by Teubner, then the following year in Klein’s *Mathematische Annalen*¹⁵⁵, with few and slight modifications. The result of Study’s efforts was a new proof of Chasles’ $\alpha\mu + \beta\nu$ theorem, and a new response to Halphen’s criticisms. Partly because of the surprising nature of this conclusion, Klein sent Study on a scientific trip to Paris at the end of March 1886, alongside Hilbert. Among other things, Klein expected Study to go there to discuss his results with Halphen.

This trip had a broader purpose: Klein viewed Paris as a “beehive of scientific activity, particularly among the young mathematicians¹⁵⁶”. There, Study and Hilbert introduced themselves to Poincaré, Jordan, Picard, and Hermite; but also Halphen, with whom Study had personal discussions regarding his new proof of the $\alpha\mu + \beta\nu$ formula, and with De Jonquières, who had his *Habilitationsschrift* translated into French. While Hilbert wrote back to Klein to describe the great warmth with which he had been welcomed¹⁵⁷, Study felt that Darboux and Halphen stood out in that regard, and that they “looked down upon” the young German mathematicians¹⁵⁸. With respect to the level of the mathematical discussions they were able to have in Paris, both Study and Hilbert were disappointed: the fact that most of the French mathematicians they engaged with felt the need to speak German made scientific communication difficult. That, and the fact that Study and Hilbert were yet to prove their mathematical ability, meant that the exchanges that Klein had hoped for did not come to full fruition. Study returned to Germany at the end of April, that is to say two months earlier than Hilbert, unchanged in his mathematical

¹⁵²“Und hier liegen in der That Schwierigkeiten, die aber so gross sind, dass ich bekenne, nicht einmal einen Begriff davon zu haben, wie man sie heben koennte”, Nachlass Klein, Letter Study to Klein 1222, dated August 21st 1884, quoted in [Hartwich, 2005], pp.53-54. Study’s opposition to Schubert’s principle would be long-lasting, as we shall see later in this chapter.

¹⁵³“Es fehlt eben ueberall die exakte Formulierung der Fragestellung”, Nachlass Klein, Letter Study to Klein 1222, dated August 21st 1884, quoted in [Hartwich, 2005], p.55.

¹⁵⁴ [Study, 1885]. A brief appendix to this text shows that Study’s proof extends to Cremona’s formula, for systems of level 2 and 3.

¹⁵⁵ [Study, 1886b], [Study, 1886a].

¹⁵⁶ [Reid, 1986], p.20.

¹⁵⁷ [Reid, 1986], p.23.

¹⁵⁸“Darboux und Halphen sind die Einzigen, die nicht freundlich mit uns sind, und uns von oben herunter behandeln”, Nachlass Klein, Letter Study to Klein 1280, dated April 15th 1886, quoted in [Hartwich, 2005], p.62.

views.

Study's visit was particularly disappointing as regards his exchanges with Halphen. Study visited him twice within a few days of arriving in Paris, and the two men had "a conversation of almost three hours, mostly about the problem of characteristics¹⁵⁹". Study would have several other exchanges with Halphen, until the very end of his Parisian stay. In fact, Halphen sent letters to Study's address in Paris until April 28th, referencing their earlier in-person conversations. Despite these frequent meetings, however, neither Study nor Halphen budged. Study spent a part of his stay reading a paper given to him by Halphen, in which the latter "solved the Charakteristikenprobleme for five 4-fold extended systems of conics [that is to say, four conditions] by a kind of *Charakteristikenformel*¹⁶⁰". Once he had read this paper, however, Study was unmoved, reporting back to Klein that "[Halphen's work] is quite pretty, but he didn't know how to do the same things with his own methods", and "that it was better to do something completely new¹⁶¹". Meanwhile, Halphen "persisted in finding nothing useful or new in [Study's] interpretation of Chasles' theory¹⁶²".

Halphen never commented publicly on Study's papers: perhaps, as Zeuthen feared, "after killing the theorem he did not deign to fight its specter¹⁶³". But more profoundly, as we shall show in the rest of this chapter, Halphen and Study were pursuing very different kinds of mathematics, both at the level of the concepts and methods they used, and at the level of the epistemic task they put at the heart of their mathematical practice. Meanwhile, Schubert wrote a brief and rather uninformative review of Study's *Mathematische*

¹⁵⁹"Vorgestern war ich ein zweites Mal bei Halphen und hatte da eine fast drei Stunden dauernde Unterredung mit ihm, meistens ueber das Charakteristikenproblem, mit der ich recht zufrieden bin", Nachlass Klein, Letter Study to Klein 1278, dated April 3rd 1886, quoted in [Hartwich, 2005], pp.60-61.

¹⁶⁰"Zum Schluss gab er [Halphen] mir, mit freundschaftlicher Widmung, eine Arbeit, in welcher er das in seinem Sinne gefasste Charakteristikenproblem fuer fuenf 4-fach ausgedehnte Kegelschnittssysteme durch ein Art Charakteristikenformel allgemein loest", Nachlass Klein, Letter Study to Klein 1278, dated April 3rd 1886, quoted in [Hartwich, 2005], p.61.

¹⁶¹"Die Arbeit, die Halphen mir gab, habe ich nunmehr gelesen. Sie ist recht huebsch, leider weiss ich noch nicht, wie ich dieselben Sachen mit meinen Methoden machen soll. Ich werde mich aber nicht sehr damit plagen, ich denke, es ist besser, man macht etwas ganz neues", Nachlass Klein, Letter Study to Klein 1280, dated April 15th 1886, quoted in [Hartwich, 2005], p.62.

¹⁶²"Quant au sujet même qui vous intéresse, je crains fort de voir encore mon opinion vous déplaire. Sans parler de vos démonstrations, que je n'ai pas eu le loisir d'étudier, je persister à ne trouver rien d'utile ni rien de neuf dans votre interprétation de la théorie de Chasles. Je m'étonne même, connaissant bien maintenant votre idée, de me trouver avec vous en contradiction sur ce point", Nachlass Engel NE090416, Letter Halphen to Study, dated April 22nd 1886. Note that Halphen is referring to his previous discussions with Study, mentioned in Study's letters to Klein. Only two brief letters from Halphen to Study are extant, the other written on April 28th, right before Study's departure. In it, Halphen discusses a technical point of his criticism of Clebsch's proof of the $\alpha\mu + \beta\nu$ formula, and refers to a discussion they had in person earlier that same day.

¹⁶³"J'ai peur qu'après avoir tué le théorème vous dédaigniez d'en combattre le spectre", Letter Ms 5624 246, Zeuthen to Halphen, dated August 19th 1886.

Annalen entries for the *Jahrbuch*¹⁶⁴, but did not substantially engage with their content, or with Study's argument against his own mathematical practice.

8.3.2 The motivated creation of mathematical concepts

In the first section of his 1886 paper published in Klein's *Mathematische Annalen*¹⁶⁵, Study gave a short historical account of the previous attempts at solving the problem of enumerating the conics in a plane satisfying five given conditions (and the equivalent problem for surfaces), whereby he positioned the novelty of his own approach¹⁶⁶. Study, like many before him – Schubert included –, emphasized the analogy between Chasles' formula and Bézout's theorem for the number of intersections of (algebraic) curves. At the level of a general (and insufficiently precise) statement of the problem, Study explains, the problem to which Chasles' formula answers is that of giving the intersection of several systems of curves (or surfaces) whose dimensions are complementary. Because all curves in a given plane form a variety¹⁶⁷ (*Mannigfaltigkeit*), the solution to this problem can indeed be understood as a generalization of Bézout's theorem: finding how many conic sections in a common plane satisfy five given conditions is equivalent to computing the number of intersections of five four-fold systems in the variety of planar conics – that is to say, in modern parlance, the intersections of five hypersurfaces in a five-dimensional projective space. For Study, this is exactly what De Jonquières' initial solution to the problem consisted in: to characterize systems of curves by a single number (its index, which plays the same role as the order in the theory of algebraic curves), whose products yield the number of intersections being sought – as in the usual form of Bézout's theorem. However, Study insists, this is a flawed solution¹⁶⁸:

Dieser Standpunkt zeigt nun offenbar den Bézout'schen Sätzen gegenüber nichts eigentlich Neues; die Theorie der Systeme algebraischer Curven selbst aber lässt er in einem falschen Lichte erscheinen, wie man schon daraus erkennen kann, dass man von hier zu Unterscheidungen gelangt, die in der Natur

¹⁶⁴ [Schubert, 1889]

¹⁶⁵ Since the differences between Study's 1885 *Habilitationsschrift* and his 1886 couple of papers are slim, we elect to give references to the latter, except at a few instances where differences are taken into account.

¹⁶⁶ [Study, 1886b], pp.58-64.

¹⁶⁷ To be more precise, for Study, the key factor is that each curve in the plane can be bi-univocally mapped to a point of a certain variety.

¹⁶⁸ "This point of view obviously shows nothing really new with regard to Bézout's theorems; but it casts a false light on the theory of systems of algebraic curves, as can be seen from the fact that it leads to distinctions which are not founded in the nature of these figures. This rests on the distinction of the point as spatial element. A curve, however, is not merely the locus of points; and one will not be entitled to view a system of conic sections only as a system of curves of the second order, but also of the second class, that is to say as a system of loci of straight lines", [Study, 1886b], p.59.

jener Gebilde nicht begründet sind.

Es beruht dies auf der Auszeichnung des Punktes als Raumelement. Eine Curve ist aber nicht allein Ort von Punkten; und man wird etwa ein Kegelschnittssystem nicht nur als ein System von Curven zweiter *Ordnung*, also von Punktcurven betrachten dürfen, sondern man wird dasselbe ebensowohl als ein System von Curven zweiter *Classe* aufzufassen haben, d. h. als ein System von Oertern für gerade Linien.

To view conics only as loci of points, Study continues, means that certain conditions are viewed as being satisfied in cases which should be rejected: for instance, all line-pairs of a system ‘touch a given curve’ if not also viewed as loci of straight (tangent) lines. Thus, Study explains, under Chasles’ superior point of view (*Standpunkt*), two curves are viewed as identical if they not only coincide point by point, but also tangent by tangent.

The key feature of Study’s account is that he presents both definitions of the identity between two curves (that is to say, both ways to form a variety of planar conic sections) as “acts of arbitrariness¹⁶⁹” (*Act der Willkür*); and the “punctum saliens” of the problem of characteristics, for Study, is precisely to find such an arbitrary definition which does not lead to the same sorts of problems as De Jonquières’. However, neither De Jonquières’ nor Chasles’ points of view were precisely defined, especially with respect to which degenerate conics ought to pass as real solutions to enumerative problems. For Study: “these older authors”, he assesses, “were guided, in their separation between different kinds of solutions, by their correct tact, rather than by sharp concepts¹⁷⁰”. This proved to be a crucial shortcoming of their theories: the first authors such as Clebsch, who attempted to prove the $\alpha\mu + \beta\nu$ formula, all ever so slightly shifted the initial meaning of the problem while trying to reformulate it. This is what happened, Study contends, when Clebsch put forth his definitions of ‘mobile conics’ and real solutions; hence leading to a necessarily flawed proof. This, Study continues, is why some thought Chasles’ formula to be outright false. Halphen’s memoirs are then read by Study as proving that Clebsch’s formulation of Chasles’ problem was not an adequate basis for the formula, and that to view ‘mobile solutions’ as ‘real solutions’ was not the right (arbitrary) definition to adopt.

Thus, after recounting the history of this problem, Study sets out to give a new formulation of it¹⁷¹. To do so, is to make an arbitrary choice, by way of selecting some properties of systems of conics which are essential to the validity of the theorem at hand

¹⁶⁹ [Study, 1886b], p.59.

¹⁷⁰“Wir haben schon angedeutet, dass die älteren Autoren sich bei ihrer Abscheidung gewisser Arten von Lösungen mehr durch einen richtigen Tact, als durch scharfe Begriffe hatten leiten lassen”, [Study, 1886b], p.61.

¹⁷¹In Study’s *Habilitationsschrift*, the second section is actually named “Ueber eine Formulirung des Problems” in the table of contents, [Study, 1885], ‘Inhalt’. In the 1886 paper, this section does not have a title, see [Study, 1886b], pp.64-68.

(namely, Chasles' formula), and to make them into definitions for the formation of a variety in which the problem can be aptly translated. Such an operation, Study claims, allows a move from the intuition of particular cases to the logical treatment of the general claim¹⁷²:

Will man aber eine in besonderen Fällen anschauungsmässig behandelte Aufgabe in voller Allgemeinheit erledigen, so muss man von der Anschauung zu Begriffen übergehen und logische Deductionen an Stelle der Berufung auf den Augenschein setzen, die nicht selten auch im einzelnen Falle, wo sie auszureichen scheint, doch nur das stille Bekenntniss enthält, dass man sich der wahren Gründe nicht vollkommen klar bewusst ist. Man hat zu diesem Behufe diejenigen Eigenschaften der betreffenden Gebilde, deren Vorhandensein man als die nothwendige und ausreichende Bedingung ansieht für das Bestehen jener anderen Eigenschaften, welche den geometrischen Satz vorstellen, von den übrigen zu trennen, welche man als Consequenzen jener oder nur als durch die zufällige Erscheinungsform des allgemeinen Satzes bedingt betrachtet. Die ersteren muss man zum Range von Definitionen erheben und zur Grundlage der Beweisführung machen. Diese Operation vollzieht Jeder, der eine Verallgemeinerung vornimmt, mit Absicht oder auch unwillkürlich. [...] Es kann eintreten, dass der Schnitt, welcher Wesentliches von Zufälligem trennen soll, an der falschen Stelle geführt wird, und dass die angestrebte Schärfe der Definition doch nicht ganz erreicht wird.

The image of mathematical life contained in this paragraph differs from that present in Schubert's philosophical musings, or Halphen's self-fashioning. Like Schubert, Study emphasizes the freedom at the heart of mathematical conceptualization; however, this freedom lies in the location of a cut, a separation between what is taken to be essential, and what is to be derivative. For Study, the creation of mathematical concepts, while free, must be motivated – by the existence of a theorem deemed important to preserve, or by the need to communicate with others by creating concepts that do not directly

¹⁷²“But if one wants to solve in full generality a problem which in particular cases has been treated intuitively, one must move from intuition to concepts, and apply logical deductions instead of appeals to the visual which, even in the isolated cases where they seem sufficient, are but the silent confession that one is not fully aware of the true reasons. For this purpose, it is necessary to separate those properties of the figures in question, the presence of which is considered to be a necessary and sufficient condition for the existence of those other properties which constitute the geometrical theorem, from those other properties which are considered to be consequences of those first properties, or which are only conditional to the accidental appearance of the general theorem. The former must be elevated to the rank of definitions, and be made the basis for the proof. This operation is carried out by anyone who makes a generalization, intentionally or involuntarily. [...] It may happen that the cut which separates the essential from the accidental is made in the wrong place, and that the desired sharpness of the definition is not entirely achieved after all”, [Study, 1886b], pp.61-62.

contradict statements in use elsewhere. In other words, the ‘general’ is not a property of a proposition, but something to be constructed through the selection of a property necessary for the validity of a theorem which one wants to make valid.

Furthermore, to this free creation of concepts must always be attached a precise delimitation of their extension. Study, throughout his mathematical and philosophical career, was no ally to the defenders of axiomatics¹⁷³, as he made clear for instance in a later text on the line-geometry of space¹⁷⁴:

Die Systeme von mathematischen Begriffen sind unsere Geschöpfe. Innerhalb der Schranken, die uns durch die Denkgesetze gezogen sind, können wir sie entstehen und vergehen lassen, nach freiem Belieben. Weil wir aber – übrigens durchaus nicht in *jeder* Beziehung – ein kritisches Geschlecht sind, so entäufeln wir uns freiwillig eines großen Teils unserer Schöpferkraft; und wir müssen das thun, müssen eine nicht leicht zu nehmende *Motivierung* neuer Begriffe fordern, wenn wir den Thatsachen nicht Gewalt anthun, wenn wir einander verstehen und nicht der Laune jedes Beliebigen Thür und Thor öffnen wollen. – Indessen kann auch begründete Vorsicht zu weit gehen. Wir suchen mit Überliefertem auch wohl da noch auszukommen, wo Bedürfnisse neuer Art vorliegen und neue Gestalten in Erscheinung treten sollten; und wir lassen vielleicht gar das Geschöpf zum Herren werden.

Furthermore, with Halphen and against Schubert, Study emphasizes rigor and precision as the core epistemic values of the mathematician, mainly with the goal of reaching an abstract form of generality. From his first reading of the *Kalkül* to the later stages of his career, Study would constantly criticize Schubert’s blind symbolism, and his reliance on the principle of the conservation of numbers¹⁷⁵. This principle, for Study, was emblematic of the “untenable state of Geometry”, wherein “in countless cases, the objects under investigation were so unclearly explained that the meaning [Sinn] of individual concepts is to be guessed from the assertions made about them, where differences of opinion can

¹⁷³Gray describes Study’s accounts of axiomatics in geometry as “reactionary”, [Gray, 2008], p.294.

¹⁷⁴“Systems of mathematical concepts are our own creations. Within the barriers that are defined by the laws of thought, we can let them come into existence and pass away at will. However, since we are a critical race – although not in every respect – we willingly divest ourselves of a large part of our creative power; and we must do that, we must demand a motivation for new concepts that is not easy to take, if we are not to do violence to the facts, if we are to understand each other, and not to capriciously open any arbitrary door and gate. Meanwhile, reasonable caution can also go too far. We also seek to get by with existing things when demands of a new sort arise and new forms might come to light, and we may even perhaps allow the creation to become the Lord”, [Study, 1902], p.99.

¹⁷⁵In particular, Study gave a paper during the 1904 International Congress of Mathematicians in Heidelberg attacking Schubert’s principle, which was later published in *Archiv der Mathematik und Physik* with minor changes, [Study, 1905a], [Study, 1905b]. He came back to this issue a decade later with renewed criticisms toward the principle, see [Study, 1916].

naturally arise¹⁷⁶. Of course, through such claims, Study was alluding to, among other things, the historical disagreements over the theory of characteristics, wherein an absence of precision had led to such divergences in opinion.

More specifically in the case of Schubert's principle of conservation, Study lamented the lack of a precise concept of geometrical figure. Of course, as we showed in the previous chapter, Schubert did propose a detailed concept of figure; but not in a sense that could satisfy Study's demands. On a mathematical level, for Study, a 'general' concept of figure can only be provided by considering figures which form a variety (or a "closed algebraic continuum"¹⁷⁷). On a philosophical level, however, what Study demanded was a language free of any ambiguity. Toward the end of his first paper against Schubert's principle, Study considers the two nominal groups 'two points' (*zwei Punkte*) and 'two different points' (*zwei verschiedene Punkte*). These two expressions ought to carry different meanings, and yet, without the recourse to a precise topological framework, the difference between them remained ambiguous to Study's eyes.

And so Study ended his communication with a call for all geometers to join forces in renewing and purifying the language of geometry, so that precision and unambiguity may be collectively achieved¹⁷⁸. This collective work, for Study, was of vital importance for geometry at large, and the anxious accents of this paper reveal how crucial he felt was the task at hand¹⁷⁹. Against Schubert's concept-free symbolism, or Chasles' intuitive geometry, but also against Halphen's restrictive reliance on a natural, yet possibly irregular, theory of conics, Study reconciled arbitrary definitions with an intransigent emphasis

¹⁷⁶"Die Geometrie ist größtenteils heute noch weit entfernt von der Präzision, die bei rein analytischen Untersuchungen, dank besonders dem Einfluß von Weierstraß, nunmehr allgemein als unerlässlich betrachtet wird, und, was schlimmer ist, es scheint in weiten Kreisen auch gar kein Gefühl für das Unhaltbare des gegenwärtigen Zustandes vorhanden zu sein. In unzähligen Fällen werden die Objekte geometrischer Untersuchungen so undeutlich erklärt, daß man den Sinn der einzelnen Begriffe aus den darüber aufgestellten Behauptungen zu erraten suchen muß, wobei natürlich Meinungsverschiedenheiten entstehen können", [Study, 1905a], p.388.

¹⁷⁷"'Gemeint' sein können nur solche Figuren, deren Mannigfaltigkeit einim Sinne von G. Cantor abgeschlossenes algebraisches Kontinuum bildet, d. h. eindeutig umkehrbar abgebildet werden kann auf eine algebraische Punktmannigfaltigkeit, die in dem projektiven Punktkontinuum irgend eines höheren Raumes verläuft", [Study, 1905a], p.391.

¹⁷⁸"Die Mitwirkung Vieler aber wird zum Reinigungswerk erforderlich sein. Vor allem wird die Kritik sich gegenwärtig halten müssen, daß Präzision in geometricis nicht in perpetuum wie eine Nebensache behandelt werden darf", [Study, 1905a], p.395.

¹⁷⁹Study's plea is not entirely different from Frege's outrage at the lack of answers to fundamental questions about arithmetic expressed in his 1884 *Grundlagen der Arithmetik*: "When we ask someone what the number one is, or what the symbol 1 means, we get as a rule the answer 'Why, a thing'. [...] Questions like these catch even mathematicians for that matter, or most of them, unprepared with any satisfactory answer. Yet is it not a scandal that our science should be unclear about the first and foremost among its objects, and one which is apparently so simple?", [Frege, 1960], pp.xiii-xiv. Earlier already, Frege had insisted in the *Begriffsschrift* on the need to "break the domination of the word over the human spirit by laying bare the misconceptions that through the use of language often almost unavoidably arise concerning the relations between concepts", [Frege, 1967], p.7.

on the importance of the mathematician's duty to precisely measure and delineate the extension of the concepts they freely produced¹⁸⁰.

To present the technical content of the entirety of Study's dissertation would require preliminary discussions of contemporary mathematical work on the theory of (ternary-quadratic) forms, and thus take us beyond the scope of this dissertation. For our purposes, it suffices to say that Study, after Cayley, but also after Segre and Veronese, elects to work in the five-dimensional variety whose points correspond to quadratic ternary forms, so that the topological properties of this variety may be defined in terms of the covariant formations of said quadratic forms. This was not entirely new: remember that Cayley had, in 1868, introduced a "quasi-geometrical" interpretation of the theory of characteristics, by considering points in a five-dimensional space whose coordinates were the coefficients of a form defining a conic section in the plane. The totality of all degenerate conic sections could then be viewed as a special locus in this space, which in fact corresponds to what is now called the 'Veronese Surface'¹⁸¹. While Cayley himself had not really pursued this line of inquiry, and in particular had not considered applying the results and methods of algebraic geometry to these objects, his work would be read with interest by the Italian geometers Giuseppe Veronese and Corrado Segre. Both Veronese and Segre would pursue much further the study of the geometrical properties of this (projective) space – all three authors being referenced by Study in his articles¹⁸². In particular, they studied the intersections that the surface of degenerate conics has with certain sheaves or special loci, relying on Hesse's "Uebertragungsprincip"¹⁸³ (*principle of transfer*). Study does not refer to Klein's work in his thesis; in particular, he does not seem to be intentionally engaging with the latter's 1872 *Erlanger Programm*, which had not gained traction on a global scale yet¹⁸⁴. However, through the intermediary of Segre, one of the few mathematicians familiar with the *Programm* in the early 1880s¹⁸⁵, Study was nudged toward working in a framework not entirely removed from what Klein had advocated for. Lastly, it is worth noting that, once again, Chasles' $\alpha\mu + \beta\nu$ formula for conic sections would gain yet another meaning when recast in a different 'mathematical laboratory': this time, as an

¹⁸⁰This last requisit puts Study outside the camp of the ideal modernist as defined by Mehrtens or Gray: a staunch realist, even in the face of Einstein's introduction of non-Euclidean geometries in physics, Study railed against axiomatizers who never inquired about what objects fell under their definitions, whereas others such as Hilbert equated the coherence of an axiomatics to the existence of its objects; see [Hartwich, 2005], pp.130-159; [Gray, 2008], pp.295-296.

¹⁸¹See 6.3.1.

¹⁸²[Study, 1886b], p.71.

¹⁸³This principle was used by Hesse to 'transfer' the projective geometry of a conic onto the projective geometry of a straight line, by means of a simple geometrical construction; see [Hawkins, 1988].

¹⁸⁴[Hawkins, 1984], pp.449-450, contends that "Klein [did not exert] any significant influence upon [Study]" in Leipzig, and that the latter became interested in the *Erlanger Programm* only much later, under the influence of Lie.

¹⁸⁵[Hawkins, 1984], p.452.

intersection formula proper, in an abstract projective space.

8.3.3 Zeuthen's 'points de vue' and Poincaré's 'conventions'

Study's recasting of the history of Chasles' problem as that of the successive definitions of various arbitrary varieties of planar conics was in fact not entirely new. Unbeknownst to him, Zeuthen had already suggested a similar assessment of the relative merits of the enumerative theories of De Jonquières, Chasles, and Halphen, in a private letter to the latter written right after Halphen's first counter-examples to the $\alpha\mu + \beta\nu$ formula had been circulated. In a remarkable convergence of terminologies, Zeuthen had also proposed to view these theories not as increasing in generality, but rather as expressing different 'points of view'¹⁸⁶:

1° Le premier point de vue est celui de Bischoff-Jonquières. Les coniques (courbes) sont ici définies exclusivement par leurs propriétés ponctuelles. Une tangente est une droite qui rencontre les coniques en deux points coïncidents, les cas de coniques ordinaires avec leurs tangentes se présentent donc à côté de ceux où la conique se réduit à une droite double, et où par conséquent – du point de vue actuel – il faut regarder la conique comme tangente à toute droite (courbe) du plan. Le nombre total de coniques d'un système satisfaisant à une condition donnée sera égal au produit $\alpha\mu$ de deux nombres qui dépendent l'un du système, l'autre de la condition. Ce point de vue a l'avantage d'être simple et parfaitement clair. La distinction du 'général' et du 'particulier' qui cause des difficultés au point de vue suivant, se fait donc ici immédiatement. Mais à cet avantage se joignent plusieurs faiblesses. Premièrement, il s'altère par les application du principe de dualité, qui conduit à la définition des coniques par leurs propriétés tangentielles (de ce point de vue un point 'se trouve sur' une conique si les deux tangentes menées par lui coïncident etc.) [...] Plus grave est peut-être la circonstance que l'introduction successive des conditions par la formule $\alpha\mu$ conduira, sitôt qu'on a dépassé certaines limites – au delà desquelles on devrait trouver, du point de vue actuel, une infinité de solutions – à des résultats sans aucun sens immédiat. On trouve par exemple 8 coniques passant par un point et tangentes à 4 droites au lieu de toutes les droites doubles par le point. 2° Ces difficultés conduisent au point de vue de Chasles, où l'on a égard d'une manière égale aux propriétés ponctuelles et tangentielles. [...] Pour ce point de vue on aura le théorème de Chasles qui en est peut-être une conséquence si intime que sa démonstration

¹⁸⁶Letter Ms 5624 224, Zeuthen to Halphen, dated August 11th 1876.

se présenterait d'elle-même, si l'on savait bien définir précisément ce point de vue. Les difficultés à cet égard résultent du point de vue de départ double qui empêche par exemple de distinguer clairement les catégories du 'général' et particulier : A peut être un cas particulier de B pour la définition ponctuelle, pendant que B est un cas particulier de A pour la définition tangentielle. On rencontre les mêmes difficultés dans les autres cas où l'on fait usage du même point de vue, notamment dans la théorie des singularités dites ordinaires des courbes planes (formules de Plücker) des courbes gauches et des surfaces, et de celles des systèmes de courbes d'ordre supérieur. M'étant occupé beaucoup de toutes ces questions, j'ai eu lieu de connaître assez bien ces difficultés qui ont rapport toutefois plus à l'énoncé complet et exact des vérités qu'à leur découverte et démonstration. [...] Votre point de vue, qui est aussi celui où se veillent placer Clebsch et Lindemann, est entièrement clair et bien défini. Vous ne voulez indiquer que le nombre des solutions propres, celles qui sont indépendantes des valeurs données étant regardées comme impropres.

When Halphen would present his enumerative theorems as more general than those of his predecessors, Zeuthen would rather speak of a more 'absolute' (*absolu*) theorem¹⁸⁷. For Zeuthen, as for Study, the crux of Chasles' problem was to define the terms 'general' and 'particular'. This understanding of the relation between the theories of De Jonquières, Chasles, and Halphen would play a crucial role in Zeuthen's later texts on the matter, and in particular his entry for Klein's *Encyklopädie* and his textbook on enumerative methods, both of which begin with epistemological discussions of the concepts of 'general' and 'particular'¹⁸⁸. Halphen did not follow Zeuthen in this interpretation, as is made clear by his later publications. One reason for this might be that Halphen credited 'computations with setting him on the right path¹⁸⁹' regarding the inaccuracy of Chasles' formula. Analytical computations, for Halphen, were a tool for complete objectivity. Zeuthen, in his own philosophical essays, viewed his craft as guided by intuition and pure reasoning instead, whereby the comparison of equally valid (if not equally worth pursuing) viewpoints becomes more amenable¹⁹⁰.

¹⁸⁷"Vous dites que votre théorème véritable est plus 'général' que ces théorèmes énumératifs généraux. Je préférerais de dire que votre théorème est absolu", Letter Ms 5624 237, Zeuthen to Halphen, dated December 18th 1879.

¹⁸⁸ [Zeuthen, 1905], [Zeuthen, 1914a].

¹⁸⁹"Vous attribuez au calcul l'honneur de vous avoir remis dans le droit chemin quant à $\alpha\mu + \beta\nu$. Sans doute vous devez connaître les moyens qui vous ont fait découvrir la vérité à cet égard, et je sais que dans les démonstrations vous vous êtes servi du calcul. Il me semble néanmoins notable que c'est vous, Monsieur, qui êtes le plus géométrique de ceux qui se sont occupé de cette question – qui avez réussi à l'éclairer complètement", Letter Ms 5624 226, Zeuthen to Halphen, dated December 15th 1877.

¹⁹⁰In 1914, Zeuthen wrote a short philosophical essay in Danish on reasoning in mathematics, wherein he appeals to Bergson's concept of intuition to justify the priority of pure thought over rote, mechanical

Henri Poincaré, in his *Notice* on Halphen after the latter's passing, also dwelled on the question of generality, and of its definition. Enumerative problems, Poincaré explained, consist in finding how many objects, defined by a certain number of parameters, satisfy an appropriate number of conditions. Each of these conditions has to be algebraic; as a result, the 'brute result' to every enumerative problem is given by the product of the degrees of the algebraic equations which translate the conditions. Such a simplistic answer, Poincaré continues, is unsatisfactory, precisely because exceptional solutions (such as some degenerate conics) should be kept out of the final number, because they do not adequately fulfil the conditions. For Poincaré, this means that enumerative problems consist in the 'equal use of analytical dexterity and sagacity or discernment to distinguish the various kinds of solutions, enumerate them separately, and recognize those who ought to be kept'. Poincaré, echoing some of his famous views on the status of hypotheses in geometry, then immediately reformulated this analysis in terms of conventions¹⁹¹:

Tout cela, dira-t-on, est une affaire de convention. Encore faut-il énoncer clairement cette convention, et l'analyse que j'ai donnée plus haut des travaux d'Halphen nous a montré que ce n'était pas toujours chose aisée. On ne l'a malheureusement pas toujours fait ; une convention semblait naturelle, parce que l'on considérait les choses d'un certain biais ; on l'admettait sans l'énoncer explicitement. D'autres chercheurs se plaçaient ensuite à un point de vue différent et oubliaient cette convention tacite ; de là des contradictions tantôt apparentes, tantôt réelles. *En général*, disait-on volontiers, il arrive ceci ou cela ; on oubliait que, sans une convention spéciale, le mot *en général* n'a aucun sens.

Neither Poincaré nor Zeuthen, as they expressed the views presented here, were in possession of Study's articles. And yet, they both understood the significance of Halphen's counter-examples to reside not (just) in the refutation of a formula, but in the distinction between several viewpoints on enumerative problems and on what counts as solutions to them.

8.3.4 Searching for Chasles' Ghost

This partial convergence in their understanding of the epistemic status of Halphen's work did not prevent Zeuthen and Study from clashing publicly over the latter's theses on the theory of characteristics. In 1887, one year after the publication of Study's papers in

computing; see [Zeuthen, 1914b].

¹⁹¹ [Poincaré, 1890], p.152. On conventions in Poincaré's philosophy of mathematics, see [Heinzmann, 2009].

Mathematische Annalen, Zeuthen wrote a long letter to Klein to express his displeasure at Study's claim that enumerative geometers, at least prior to Clebsch, had been very naïve. Zeuthen also lamented Study's refusal to even discuss the matter privately during his stay in Copenhagen¹⁹². Zeuthen then went on to express his doubts regarding Study's proof, while admitting to not fully understanding it. For Zeuthen, the discussion of Clebsch's 'mobile solutions' was not crucial to the affair; rather, he took Halphen's essential discovery to be the third kind of degeneration for conics, and most importantly the fact that it depends on four conditions (and not on three, which would make it a second-order degeneration, thus not relevant to the generality of Chasles' formula). Thus, Study's claim to have perfectly defined a conic section as simultaneously a locus of points and straight lines was for Zeuthen wrong¹⁹³.

Zeuthen did not immediately publish these remarks, and neither did Halphen; however, upon seeing that these two established experts had cast doubts over the validity of Study's proof, Klein, as editor of the *Mathematische Annalen*, wanted to remain impartial: in 1887 already, he had suggested Halphen write a response, but by then the latter had moved on to other areas of mathematics. After Halphen's passing, and after a visit by Zeuthen, Klein set out to have the matter resolved and the dispute cleared. In June 1890, he asked Zeuthen to write a letter which would clarify the substance of their disagreement; the letter was indeed written and published in the same journal as Study's papers, with a direct reference to them¹⁹⁴. In parallel, Klein warned Study that such a letter would be

¹⁹²“[Ich werde] es hier noch bedauern, dass ich die Gelegenheit verfehlte zu einer mündlichen Polemik mit dem Herrn Study, der in Kopenhagen war, mich aber nicht traf. Seine Arbeit über die Charakteristiken zeigt, dass er hinlängliche Begabung besitzt, um eine Discussion interessant zu machen; meine Unzufriedenheit mit dieser Arbeit würde aber einer solchen Diskussion hinlänglichen Inhalt geben. [...] Wir alten Charakteristiker waren nicht so naiv wie er voraussetzt”, Nachlass Klein, Letter Zeuthen to Klein 436, dated November 4th 1887, quoted in [Hartwich, 2005], p.73. Note that Zeuthen had already complained about Study's assessment of the 'naïvety' of past enumerative geometers in a letter to Halphen dated August 19th 1886, Ms Cod 5624 246, writing “Sa considération de ces courbes [dégénérées] ne diffère donc pas de celle dont nous autres auteurs antérieurs à Clebsch – qu'il traite d'assez naïfs – nous sommes servi. Il se rend donc aussi coupable de la même faute que tout le monde avant vous, et n'a pas compris le fait qu'aussi la troisième classe de coniques dégénérées peuvent dépendre de quatre conditions.”

¹⁹³“Seine Missverständnisse culminierten in der Bemerkung Seite 9, wo er sagt, ‘dass ein Kegelschnitt vollkommen bestimmt ist sobald er entweder als irreduzible Curven oder als Punktpaar oder als Linienpaar gegeben vorliegt’. Das ist eben eine Wiederholung der wirklichen Naivität der alten Charakteristiker, und wenn wir daran Recht gehabt hätten, würde die Formel $\alpha\mu + \beta\nu$ vollkommen richtig sein. Bei uns konnte man sie entschuldigen, aber nicht bei dem Herrn Study, der wissen sollte, dass die Erneuerung Halphens eben in der Bemerkung besteht, dass es eine dritte Art von Spezialfällen von derselben Allgemeinheit ist, nämlich diejenige welche aus den anderen sich bildet resp. durch Zusammenfallen der zwei Punkte oder durch Zusammenfallen der zwei Geraden. Wir kannten – und benutzten – natürlich auch diese Grenzform, übersahen aber, dass auch sie eine vierfache Unendlichkeit haben konnte”, Nachlass Klein, Letter Zeuthen to Klein 436, dated November 4th 1887, quoted in [Hartwich, 2005], p.74. It must be noted in passing that a rather general feature of these discussions of Chasles' formula is that each actor seems to identify something different as being key to the generality (or lack thereof) of formula, thus making for conversations which sometimes lead to nowhere.

¹⁹⁴ [Zeuthen, 1890].

published shortly, and asked him to prepare a reply – not without admonishing him for not having already done so when Halphen had expressed reservations regarding his papers while in Paris. Klein was not only acting as an impartial editor, but also as a mentor trying to safeguard Study's career: Zeuthen's criticisms were so sharp, Klein wrote to Study, that they might be prove fatal to his students' professional aspirations¹⁹⁵. Klein's concerns were founded: the content of Zeuthen's letter would be discussed even amongst mathematicians who had not directly engaged with enumerative geometry proper, as for instance can be seen in the correspondence between Schur and Engel¹⁹⁶.

By the time Study decided to answer, Klein had passed down a part of his editorial duties to von Dyck, by another student of his but also his assistant¹⁹⁷. Von Dyck rejected Study's immediate answers, as being too brash and unconvincing¹⁹⁸. Early versions of Study's response would also be criticized by some of the internal reviewers of the journal: as Von Dyck himself reported back to Study, Gordan and Nöther found "Study's formulation of the question [in his Habilitationsschrift] too vague for one to see what is in it", and saw in his new papers mostly "polemics and repetitions¹⁹⁹". Lie and Mayer, on the other hand, abstained from any definitive judgment. It would take over a year for Study to publish his response, by way of three successive papers²⁰⁰, of which the last words were written in April 1892. The first of these papers consists in a more detailed discussion of the way in which Study maps the conics of a plane onto the points of a certain variety; a mapping which was at the heart of his 1886 proof. The second of these papers is a direct response to Zeuthen, relying on this new explanation of the construction of the variety of conics; the tone of which is extremely polemical. Lastly, a third paper gives more details as to how the theory of quadratic forms can be used to analytically represent systems of conics within the settings of this variety. Zeuthen was still not convinced: in October 1892, that is to say immediately after the publication of Study's response, he penned yet another letter. In it, he gave another example inspired by Halphen's memoirs which supposedly proved that Chasles' formula was not exact in general, and reiterated

¹⁹⁵ [Hartwich, 2005], pp.74-76.

¹⁹⁶The Zeuthen-Study dispute is mentioned in two letters by Schur to Engel, respectively dated Dec. 19th 1890, and Jan. 22nd 1891; see in Nachlassverzeichnis Engel (Universitätsarchiv Gießen), ref. NE110313 and NE110314 respectively

¹⁹⁷ [Hashagen, 2003], pp.383-391.

¹⁹⁸ [Hartwich, 2005], p.78.

¹⁹⁹"[Gordan und Nöther] Wir stimmen mit Prof. Kleins Brief überein [...]. Study's Fragestellung (Habilitationsschrift) ist zu unbestimmt, als dass man das darin Steckende erkennen könnte. Erst aus Halphens Definitionen lässt sich ersehen, dass die Ausartungen [...] alle bei Study mitgenommen sind.[...] Study's neuer Aufsatz besteht aus drei Teilen [i. W. Polemik und Wiederholungen]. Keiner dieser drei Teile spricht also für unveränderte Aufnahme in die Annalen", Nachlass Engel, Letter von Dyck to Study, dated February 13th 1891, quoted in [Hartwich, 2005], pp.80-81.

²⁰⁰ [Study, 1892a], [Study, 1892b], [Study, 1892c].

his criticisms toward Study's work²⁰¹, after which the editorial board of *Mathematische Annalen* put an end to the dispute²⁰².

In fact, this dispute could not possibly end in any other way. As Nöther, acting as a reviewer for the whole exchange, had noticed, the dispute had shifted from that over the validity of a formula and its proof, to a debate about who of Halphen and Study had more aptly captured Chasles' original intuition²⁰³. Indeed, Study and Zeuthen both acknowledged that the proofs contained in Study's *Habilitationsschrift* and Halphen's memoirs were equally valid, but fought over which actually proved what was to be proven. For instance, Zeuthen wrote at the end of his first open letter²⁰⁴:

C'est donc, selon moi, Halphen qui a répondu à la question qui avait occupé les géomètres depuis 1864, et, en tout cas M. Study n'a répondu qu'à une modification de cette question; mais la solution d'Halphen a un avantage encore plus essentiel : elle est une solution complète qui *comprend en elle la réponse à toutes les questions particulières qu'on obtient par des formulations particulières de la question*. Elle donne donc aussi la réponse à la question que se propose M. Study. [...] La présente critique paraissant, à cause des circonstances dont j'ai déjà rendu compte, après un intervalle assez long pour faire croire à M. Study que les géomètres accepteraient ses vues, je dois ajouter qu'elle n'a égard qu'à la question qu'il se propose et aux rapports avec des recherches antérieures qu'il lui attribue, et non pas à l'analyse dont il se sert ensuite pour résoudre cette question.

Study gladly accepted to have the fight on this terrain: in the first of his three papers, he concluded that "Halphen's criticism of Chasles' theorem is only justified here to the extent that it refers to its deficient formulation, and his own theory does not make the addition we have given seem superfluous²⁰⁵". In his more vindictive *Entgegnung*, Study

²⁰¹ [Zeuthen, 1893].

²⁰² [Lange, 1895], p.627.

²⁰³Von Dyck reports in the same letter to Study that Nöther was opposing Study's papers precisely because they framed the question in these terms: "Nach Nöthers Gutachten kann es sich dabei [bei Study's *Entgegnung*] nicht mehr um die Frage handeln, was Chasles gewollt hat, was Halphen "fremdes" hereingebracht hat – sondern nur darum, dass Sie scharf präzisieren, in welcher Form Ihre Definitionen die Abzählung in jener allgemeinen Form ergeben", [Hartwich, 2005], p.81.

²⁰⁴"Therefore, according to me, Halphen is the one who answered the question which geometers had been busy with since 1864, and, at any rate, Mr. Study only answered a modified version of this question; but Halphen's solution has an even more essential advantage: it is a complete solution which contains the answer to all particular questions one obtains through particular formulations of the question. Thus, it also gives the answer to the question proposed by Mr. Study. [...] As this criticism appears, due to circumstances which I have already reported, after an interval long enough to lead Mr. Study to believe that geometers would accept his views, I must add that it pertains only to the question he proposes to solve, and to the relationship with previous research which he attributes to it, and not to the analysis which he then uses to solve that question", [Zeuthen, 1890], p.464.

²⁰⁵"Halphen's Kritik des Chasles'schen Satzes ist hiernach nur soweit gerechtfertigt, als sie sich auf dessen

attacks Zeuthen's understanding of Halphen's theory, as well as the adequation between this theory and the usual understanding of degenerate conics. Zeuthen's 1893 reply, meanwhile, asserts once more that Study's theory is at odds with "the real signification of Chasles' hypothesis on the expression $\alpha\mu + \beta\nu$ ²⁰⁶", and provides yet another example in which to accept Chasles' formula means to count conics which do not satisfy in a meaningful way the condition being proposed. It must be noted that neither Zeuthen nor Study really ever engaged with the technical, inner workings of the other side's theory: Zeuthen does not seem to have mastered the theory of forms and the concept of variety, while Study never really discusses the intricate analytical computations of Halphen's.

Remarkably, after decades of undecidedness, Chasles' formula had become in the early 1890s the object of yet another kind of dispute. This time, it was not so much a question about its truth, but about its (geometrical) significance. At the core of the Zeuthen-Study dispute lies the question of assessing whether or not a valid proof addresses the initial question – that is to say, the question asked in a non-formal language – adequately and meaningfully²⁰⁷. For this new epistemic task, new modes of argumentation were needed: writing his letters in French was for Zeuthen part of a larger arsenal in his dispute, a way to show his familiarity with the works of both Chasles and Halphen²⁰⁸. More profoundly, the examples constructed by both Zeuthen and Study in their replies all aim to show cases where the other side's theory does not yield results with the proper sort of geometrical significance. For those who, like Zeuthen and Study, had accepted the multiplicity of possible points of view over a common geometrical problem, mathematical life had something entirely new. For these modern mathematicians, the validity of a proof was not the end of a scientific endeavour, but rather the beginning of new discussions.

Conclusions

The story of Chasles' formula, and of the disputes it gave rise to, is also the story of the modernist transformation of mathematical life. The epistemic virtues to which a mathematician is beholden, the inferential moves and literary output at the heart of their scientific practices, as well as their cultural status: all of these changed rapidly throughout the course of the years surveyed in this chapter, and shaped in different ways

mangelhafte Formulierung bezieht, und seine eigene Theorie lässt die von uns angegebene Ergänzung nicht als überflüssig erscheinen", [Study, 1892a], p.558.

²⁰⁶"la véritable signification de l'hypothèse de Chasles sur l'expression $\alpha\mu + \beta\nu$ ", [Zeuthen, 1893], p.539.

²⁰⁷Lakatos has famously argued that formal mathematics necessarily beget such a problem, which he called 'the problem of translation'; [Lakatos, 2015], pp.112-134

²⁰⁸Another explanation for that, suggested in [Hartwich, 2005], p.78, is that writing in French meant that Klein would not be entitled to correct Zeuthen's grammar, and thereby, soften the tone of his criticism.

the approaches that Chasles, Schubert, Halphen, Zeuthen, and Study had of a common, somewhat vague geometrical problem. Modern-day mathematics tells us that there always was a plurality of valid answers to that common problem – various models of a same set of paradigmatic statements. This is not to say that the uncertain mathematics at the heart of the disputes presented here were not insightful; rather the opposite. However, more profoundly, the struggles to clarify the status of Chasles' problem have helped emerge not just the realization that several answers to it are possible, but the very epistemic and cultural conditions for this diversity to become an accepted (and almost banal) part of mathematical life.

Conclusion

“Ni fait ni à faire”, disaient autrefois les dames bourgeoises du travail de leurs bonnes quand elles en étaient mécontentes. Fait et à faire pourrait être le sous-titre de tout travail philosophique digne de ce nom.

C. Castoriadis, *Fait et à faire. Les carrefours du labyrinthe*, volume V, 1997

In the first decades of the 20th century, enumerative geometry was progressively incorporated into algebraic geometry²⁰⁹. After Hilbert’s 1900 Paris address, many geometers such as Van der Waerden, Zariski, or Severi, all sought to obtain Schubert’s results within their respective approaches – thereby relegating the theory of characteristics to a less central position than it historically had occupied during the episode we have presented here. For these mathematicians, to rigorously define the algebra of conditions (via a calculus of algebraic cohomology classes) and the concept of intersection multiplicity was a much more pressing concern than to generate dozens of formulae for the enumeration of specific objects, as Chasles and Schubert had done. Conversely, these formulae were viewed as rather simple consequences of a much more difficult problem, namely that of the formulation of a suitable theoretical framework in which to interpret them.

By the 1950s, this incorporation of enumerative geometry within algebraic geometry was largely considered to have been successfully conducted. This was, for instance, the

²⁰⁹Remarkably, in many accounts written by mathematicians of the development of algebraic geometry, Schubert’s 1879 *Kalkül* stands amongst the books of algebraists such as Dedekind, Severi, or Lefschetz, despite, as we saw in chapter 7, operating with a widely different concept of algebra; see for instance [Van der Waerden, 1971]. In fact, Schubert’s book seems to have been part of the curriculum for geometry students in several European universities at least until the 1930s, as indicate for instance the writings of Van der Waerden who studied in Amsterdam and Göttingen: “in 1936, de Vries published a textbook *Introduction to Enumerative Geometry* in Dutch which Van der Waerden reviewed very briefly for *Zentralblatt* (vol. 15, p. 368f), writing in particular that, according to his own experience, there was no better way to learn geometry than to study Schubert’s *Kalkül der abzählenden Geometrie*”, [Schappacher, 2007].

opinion of the French mathematician André Weil, who wrote in his 1946 *Foundations of Algebraic Geometry*²¹⁰:

Our results include all that is required for a rigorous treatment of so-called “enumerative geometry”, thus providing a complete solution of Hilbert’s fifteenth problem. They could be said, indeed, to belong to enumerative geometry, had it not become traditional to restrict the use of this phrase to a body of special problems, pertaining to the geometry of the projective spaces and of certain rational varieties (spaces of straight lines, conics, etc.), whereas we shall emphasize the geometry on an arbitrary variety, or at least on a variety without multiple points. [...] A history of enumerative geometry could be a fascinating chapter in the general history of mathematics [...], provided it brought to light the connections with related subjects, not merely with projective geometry, but with group-theory, the theory of Abelian functions, topology etc.

As the last sentence in this quotation shows, what was understood to constitute enumerative geometry in 1946 was a far cry from what we have presented in this dissertation. Topology only marginally appeared in Study’s defense of Chasles’ $\alpha\mu + \beta\nu$ formula, while no connections between group theory or abelian functions and enumerative methods had been established throughout the 19th century.

Instead, in this dissertation, we have painted a picture of enumerative geometry as a discipline which emerged from the circulation of a shared question across various mathematical cultures. We have argued that Chasles’ theory of characteristics ought to be read against the backdrop of a research programme centered around the construction of devices for the writing of the (geometrical) equations of curves not relying on axes of coordinates and other extrinsic means of representation. The theory of characteristics, in turn, quickly and widely circulated across Europe, thereby becoming the focus of most of the research being produced on enumerative problems, widely conceived.

This circulation, however, involved a variety of locales; some quite removed from the Paris Académie in which Chasles had first communicated his results. Not only was the history of this theory transnational, but it also involved actors who shared none of Chasles’ mathematical methods, or indeed his research programme. For instance, this theory was reformulated within the theory of invariants and algebraic analysis by Clebsch and Halphen; while, Schubert turned this theory into a universal symbolic device for the expression and computation of geometrical conditions, borrowing from other synthetic geometers an approach of the concept of figure as something to be systematically and uniformly generated from fundamental elements (points, rays, planes).

²¹⁰ [Weil, 1946], pp.viii-ix.

However, as we have shown, these translations were not merely the adaptation of an unchanging mathematical core to various algebraic or geometrical languages. Halphen's analytical translation of Chasles' theory was motivated by the view that only analysis was able to properly define the concepts underlying this theory – concepts such as condition, independence, or even solution. By mobilizing analysis, he was responding to certain epistemic worries about what he perceived to be the undue reliance on intuition of past geometers, and mobilized the values of rigor and meticulousness to justify his own methods. By contrast, Schubert's symbolic recasting of this theory was conducted in keeping with epistemic norms such as unitariness or simplicity. For Schubert, the end goal of enumerative methods was to yield a unique formula for each figure, wherein was contained the entirety of the enumerative theory of this figure. These results embodied Schubert's normative description of mathematical practice as the free creation of (non-contradictory) symbolic laws, devoid of epistemic worries about the rigor of such practices.

However, as these mathematicians had realized, the very meaning of enumerative results crucially hinges upon the stipulation of a certain mode of generality from which they are to be read. To state that a constant number of curves satisfy a fixed set of conditions, one must also decide what curves are to be accepted as properly satisfying these conditions. To agree on a common notion of generality, they all recognized, was necessary for the collective enquiry over enumerative problems. As a result, the disagreements over the epistemic norms of mathematical activity quickly led to lasting discord over the proper way of proving and stating enumerative results, which crystallized around Chasles' $\alpha\mu + \beta\nu$ formula.

At the end of this study, we hope to have showcased the importance and fruitfulness of the history of enumerative geometry. This episode informs and enriches our understanding of the history of various other branches of mathematics, from the theory of invariants to that of projective geometry. More crucially, however, the history of the theory of characteristics serves as a rich case-study for the description of the profound transformation of mathematics as a cultural and scientific practice underwent during the second half of the 19th century. Lastly, this dissertation adds to our understanding of the history of the value of generality in mathematics, and of its crucial role in the shaping of collectively-shared objects and methods for the stating, proving, and debating of mathematical propositions.

There is much left to study regarding the history of enumerative geometry, even within the time frame to which we have elected to restrict ourselves in this dissertation. A global analysis of the circulation of enumerative questions in mathematical journals, in Europe and elsewhere, would be welcome. In particular, the circulation of Schubert's 1879 *Kalkül* deserves more attention, as this book seems to have quickly become a staple in courses on geometry in many universities. Besides the social history of this discipline,

we still need a detailed understanding of the mathematics involved in Halphen's and Study's work on Chasles' $\alpha\mu + \beta\nu$ formula. Reconstructing the fine-grained details of the mathematical practice of Halphen and Study would require further discussions of other technical developments in late 19th-century mathematics, such as the theory of forms and the emergence of the concept of variety. Thus, the limited case of the theory of characteristics provides a window from which to observe the state of geometry in this period.

Beyond enumerative geometry, there also remains to be seen how the history of mathematical generality presented in this dissertation echoes other episodes in the history of mathematics. This could be done in at least two ways. One would be to select contemporary developments, with actors belonging to the same scientific cultures as the ones discussed in the present dissertation, so as to compare the formation of concepts and practices of generality in both episodes. For instance, it would be useful to compare what we have said of Halphen to other Paris-based mathematicians with a professional identity firmly rooted in their military duties, and to see to what extent they shared a conception of generality, of the role and value of analysis, and through this lens compare their mathematical practices. Another approach would be to compare the ideals and practices of generality in a diachronic perspective. For instance, the linked values of simplicity and generality in Chasles' epistemology of geometry could be compared to that expressed by Georges Bouligand in his papers on "direct methods"²¹¹, or even to Grothendieck's description of the "childish simplicity"²¹² which presided over some of his major discoveries.

²¹¹ [Bouligand, 1932]. I thank Michael Chalmers and David Waszek for drawing my attention to this text.

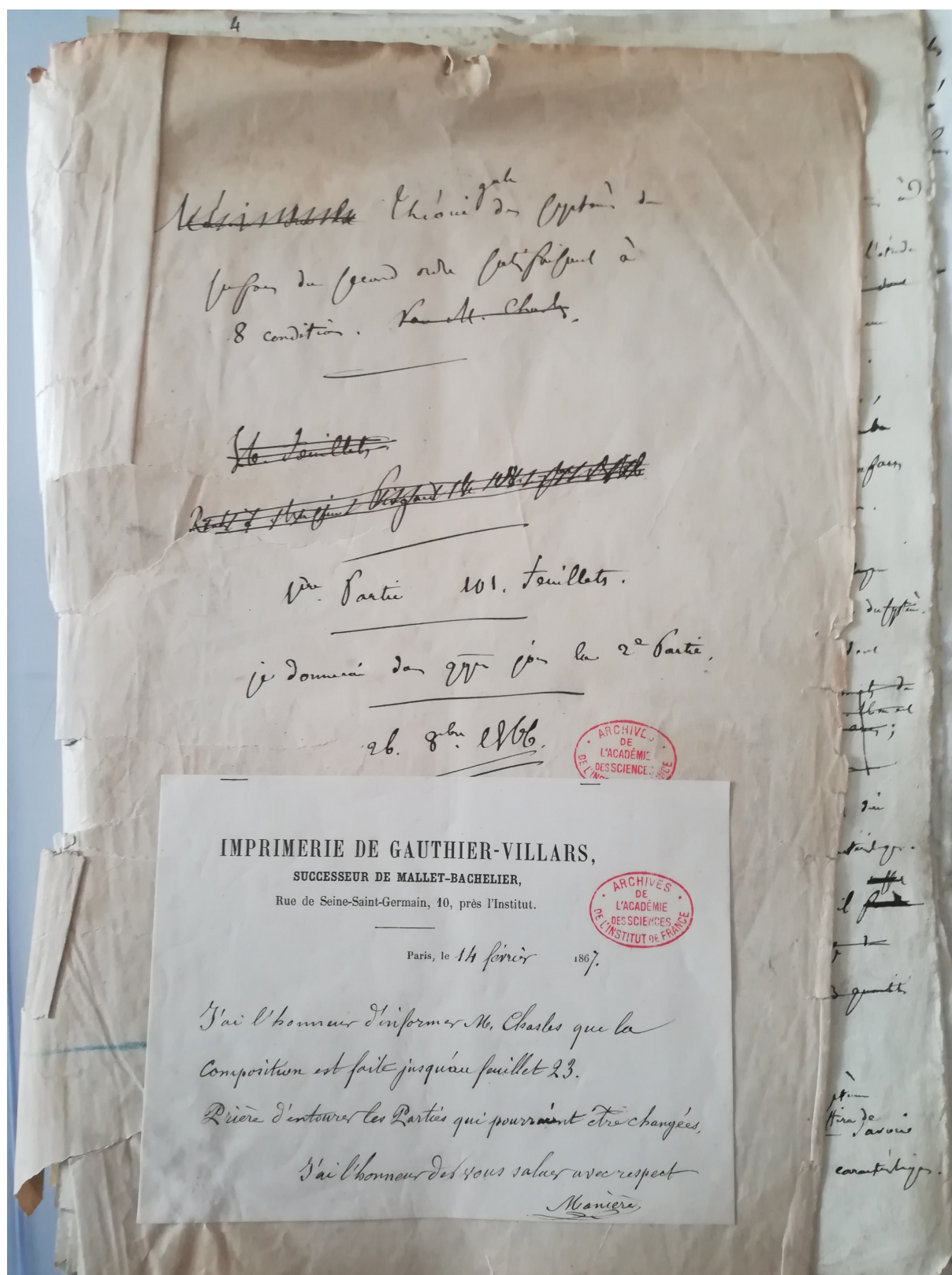
²¹²"Simplicité enfantine"; see [Grothendieck, 1986], pp.50-69.

Chasles' unpublished theory of systems of second-order surfaces

Between October 1866 and February 1867, Chasles was in the process of writing a memoir on the theory of characteristics for systems of second-order surfaces, which he intended to have published by Gauthier-Villars. This memoir was never finished; however, in the Archives of the Paris Académie des Sciences¹, we have found a handwritten version which covers a large portion of the intended volume, dated October 26th 1866. Furthermore, the first few sections of this memoir had been printed and sent back to Chasles for corrections on February 11th 1867. Chasles inked in some changes, which mostly consist in corrections of grammatical or formatting mistakes. In what follows, we reproduce this printed document, using brackets to distinguish Chasles' corrections from the printed text. As the printed text stops mid-sentence, we transcribe the rest of the section on exceptional surfaces from the earlier, handwritten-version.

The text goes on for some 90 more pages, which we will not transcribe here. In these pages, Chasles computes the characteristics of elementary systems of surfaces, as outlined in the introduction of this memoir. To that end, Chasles employs a variety of techniques, from the principle of correspondence, to the enumeration of special surfaces and the use of the relations between characteristics and numbers of special conics in a system.

¹Académie des sciences, Archives et patrimoine historique, Dossier Chasles, 35J/8.



A.1 The printed document (February 11th 1867)

[1^{re} / 11 février 1867]

THÉORIE GÉNÉRALE DES SYSTÈMES DE SURFACES DU SECOND ORDRE SATISFAISANT À HUIT CONDITIONS

INTRODUCTION

- 1 1. L'étude des systèmes de courbes planes assujetties à autant de conditions moins une qu'il n'en faut pour déterminer une courbe de l'ordre proposé se ramène à l'étude d'un système unique défini par deux nombres appelés *caractéristiques*, savoir : le nombre des courbes du système qui passent par un point, et le nombre des courbes qui touchent une droite. Cette théorie s'étend naturellement aux systèmes de surfaces que l'on définit par trois caractéristiques, dont la première exprime le nombre de surfaces qui passent par un point, la seconde le nombre des surfaces tangentes à une droite, et la troisième le nombre des surfaces tangentes à un plan.

De même que pour les courbes, les propriétés d'un tel système de surfaces ne dépendent que des caractéristiques du système. Par exemple, pour les surfaces du deuxième ordre, les seules dont il doive être question ici, en appelant μ, ν, ρ les trois caractéristiques, on trouve que :

Le lieu des axes des surfaces est une surfaces de l'ordre $(\mu + \nu + 2\rho)$;

La développable enveloppe des plans diamétraux principaux est [s] de l'ordre $(\mu + \nu + \rho)$.

Les plans diamétraux conjugués aux diamètres qui passent par un point donné coupent les surfaces suivant un système de coniques $(\mu + \rho, \mu + \nu + 2\rho, \rho)$ [italiques]

De sorte que l'on peut dire que tous les systèmes se ramènent à un seul, défini par les trois caractéristiques.

Ce sont donc les propriétés de ce système unique qu'il suffit d'étudier. Puis, pour appliquer ces propriétés à un système particulier défini par huit conditions données, il suffira de savoir conclure de ces huit conditions les valeurs numériques des trois caractéristiques du système proposé.

Ainsi, toute la théorie des systèmes de surfaces se réduit, comme celle des systèmes de courbes, à ces deux questions fondamentales : 1° trouver les propriétés du système défini par les trois caractéristiques μ, ν, ρ ; 2° calculer les caractéristiques d'un système défini par huit conditions données.

2. Chacune des conditions données exige la connaissance d'une propriété du système général : on le conçoit, car si une condition, est, par exemple, que les surfaces aient un sommet sur un plan donné, il faut, pour introduire cette condition, connaître l'ordre de la courbe lieu des sommets des surfaces du système général (μ, ν, ρ) , d'où l'on conclura le nombre des surfaces qui satisfont à la condition proposée.

Connaissant les propriétés relatives aux conditions qui définissent un système, on a donc à calculer les caractéristiques de ce système. Cela se fait par une *méthode de substitution* tout à fait semblable à celle qui nous a servi dans la théorie des courbes. Ce procédé, comme on l'a vu, se distingue essentiellement des méthodes analytiques, en ce que l'on y évite les équations de condition et les calculs d'élimination, souvent si difficiles, et plus souvent encore impossibles. C'est là le caractère propre de la méthode. Et il est permis de croire que l'on trouvera d'autres questions, même de pure analyse [A], auxquelles elle s'appliquera : car toutes les parties des Mathématiques se touchent par certains points, que l'étude et les progrès successifs de la science font découvrir.

3. L'application de ce procédé de substitution à la théorie des surfaces du second ordre demande que l'on connaisse les caractéristiques des systèmes *élémentaires* de ces surfaces. Nous appelons *élémentaires* les systèmes qui ne renferment que les conditions de passer par des points et de toucher des droites et des plans. C'est dans ces systèmes que l'on substitue successivement aux conditions *élémentaires* les conditions du système proposé, pour arriver à la connaissance des caractéristiques de ce système.

Cette substitution doit se faire dans tous les systèmes élémentaires, et comme ils sont nombreux, le calcul serait long ; mais le résultat se peut exprimer par une formule générale dans laquelle on introduit immédiatement et sans calcul les huit conditions données. De sorte qu'une seule formule résout tous les problèmes.

4. La connaissance des caractéristiques des systèmes *élémentaires* est donc la base de notre méthode. Les trois sortes de conditions simples, passer par des points et toucher des droites et des plans, combinées huit à huit, donnent lieu à quarante-cinq systèmes différents. Chaque espèce [système] a trois caractéristiques : toutefois, chacun d'elles entrant dans plusieurs systèmes, il n'y a, en réalité, que cinquante-cinq caractéristiques différentes. Ce sont ces cinquante-cinq nombres qu'il faut connaître.

Mais il y a lieu d'admettre d'autres systèmes *élémentaires*, car les quarante-cinq dont 2 il s'agit sont formés de huit conditions distinctes, tandis que ces conditions, points, droites et plans, se peuvent associer de manière à former des conditions multiples, indivisibles, qui constituent encore des systèmes élémentaires. Par exemple, que les surfaces doivent toucher une droite en un point donné, ou bien toucher un plan en un point non déterminé d'une droite donnée dans le plan, ce sont là des conditions doubles ; que les surfaces

doivent passer par une droite, ou bien toucher un plan en un point donné, ce seront des conditions triples ; que les surfaces doivent passer par une conique, condition quintuple ; etc.

Tous ces cas constituent des questions spéciales et indépendantes, et non des cas particuliers, comme on pourrait les considérer en Géométrie analytique. Ils donnent lieu à des classes ou à des types de systèmes différents, dont il faut déterminer directement les caractéristiques, ainsi que celles des quarante-cinq systèmes de la classe des conditions simples ; ce qui entraîne dans d'assez nombreuses recherches.

ÉNUMÉRATION DES CONDITIONS MULTIPLES. – NOTATIONS.

5. Conditions doubles. – 1° Les surfaces touchent une droite L en un point donné θ ; ce que nous exprimerons par θL ; 2° les surfaces touchent un plan P en un point situé sur une droite Δ donnée dans le plan ; $\overline{P\Delta}$.

Conditions triples. – 1° Les surfaces passent par une droite G ; 2° les surfaces touchent un plan K en un point donné θ , $K\theta$.

Condition quadruple. – Les surfaces passent par une droite G et sont tangentes, en un point θ de cette droite, à un plan K mené par la droite ; $K\theta G$.

Conditions quintuples. – 1° Les surfaces passent par une $[\sharp]$ conique Σ ; 2° les surfaces passent par deux droites G, G' qui se coupent ; $\widehat{G, G'}$; 3° les surfaces sont inscrites dans un cône du second ordre C .

Conditions sextuples. – 1° Les surfaces passent par une conique Σ et sont tangentes, en un point θ de Σ , à une droite θL ; $\Sigma\theta L$; 2° les surfaces passent par deux droites G, G' qui se coupent, et elles sont tangentes, en un point θ de G , à un plan K mené par G ; $K\theta\widehat{G, G'}$; 3° les surfaces sont inscrites dans un cône C et sont tangentes à une arête du cône en un point donné.

Conditions septuples. – 1° Les surfaces passent par trois droites G, G', G'' , dont l'une G' s'appuie sur les deux autres ; $\widehat{G, G', G''}$; 2° les coniques passent par deux droites G, G' et sont tangentes, en deux points θ, θ' de ces droites, à deux plans K, K' passant, respectivement, par les deux droites ; $(K\theta G, K'\theta' G')$.

Conditions octuples. – 1° Les surfaces sont toutes inscrites dans un cône avec lequel elles ont une même courbe de contact Σ_2 ; 2° les surfaces passant [e] toutes par deux coniques Σ et Σ' , qui se coupent en deux points ; $\widehat{\Sigma, \Sigma'}$; 3° les surfaces passent par une conique Σ et par deux droites G, G' qui passent par deux points de Σ et se rencontrent ; $\widehat{\Sigma, G, G'}$; 4° les surfaces passent par une conique Σ et touchent un plan K en un point θ ; 5° les surfaces passent par une conique Σ et par une droite G qui rencontre Σ , et elles sont tangentes, en un point θ de G , à un plan K mené par G ; 6° les surfaces passent par quatre droites G, G', G'', G''' qui se coupent deux à deux consécutivement (c'est-à-dire qui

forment un quadrilatère gauche) ; 7° les surfaces sont toutes inscrites dans deux cônes qui ont deux plans tangents communs ; 8° les surfaces sont inscrites dans un cône et passent par deux droites situées dans deux plans tangents, et qui se coupent (sur l'arête commune aux deux plans) ; 9° les surfaces sont inscrites dans un cône, et passent par une droite située dans un plan tangent au cône ; et en outre, elles touchent, en un point de cette droite, un plan mené par la droite.

Ces diverses conditions multiples élèvent à dix-huit le nombre des classes de systèmes dont il faut déterminer les caractéristiques.

SURFACES EXCEPTIONNELLES.

6. On a vu qu'il existe, dans tout système de coniques, des coniques exceptionnelles ou *quasi-coniques*, les unes formées de deux droites et les autres de deux points, celles-ci appelées *coniques infiniment aplaties*. Il existe pareillement, dans les systèmes de surfaces, des surfaces exceptionnelles qui sont des cônes et [:] des coniques représentant des surfaces infiniment aplaties, [:] et parfois aussi des couples de plans et [sur l'arête desquels sont des couples] de points ~~tout à la fois~~ [l'ensemble des 2 plans et des 2 points représentant une surface du système, comme il va être dit !]

On conçoit, *à priori (sic)*, qu'il se trouve, dans un système de surfaces défini par huit conditions, des coniques représentant des surfaces infiniment aplaties, puisque huit conditions déterminent un certain nombre de coniques dans l'espace. C'est pour cela qu'il nous a été nécessaire d'étudier préalablement la théorie des coniques dans l'espace, ce qui a été le sujet du Mémoire inséré dans les *Comptes rendus de l'Académie des Sciences* (t. LXI, p. ; 1865). Dans ce Mémoire se trouve la détermination des caractéristiques des systèmes de coniques satisfaisant à sept conditions *élémentaires*. Ces conditions sont que les plans des coniques passent par des points, que les coniques rencontrent des droites données dans l'espace, et qu'elles touchent des plans. Les trois caractéristiques de chaque système expriment donc le nombre des coniques dont le plan passe

A.2 Excerpt from the manuscript (October 26th 1866)

Théorie générale des systèmes de surfaces du second ordre satisfaisant à 8 conditions.

1^{ère} Partie 101 feuillets

Je donnerai dans quelques jours la 2^e partie

26 8^{bre} 1866

4 [...] Les 3 caractéristiques de chaque système expriment donc le nombre des coniques dont le plan passe par un point, le nombre des coniques qui contiennent une droite, et le nombre de celles qui touchent un plan².

Huit conditions déterminent aussi un certain nombre de cône du second ordre. Il y a donc lieu de former de même une théorie des cones ; mais cette théorie est corrélatrice de celle des coniques dans l'espace ; et l'on conclut immédiatement quant aux couples de plans qui se présentent dans beaucoup de cas, notamment dans les plus simples, où entrent des conditions multiples ~~et dont il faut tenir compte~~, on reconnaît sans difficultés leur présence.

4bis Toutefois 2 simples plans, sans autres éléments, ne suffisent point pour représenter une surface d'un système satisfaisant à 8 conditions. J'entends là que si cette surface n'est rencontrée par une droite qu'en deux points, comme cela doit être, il faut aussi que par une droite on ne puisse lui mener que 2 plans tangents. On satisfait à cette condition, en regardant la surface comme terminée à 2 points de l'arête d'intersection des 2 plans. Tous les plans tangents de la surface doivent passer par l'un ou l'autre de ces 2 points, de même que toutes les tangentes d'une conique infiniment aplaties passent par l'un ou l'autre des 2 points qui limitent la conique.

Ces 2 points que nous appellerons aussi les sommets de la surface, sont indiqués encore par la correspondance corrélatrice qui a lieu entre les plans et les points dans la géométrie des surfaces du 2^d ordre.

Toutes les droites menées dans les 2 plans que l'un des sommets représenteront les génératrices de la surface, puisque les génératrices d'une surface du 2^d ordre sont les 2 droites (réelles ou imaginaires) suivant lesquelles chaque plan H coupe la surface. En outre, par chaque point sur la surface, autre que les 2 sommets, il ne passe que 2 génératrices, comme dans les surfaces générales.

La détermination de 2 plans, dans l'espace, et de deux points sur leur intersection comporte 8 conditions. C'est ainsi qu'un tel système peut satisfaire, de même qu'une conique ou un cône, aux 8 conditions qui déterminent un système de surfaces.

5 Dans les systèmes de coniques, les coniques exceptionnelles formées de 2 droites, ou infiniment aplaties, sont, en général, multiples, c'est-à-dire qu'une telle conique, effective, compte pour plusieurs. Il en est de même dans les systèmes de surfaces, à l'égard des cônes, des surfaces infiniment aplaties, et des couples de plan.

Description de l'ouvrage

²The first three pages of the handwritten manuscript correspond to the printed excerpt, with minor changes. We do not reproduce them here.

7. Cet ouvrage comprend 2 parties.

Dans la 1^{ère} se trouve [sic] la détermination des caractéristiques des 18 systèmes élémentaires, et la démonstration de la formule générale qui exprime le nombre des surfaces qui satisfont à 9 conditions quelconques.

La seconde partie est destinée à la démonstration d'un assez grand nombre des propriétés du système général (μ, ν, ρ) défini par 3 caractéristiques quelconques ; propriétés qui expriment des conditions données et servent au calcul des caractéristiques d'un système proposé.

8. Pour la détermination des caractéristiques des systèmes élémentaires, nous nous appuyerons sur quelques théorèmes [tout simple qui] dérivent de la théorie des systèmes des coniques ~~dans le plan~~, et des quelques relations générales qui ont lieu entre les caractéristiques des divers systèmes, et qui servent à calculer les unes au moyen des autres. Nous procéderons suivant la méthode la plus propre aux conceptions de la pure géométrie, du simple au composé. Les questions les plus simples ici sont celles dans laquelle plusieurs conditions se groupent pour n'en faire qu'une ; par exemple, que les surfaces doivent passer par 2 coniques qui se coupent en 2 points ce qui ~~équivalait à impliquer~~ est un cas particulier de la condition générale de passer par 8 points ; la question sera fort simple, et servira à passer à une autre question un peu moins simple, et ainsi successivement pour ~~terminer par~~ arriver finalement au cas général de 8 conditions distinctes.

6

Théorèmes préliminaires

9. Dans un système de surfaces (μ, ν, ρ) , le nombre des surfaces infiniment aplaties est $(2\mu - \nu)$.

En effet un plan Q coupe les surfaces suivant un système de coniques (μ, ν) qui admettent $(2\mu - \nu)$ coniques infiniment aplaties, qui appartiennent à des surfaces infiniment aplaties. Mais de même que le nombre $(2\mu - \nu)$ des coniques infiniment aplaties est théorique et distinct, en général, du nombre effectif de ces quasi-coniques, parce qu'elles peuvent compter chacune pour plusieurs, de même $(2\mu - \nu)$ reste un nombre théorique ; les surfaces infiniment aplaties pouvant être multiples, c à d compter chacune pour plusieurs.

[Verso: (†) Autrement. Sur un point x d'une droite L passent μ surfaces, qui coupent L en μ points u . ~~De même par un point u p~~ Ainsi à un point x correspondent μ points u . De même, à un point u correspondent μ points x . Donc il existe 2μ points qui coïncident chacun avec un point u correspondant. Ces points appartiennent aux surfaces tangentes à la

droite L , et aux surfaces infiniment aplaties. Les 1^è sont en nombre ν ; donc les secondes sont en nombre $(2\mu - \nu)$.

10. Dans un système (μ, ν, ρ) , il est $(2\rho - \nu)$ cônes.

Ce théorème résulte corrélativement du précédent ; mais se démontre simplement ainsi. Que l'on circoncrive aux surfaces des cônes ayant un sommet commun en un point S de l'espace : ces cônes forment un système (ν, ρ) ; c'est-à-dire que les bases sur un plan Q mené arbitrairement forment un système de coniques (ν, ρ) . Car par un point a passeront les ν coniques bases des cônes circonscrits aux ν surfaces tangentes à la droite Sa ; et ρ coniques seront tangentes à une droite, ce seront les bases des cônes circonscrits aux ρ surfaces tangentes au plan mené par la droite et le point S . Ainsi le théorème est démontré (§).

[Verso: (§) Ainsi que les surfaces infiniment aplaties, les surfaces coniques peuvent être multiples ; et le nombre $(2\rho - \nu)$ est [illegible] théorique, et diffère en général, du nombre des cônes effectifs.

Autrement. Un plan X mené par une droite L est tangent à ρ surfaces ; auxquelles on mène par la droite L ρ plans tangents U , qui correspondent aux plans X . De même, à un plan U correspondent ρ plans X . Donc il existe 2ρ plans X qui coïncident chacun avec un plan U correspondant. Ces plans appartiennent aux ν surfaces tangentes à L , et aux cônes du système. Donc le nombre des cônes est $(2\rho - \nu)$.]

11. Dans un système (μ, ν, ρ) il existe ~~un nombre théorique~~ $(2\nu - \mu - \rho)$ de surfaces représentées par 2 plans.

En effet, un plan Q coupe les surfaces suivant un système de coniques (μ, ν) , qui admettent $(2\nu - \mu)$ coniques représentées par 2 droites. Ces coniques appartiennent à des surfaces tangentes au plan, et à des surfaces représentées par 2 plans. Donc le nombre ~~théorique~~ de ces surfaces dernières est $(2\nu - \mu) - \rho$.

De même que pour les cônes, et pour les surfaces infiniment aplaties, ce nombre $(2\nu - \mu - \rho)$ est théorique, et généralement, le nombre effectif des surfaces formées de 2 plans est moindre.

Ces surfaces formées de 2 plans peuvent être considérées soit comme des surfaces qui ont 2 axes nuls, et un axe terminé à 2 points qui représentent 2 sommets de la surface, dont un peut être à l'infini ; soit comme un cône, ou comme un cylindre formé de 2 plans.

Appendix B

Some letters from Schubert to Halphen

In what follows, we transcribe the entirety of the Schubert-Halphen correspondence preserved at the Paris Bibliothèque de l'Institut. All letters, except for three, are in folder Ms 5624. The letters written between November 11th 1879 and January 7th 1880 can be found in folder Ms 5261. These three letters are the longest and the most substantial of the collection. They most deal with the disagreement between Schubert and Halphen over the validity of the $\alpha\mu + \beta\nu$ formula.

B.1 May 18th 1876

Hamburg, 18^{ten} Mai 1876

Sehr geehrter Herr,

Erst jetzt habe ich Ihren freundlichen Brief vom 2 auf dem Umwege über Hildesheim erhalten. Seit Ostern bekleide ich nämlich die Stelle eines Oberlehrens an der hiesigen Gelehrtschule.

Zunächst bitte ich um Entschuldigung, dass ich in deutscher Sprache an Sie schreibe. Obgleich ich beim Lesen die französische Sprache für mathematische Dinge sehr liebe, so getraue ich mich doch nicht, französische Briefe zu schreiben, weil ich zu grobe Germanismen dabei fürchte, und nicht gern ein Französisch schreiben möchte, ~~dem~~ welches nach ~~sofort~~ deutscher Sprache riecht. Ich sehe ja auch, dass Sie der Deutschen hinlänglich Herr sind, um deutsche Briefe lesen zu können, da Sie den Inhalt meiner Abhandlung schon erkannt haben.

Doch nun zur Mathematik !

Die symbolische Multiplication, welche übrigens doch schliesslich von Ihnen her stammt, ist in der That eine prachtvolle Machine, deren Nutzen ich in meinen "Beiträgen zur abzählende Geometrie" wohl hinlänglich bewiesen habe. Ebenso ist aussen dem Corr.

Pr. auch das Pr. des speciellen Lage (§7 meiner Beitr.) nicht zu verachten. Leider hat es mir bei dem Wechsel meiner Stellung in den letzten Monaten an Zeit gefehlt, um die IIte und IIIte Abh. noch einmal durch zu arbeiten und fertig zu stellen. Sie kennen etwas davon vermuthlich durch unsern gemeinsamen mathematischen Freund Zeuthen. Aus dessen Mittheilungen über Sie nach seiner Pariser Reise 1874 kan mir die Furcht, Sie bei dem Kopenhagener Preise als Concurrenten zu haben. Ich würde mich sehr freuen, wenn meine Abhandlung den Anstoss dazu gegeben hätte, dass Sie Ihre Beschäftigung mit der abzählenden Geometrie, und specieller mit der eigentlichen Charakteristikentheorie wieder aufnahmen, und Ihren früheren schönen Resultaten neue hinzufügen. Die wesentlichsten Resultate meiner in diesem Sommer hoffentlich fertig gestellten II und III Abh. der ~~Beiträge~~ werden Ihnen aus der Iten Abh., aus Zeuthens Bericht über meine Preisschrift und aus Gött. Nachr. Mai 1875 ersichtlich sein.

Sehr lieb wäre es mir, wenn Sie mir ab und zu den wesentlichen Inhalt Ihrer Resultate und die von Ihnen gemachten Publikationen mittheilten. Da ich nämlich nicht in einer Universitäts- oder Bibliotheks- Stadt lebe, so kann ich gewisse Zeitschriften, namentlich auch die Comptes Rendus und das Bull. de la Soc. Math., nur durch Zuschickung von ausserhalb erlangen. Unter diesen Umständen ist es nicht mir lieb, wenn ich von den Autoren durch einige Zeiten auf ihre mich in interessirenden Publikationen aufmerksam gemacht werde. Die meiner Specialität nahe stehende deutsche mathem. Liter. empfangen ich meist durch Separatabzüge.

In der Hoffnung, dass Sie mir gelegentlich auch Ihre Resultate brieflich kurz mittheilen wollen, erlaube ich mir, Sie Ihnen etwas von dem Inhalt meiner letzten Publikation, von der ich gerade eben den letzten Correcturbogen empfangen, zu erzählen.

Sie haben im Bull. de la Soc. Math. Bd II (die pag. weiss ich nicht, weil mir die Zeitschrift in den letzten Monaten nicht zugänglich war ich werde mich doch wohl entschliessen, darauf zu abonniren) erwähnt, dass bei der Fläche zweiter Ordnung auch jede ~~mehr~~ a -fache, nicht in einzelne Bedingungen zerlegbare Bedingung durch diejenigen

$$\frac{1}{2}(a+1)(a+2)$$

Bedingungen ausgedrückt (\star) werden kann, welche nur aus μ, ν, γ zusammengesetzt sind [(in a footnote:) (\star) Ich benutze hier, der Kürze wegen, die in meinen ~~Beiträgen~~ erläuterte Terminologie.]. Haben Sie diess dort ganz allgemein bewiesen? Ich habe nun ~~eine~~ die Moduln einer gewissen Klasse solcher mehrfacher Bedingungen wirklich ~~durch~~ als Funktionen von μ, ν, ρ berechnet. Es sind diese die Bedingungen, welche aussagen, dass eins der ∞^2 auf der F_2 liegenden Geradenpaare alle möglichen Grundbedingungen ~~unter~~ erfüllt, wo unter Geradenpaar das aus Punkt der F_2 , zugehöriger Tangentialebene, und den beiden sich in diesem Punkte schneidenden ganz in der F_2 liegenden Geraden bestehende

Gebilde verstanden werden soll. Dazu gehört beispielsweise : “Der Modul der dreifachen Bedingung, eine Gerade zu enthalten, ist

$$X' = \frac{1}{4}(2\nu^3 - 3\nu^2\mu - 3\nu^2\rho + 3\nu\mu^2 + 2\nu\mu\rho + \nu\rho^2 - 2\mu^3 - 2\rho^3)”$$

Dabei ~~sind~~ ist auch bewiesen, dass zwischen den

1) <u>3</u>	Symbole, μ, ν, ρ	0	allgemeine Relationen bestehen
2) <u>6</u>	” $\mu^2, \mu\rho, \rho^2, \mu\nu, \rho\nu, \nu^2,$	0	” ”
3) <u>10</u>	” μ^3, \dots	0	” ”
4) 15	” μ^4, \dots	2	” ”
5) 21	” μ^5, \dots	21 - 13	” ”
6) 28	” $\mu^6 \dots$	28 - <u>10</u>	” ”
7) 36	” μ^7, \dots	36 - <u>6</u>	” ”
8) 45	” μ^8, \dots	45 - <u>3</u>	” ”

Diese Relationen sind theils berechnet, theils sind Methode zu ihrer Berechnung angegeben. Die Relationen zwischen den fünf- und mehrfachen Symbolen können auch durch ein nahe liegendes Eliminationsverfahren, welches dem in Clebsch-Lindemann (pag 406) analog ist, aus den Elementarzahlen der F_2 hergestellt werden. Am interessantesten sind daher wohl die beiden Relationen zwischen den 15 vierfachen Symbolen. Diese lauten in einer sich selbst dualistisch entsprechenden Form :

$$\begin{aligned} 1) & 2\nu^3\mu - 2\nu^3\rho - 3\nu^2\mu^2 + 3\nu^2\rho^2 + 2\nu\rho^3 - 2\nu\rho^3 = 0 \\ 2) & 2\nu^4 - 5(\nu^3\mu + \nu^3\rho) + 6(\nu^2\mu^2 + \nu^2\rho^2) + 8\nu^2\mu\rho \\ & - 4\nu(\mu^3 + \rho^3) - 6\nu(\mu^2\rho + \mu\rho^2) \\ & + 4\mu^3\rho + 4\mu\rho^3 = 0 \end{aligned}$$

Alle diese Relationen sind ganz allgemein, gelten also für jedes durch noch so complirte Bedingungen definirte System.

Beim Kegelschnitt im Raume, wo m , die Ebene durch einen Punkt schicken, n , eine Gerade schneiden, ν eine Ebene berühren, bedeuten möge, sind nur immerhöchstens

3 einf. und 3 siebenfache
6 zweif. und 6 sechsfache
9 dreif. und 9 fünffache
und 10 vierfache

aus μ, ν, ρ zusammengesetzte Bedingungen von einander unabhängig. Die eine Relation zwischen den 10 dreifachen Symbolen $n^3, n^2\nu, n\nu^2, \nu^3, mn^2, mn\nu, m\nu^2, m^2n, m^2\nu, m^3$ lautet

:

$$2n^3 - 3n^2\nu + 3n\nu^2 - 2\nu^3 - 6mn^2 + mn\nu + 12m^2n - 8m^2\nu = 0$$

Sie ist die Erweiterung zu Formel 11) auf pag 406 das Clebsch-Lindemannschen Werkes.

Die Abhandlung, von deren Inhalt ich Ihnen eben etwas abgeschöpft habe, erscheint im 3ten Hefte des Xten Bundes der Klein-Meyerschen Annalen. Namentlich möchte ich Ihnen noch diess hinzufügen, dass ein strengen Beweis, dass überhaupt jede Bedingung durch die Symbole $(\star) \mu^a\nu^b\rho^c$ ausgedrückt werden kann, darin nicht geliefert ist. [(in a footnote:) (\star) Ich habe nur immer gesagt : “Wenn es der Fall ist, so reichen schon so und so viel Symbole $\mu^a\nu^b\rho^c$ aus.”] Ich möchte Sie nun namentlich fragen, ob Sie diess schon bewiesen haben, oder beweisen können.

In der ersten Tagen halte ich das Vergnügen, Lindemann aus München (er ist von Michaelis ab Privatdocent in Würzburg) bei mir zu sehen und zu sprechen. Er war auf der Durchreise nach London, wo er Cayley, Hirst, Sylvester zu sprechen hofft, vielleicht auch Salmon. Lindemann ist in Folge der Bearbeitung des Capitels über Char. Theorie in seinem Werke jetzt auch etwas abzählend-characteristisch angehaucht. Wir sprachen namentlich über Ausartungen (courbes dége singulières).

Geehrter Fachgenosse, ich sage Ihnen nochmals meinen besten Dank dafür, dass Sie die Correspondenz zwischen uns in so lebenswürdiger Weise eröffnet haben, und füge die Bitte hinzu, dass Sie dieselbe mit einem, wissenschaftlich hier fast auf dem Tracknen Sitzenden Fachgenossen, nicht wie der einschlafen lassen möchten.

Ich benutze die Gelegenheit, und Sie zu ersuchen, dem ehrwürdigen Vater unserer gemeinsamen Specialität, dem Mr. Chasles den Ausdruck meiner besondern Hochachtung zu übermitteln.

Ich empfehle mich Ihnen als

Ihr ganz ergebenster

H.Schubert

in Hamburg, Baumeisterstrasse 1

B.2 May 21st 1876

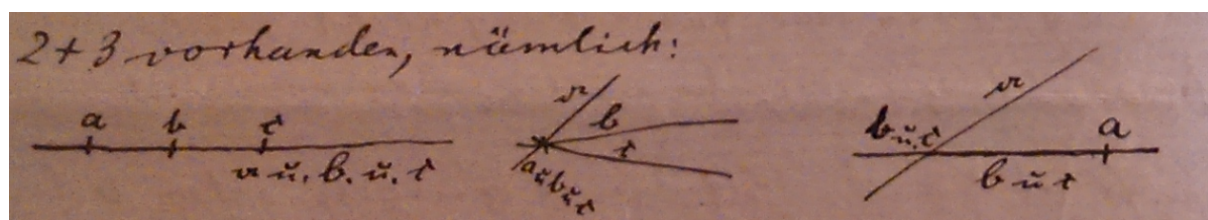
Hamburg, 21/5 76

Geehrter Herr,

Ich bin sehr erfreut, dass ich mit Ihnen nun wie mit Zeuthen Gedanken und Forschungsresultate austauschen darf.

Ich schicke Ihnen hiermit den von Klein gelesenen und dann mir zur Vergleichung zugeschickten Correcturbogen meiner Abhandlung über die Moduln gewisser Bedingungen bei der F_2 . Es ist darin eigentlich ein eng begrenztes Thema behandelt, dieses aber auch vollständig, und bei dieser Gelegenheit ergeben sich jene Relationen. Es war sehr schade, dass mir Ihre Abhandlung in Bull. de la Soc. Math. bei der Abfassung meiner Arbeit nicht zugänglich waren. Dann hätte ich pag 139 in der Anmerkung nicht geschrieben bemerkt "sondern bewiesen". So war ich zweifelhaft, ob Sie den Satz nur ausgesprochen, oder auch streng bewiesen haben. Ich bitte also, die Ungenauigkeit nicht bösem Willen zu zuschreiben. Die Formel dritter Dimension beim Kegelschnitt ~~im Raume~~ in der Ebene habe ich glücklicher Weise Cremona-Halphen'sche "genannt, und glaube" damit das Richtige getroffen zu haben (genüss den Anmerk, in Lindemann's Werke), obwohl ich Cremona's Arbeit nicht kenne.

Es befriedigt mich sehr, dass meine Gedanken über Produktsätze Sie dazu verleitet haben, Prod. Sätze für diejenigen Gebilde aufzustellen, mit denen man doch von Rechts wegen anfangen muss. Ich glaube, dass Ihre Untersuchungen, deren Inhalt Sie andeuten, mehr Klarheit in die Probleme der eigentlichen Charakteristiken bringen werden. Behandeln Sie auch Tripel? Ich dachte früher einmal daran die vor einem Dreieck erzeugten Systeme so zu behandeln, wie in meiner Abh. das Punktepaar Ausartungen sind 2+1 oder wenn man die Leitern und Ecken unterscheidet 2+3 vorhanden, nämlich:



Der Ausdruck abzählende Geometrie ist wohl sonst noch nicht angewandt. Wohl aber war schon üblich zu sagen, "Abzählungsmethoden, z.B. die v. Chasles, v. Voss etc." Ich hatte in die "Fortschritten der Mathematik" den Namen "Geometrie der Anzahl" hineingebracht. Doch gefällt mir dieser Ausdruck jetzt nicht mehr, und es soll jetzt auch dort dieses Capitel mit "abzählende Geometrie" überschrieben werden. Dieser Ausdruck ist dem sehr gebräuchlichen Ausdruck "darstellende Geometrie" (z.B. v. Fiedler) nachgebildet. "Géométrie numérique" scheint mir die einzig mögliche Übersetzung zu sein. Ich glaube, dass es auch treffend bezeichnet. Dass man den Ausdruck Charakteristikentheorie für die wirklichen Charakteristiken aufheben muss, darin werden Sie mit mir übereinstimmen.

Da Sie, wie Sie schreiben, die Beschäftigung mit den Raumcurven dritter Ordnung auch einmal angefangen haben, so interessieren Sie vielleicht beiliegende Blätter aus einer

kleinen Note, welche ich aber dann doch nicht veröffentlicht habe. Die Ausartungen der ent. Raume kommen natürlich erst in die IIIte Abh. der “Beiträge”.

Ich habe mit einem jungen Mathematiker Hurwitz in Hildesheim viel gesprochen über Methoden, durch welche man [?] mit Hilfe des Chasles’schen Cor. Pr. und des Pr. des spec. Lage zur Bestimmung der Anzahl der gemeinsame Elemente zweier Systeme direct gelungen kann. Dies ist ihm in der That für Kegelschnitte und Flächen IIⁿ Or. gelungen. Die Zahl der gemeinsamen Flächen eines 1- und eines 8-stufigen Systems gewinnt Hurwitz durch das Cor. Pr. in folgender Form:

$$\begin{aligned} 2\mu(\mu_1^8) + 2\mu_1(\mu_1^8) &= 2\varepsilon + 2\mu \cdot (\mu_1^8) \\ &+ 2(2\mu - \nu) \cdot \frac{1}{8}\mu_1^3\nu_1^4(2\mu_1 - \nu_1) \\ &+ 2(2\nu - \mu - \rho) \cdot \frac{1}{10}\mu_1^6\rho_1(2\nu_1 - \mu_1 - \rho_1), \end{aligned}$$

wo ε die gesuchte Zahl ist, und die übrigen Symbole wohl nicht erklärt zu werden brauchen. Das $2\mu - \nu$ resp. $2\nu - \mu$ stammt natürlich von den Ausartungen her.

Hurwitz (Hildesheim, Am Hagenthor) spricht auch folgenden Satz aus:

“Sind die 9 einfachen Bedingungen b_1, \dots, b_9 Charakteristiken der einstufigen Systeme einer Fläche beliebiger Ordnung, so bilden die aus diesen 9 Bedingungen zusammengesetzten a -facher Bedingungen, oder ein Theil von ihnen, wieder eine Gruppe eigentlicher Charact. für die Systeme a -ter Stufe.”

Dieser Satz ist sehr nahe liegend. War es Ihnen in dieser Allgemeinheit bekannt ?

Ich habe in der letzten Zeit einmal angefangen, die Plancurve dritter Ordnung, vierter Klasse, in ähnlicher Weise hinsichtlich ihrer Ausartungen zu behandeln, wie ich in den Gött. Nachr. Mai 1875 die C_3^3 behandelt habe, um so meine IIte Abh. ~~noch~~ zu vervollständigen. Ich sehe jedoch dabei zu viel mühsame Arbeit, und werde es wohl lassen. Interessant ist, dass man so zu den geometrischen Beziehungen zwischen den singulären Elementen der Curven gelangen kann. Z.B. Für die C_3^3 :

“Auf jeder Geraden der Ebene werden von einer C_3^3 3 Schnittpunkte der Curve, 1 Schnittp. mit d. Wendet., 1. Schnittp. mit d. Rückkehrtang, u. 1 Schnittp. mit d. Verbindungsgeraden von Wendep. und Spitze, im ganzen 6 Punkte bestimmt. Zwischen diesen bestehen 2 Relationen, so dass z.B. die 4 ersten Punkte die beiden letzten 4 deutig bestimmen etc.”

Leider habe ich hier noch immer sehr viel zu thun, so dass ich, auch viel Touren machend (heute Nachmittag z.B. die Elbe abwärts nach Blankenese), in Hamburg noch nicht ordentlich zum privaten Arbeiten gekommen bin.

Mit den besten Grüßen

hochachtungsvoll

Ihr Schubert

B.3 July 12th 1876

Hamburg, d. 12/July 76

Sehr geehrter Herr,

Es ist Zeit, dass ich Ihnen antworte. Ich thue dies kurz vor einer Reise mit Frau und Kind nach Potsdam, wo ich

Hoditzstrasse 2

wohnen, und vier Wochen, bis 12. August, bleibe werde.

Besten Dank für Ihre inhaltreiche Abhandlung, in die ich mich allmählich hineinfinde.

Gegen den Beweis des Ihnen mitgetheilten Satzes von Hurwitz hatte ich einen gewissen Einwand, welchen ich auch gegen Lindemann's Beweis für die Zahl der einen zwei- und einem dreistufigen Systeme gemeinsamen Kegelschnitte (Clebsch's Vorles.) erlebe. In folge dessen modificirt sich der allgemeine Satz von Hurwitz zu folgendem :

“Wenn bei einer Plancurve, Raumcurve, Fläche q einfache Bedingungen existiren, durch welche jede Ausartung (courbe, surface, singulière) ausgedrückt werden kann, so sind diese q Bedingungen die einstufigen Charakteristiken, und die aus ihnen zusammengesetzten a -fache Bedingungen die a -stufigen Charakteristiken.”

Ich fürchte nur, dass die Voraussetzung dieses Satzes ausser bei Kegelschnitt und quadrat. Fläche, bei nur wenigen Gebilden erfüllt werden kann.

Die Fouret'sche Priorität erkenne ich an. Ich freue mich sehr, inder für das “Jahrbuch über die Fortschritte der Math.” zu bearbeitenden Literatur mehreres von Fø diesem Gelehrten zu finden, namentlich auch die Arbeit über Flächensysteme und implexes de surf. Seine nachträglich in meinem "Beiträgen zur abz. G." erwähnte Arbeiten über den Zusammenhang der Curvensysteme mit den Differentialgleich. erst. Ordnung werde ich aus dem II. Bande des Bulletin nächstens studiren. Aus diesem II. Bande, dem ich mir per Buchhandel kommen lasse, werde ich auch noch einmal genaue Kenntniss über Ihre Arbeiten nehmen.

Ich bin erfreut, dass im Kreise der société mathématique meine Sachen Interesse erregen. Namentlich freut es mich, dass ich auch bei dem vielseitigen M. Camille Jordan Anerkennung finde.

Hurwitz und ich haben jetzt die Formeln aufgestellt, welche zwischen den Grundbedingungen von drei in gerader Linie, von vier in einer Ebene befindlichen Punkte und von

fünf in einer Strahlenaxe befindlichen Strahlen bestehen. Nur der letzte Fall macht einige Schwierigkeiten.

Haben Sie auch wohl versucht, Produktsätze aufzustellen für das Gebilde welches aus drei in geraden Linie befindlichen Punkten besteht ?

Sie schreiben, dass es leicht sein müsste, durch Rechnung die geometrischen Beziehungen zu beweisen, welche sich Ihnen im vorigen Briefe über die Plancurven dritter Ordnung mittheilte. Ich bin nicht im Stande, durch gewöhnlichen Calcul den Grad der bezüglichen Gleichung zu erkennen.

Vor einigen Wochen hatte ich einen Ruf als ord. Prof an das Polytechn. zu Darmstadt an Stelle von E.Schröder erhalten, ihn aber wegen der Kargheit des dortigen Budgets ablehnen müssen. Die Professoren in Deutschland werden noch gar zu schlecht bezahlt, mit wenigen Ausnahmen.

Ich hoffe, dass ich während der Ferien in Potsdam etwas mehr zum Arbeiten komme, als dies bisher hier der Fall war. Klein drängt mich, die IIte und IIIte Abhandlung fertig zu stellen. Meine Abhandlung über die Moduln vielfacher Bedingungen bei den F_2 muss nun nächstens erscheinen. Ich schicke Ihnen dann einen Separatabzug zu. Wie sind die Bedingungen für den Beitritt ausländischer ~~Mit~~ Mathematiker zur Société mathématique ?

Ich ersuche Sie, Herrn Fouret und Herrn Jordan ~~meine~~ mich bestens zu empfehlen. Was macht eigentlich Herr Maillard ? Ich habe von ihm seit seiner berühmten Doctor-Dissertation über die Charact. (bester wohl "Elementaren Anzahlen") der cub. Plancurven nichts wieder gehört.

Haben Sie schon von dem eben erschienenen ersten Hefte des mathem. Reportoriums für Autoren Referate von Königsberger-Zeiner Kenntniss genommen ?

Wenn Sie ihm nach Postdam (Hoditzstrasse 2) einen wenn auch nur kurzen Brief schicken, würden Sie sehr erfreuen.

Ihren ganz ergebenen
H. Schubert

B.4 August 12th 1876

Potsdam, 12 August 76

Verherter Herr,

Ich habe bis zum Schluss meiner Ferien mit meiner Antwort auf Ihren liebenwürdiger Brief gezögert. Sie werden dieses erklärlich finden, wenn Sie hören, dass ich hier, in

meiner und meiner Frau Heimath, in einem bunten Gewirr von Land und Wasserparthieen, gesellschaftlichen Zerstrennungen aller Art, Fahrten nach Berlin, Besuchen von Berlin etc. die 4 Wochen verbraucht habe. Um einmal geistig Ruhe und Erholung zu finden, habe ich mit einem Collegen aus dem Harz eine Woche lang sehr ermüdende Touren durch den Oberharz gemacht, die den Körper ermüdeten, mir aber die geistige Abgespanntheit nahmen.

Sie haben, Ihnen Briefe zufolge, jedenfalls eine stillere, geräuschlosere Sommerfrische gehabt, als ich. Der Mangel an Verkehr mit mathematischen Verwandten ist etwas, was ich von Hildesheim her und auch von Hamburg her besser kenne als irgend einer. Vor einigen Tagen habe ich einen ganz hübschen Nachmittag hier verlebt mit fünf Mathematikern, Ohrtmann, Müller, Netto, Schemmel, Mainz, alles Mitarbeitern an dem Jahrbuch für die Fortschr. der Math., die mich von Berlin aus hier besuchten.

Besten Dank für Ihre gütigen Mittheilungen über die Statuten der Société math. de France. Ich theile vollkommen Ihre Ansicht, dass jedwede Vermengung des Politischen mit dem Wissenschaftlichen bedauernwerth ist. Ich ziehe ferner aus Ihren Briefe den Schluss, dass Sie gewissermaassen dafür bürgen können, dass meiner Aufnahme in die Société kein Widerspruch entgegengesetzt werden wird. Darauf fassend, ersuche ich Sie hierdurch, mich bei zur Aufnahme in die Société gefälligst vorschlagen zu wollen, mit dem Bemerken, dass ich es mir zur hohen Ehre anrechne, Mitglied einer Gesellschaft zu sein, welche so viele mathematische Grössen enthält. Sollten Sie jedoch fühlen, dass gegen meine Aufnahme irgend welcher Einspruch erhoben werden könnte, so haben Sie wohl die Güte, die présentation zu unterlassen.

Zugleich bin ich beauftragt, bei Ihnen im Vertrauen anzufragen, ob auch Herr Oberlehrer Dr. Ohrtmann in Berlin, der Redacteur des Jahrbuchs der Fortschr. der Math., Aussicht hat, Mitglied werden zu können. Derselbe verfolgt dabei den Nebenzweck, so schneller in den Besitz des Bulletin gelangen zu können, in den er, trotz des angebotenen Austausches, so viel ich weiss, immer noch nicht gelangt ist.

Ich habe mir jetzt den II. Band des Bull. wegen Ihrer, Saltel's u. Fourret's Abhandlungen buchhändlerisch zukommen lassen, zu dem enormen Preise von 18 Reichsmark = 22 $\frac{1}{2}$ francs. Es wäre mir lieb, wenn ich den I. Band, den ich auch zu Referaten für das Jahrbuch nächstens das Jahrbuch nächstens brauche, direct oder als Mitglied billiger bekommen könnte. Natürlich braucht ihn nur einer, der Redacteur Ohrtmann oder ich.

Doch nun zu den Relationen zwischen den Singulären! Sie haben Recht, bei der C_3^3 ist es nicht schwer, die Gleichungen selbst aufzustellen. Doch in weiteren Fällen ist die Angabe der Gleichung nach meiner Meinung ein noch nicht gelöstes Problem. Ich wähle als Beispiel die Beziehung zwischen den 12 Tangente, die von einem Punkte an eine C_4^{12} möglich sind. Schon Zeuthen findet (Ahm. Egensk. Pag 391), dass durch

11 solcher Tangenten die zwölfte 451440-deutig bestimmt wird. Ich kann zu Zeuthens Resultaten hinzufügen, dass wenn sechs dieser Tangenten zusammenfallen, diese vielfache durch die übrigen 6 eindeutig, aber eine dieser 6 durch die übrigen 120-deutig bestimmt wird. Ebenso bekommt man auch schon hohe Zahlen bei der C_3^4 .

Sie haben verstanden, dass Fouret im Königsbergerschen Journal referirt hätte. Ich meinte, dass ich über Fouret's Arbeiten zu referiren hätte in Ohrtmann's Jahrbuch, und meine Erwähnung Königsberger's war unabhängig davon.

In Hamburg haben wir vom 17 bis 24 September Naturforschersammlung, wozu Zeuthen wahrscheinlich kommt, ebenso einige deutsche Mathematiker. Mit herzlichen Grüßen und mit der Bitte, die Verzögerung dieser Antwort auf Ihren lieben Brief zu entschuldigen

Ihr ergebenster Schubert.

B.5 November 5th 1876

Hamburg, d. 5. Nov 76

Verehrter Herr Professor !

Zunächst besten Dank für Ihre werthvollen Abhandlungen.

Wenn ich in der Soc. Math. de France auf Ihren Vorschlag gewählt werden sollte, werde ich hierhin eine grosse mir erwiesene Ehre und zugleich einen Ausdruck unsrer gemeinsamen Ansicht sehen, dass es in der exactesten aller Wissenschaften keine Nationen geben darf.

In Namen Ohrtmanns ersuche ich Sie, in der Sitzung vom 15 Nov. dafür zu sorgen, dass ihm ~~umgehend~~ wenigstens der Ite Band des Bulletin de la Soc. Math. geschickt werde. Er wird ihn mir dann schicken, damit ich noch schnell die Referate über die darin enthaltenen Abhandl. anfertigen kann. Es sollen nämlich die Referate bis spätestens Ende November eingereicht sein. Insofern hat es Eile. Unter diesen Umständen, möchte ich mir augenblicklich den Iten Band nicht anschaffen, vielleicht später einmal. Den II. Band besitze ich. Wenn ich Mitglied werden sollte, werde ich ja wohl immer die Hefte des V den Bundes sofort direct erhalten.

Dass Cremona in Frankreich war, hörte ich durch Sturm, beidem er mehrere Tage war. Während der hiesigen Naturf. Versaml. hatte ich nette und anregende Studien durch die Anwesenheit von Zeuthen, Schröter, Lüroth. Ein sehr kurzes Referat über einen in der math-astron. Section der Natf. Vers. von mir gehaltenen Vortrag liegt hier bei. Es ist ein Ihnen bekanntes Gegenstand.

Ich habe jetzt eine Abh. fertig, (die Redaction halb fertig) über die Singularitäten-Zahlen der allg. Ordnungsfläche F_n , welche sich auf die in demselben Punkte berührenden

Haupttang und Döppelt etc. beziehen. Danach ist Clebsch's Resultat in Crelle's Journal Bd.63, pag 14, unrichtig (cf. auch Salmon's Buch). Die Zahl der Punkte einer F_n , wo beide Haupttangente vierpunktig berühren, ist

$$5n(7n^2 - 28n + 30)$$

Clebsch's Fehler ist leicht zu finden, er ~~hat~~ nimmt einmal 1 als Coefficienten, statt 3.

Auf Ihre Note gegen $\alpha\mu + \beta\nu$ war ich schon durch Zeuthen, und durch Klein gleich nach ihren Erscheinen aufmerksam gemacht. Ehe ich mir aber das betreff. Heft aus der hiesigen, sehr schwerfälligen Stadt-Bibliothek verschaffen konnte, bekam ich am vorrigen Dienstag Ihre gütige Zusendung. Am denselben Abend noch annoncirte ich Stern eine Note für die Gött. Nachr., die ich Mittwoch und Donnerstag abfasste, und die er Sonnabend der Göttinger Societät vorgelegt hat. Vielleicht aus einer gewissen Pietät gegen Chasles's Satz, habe ich in dieser Note durch Publikation des von mir und Hurwitz gefundenen Beweises von $\alpha\mu + \beta\nu$ eine Lanze – vielleicht die letzte – ~~zu brechen~~ für diesen interessanten Satz zu brechen versucht.

Sie werden es vielleicht abenteuerlich finden, ~~einen Satz~~ den Beweis eines Satzes zu publiciren, der eben in seiner Allgemeingültigkeit angegriffen ist. ~~Ieh~~ Die Hauptsache ist war mir aber die Liebe zur Wissenschaft. Ich glaube, dass durch meine Note, auch wenn dieselbe Irrthüme enthält, die Sache ~~geklärt~~ noch mehr aufs Tapet gebracht wird, noch mehr Interessenten und Mitarbeiter wirbt, die vielleicht von noch andern Standpunkten die Sache beleuchten. So publicirte ich den Beweis und einige Bemerkungen über den Sinn des Satzes, veranlasst durch Ihren Angriff, selbst auf die Gefahr hin, dass Sie durch Ihre ausführliche Abhandlung mir beweisen, dass ich mich total geirrt habe, was ich gut werde tragen können. Dazu kann mein begründetes Vertrauen darauf, dass Sie meine Vertheidigung des Satzes nicht als Eröffnung einer persönlichen Polemik gegen Sie auffassen werden.

In der Hoffnung, dass Sie mir Ihr gütiges Wohlwollen auch ferner bewahren werden, ersuche ich Sie, Herrn Chasles und der Société, deren Mitglied bald zu sein ich mich freue, meine Hochachtung aussprechen zu wollen, als

Ihr ergebenster Schubert

B.6 December 4th 1876

Hamburg, d. 4/Dec. 1876

Verherter Herr,

Besten Dank dafür, dass Sie mich in der Soc. Math. vorgeschlagen, und meine Wahl mir mitgetheilt haben. Freilich hat doch Herr Fouret den Ruhm, Ihnen in dieser Mittheilung zuvorgekommen zu sein. Ich werde nächstens diese officielle Benachrichtigung des Secretariats dankend beantworten, so dass meine Antwort vor der nächsten Sitzung in Händen des Herrn Fouret ist.

Ich freue mich sehr, dass Sie in meiner Aufregung gegen eine Anzweiflung des $\alpha\mu + \beta\nu$ keine persönliche Entgegnung gegen den Zweifel selbst sehen wollen. Sie werden meine Note mit dem Beweise von Hurwitz in Händen haben. Ich habe jedoch Ihren Brief allein, ohne die erwähnte Note in den C.Rend. erhalten. Ehe ich desswegen bei der Post eine sogenannte Reclamation einreiche, möchte ich noch einmal anfragen, ob Sie die Note auch schon gleichzeitig mit dem Briefe abgeschickt haben. Es thut mir leid, dass Sie die ausführliche Abhandlung schon in das Journ. de Math. abgeschickt haben. Sonst hätten Sie vielleicht noch Gelegenheit nehmen können, den Fehler in unserem Beweise dort anzugeben. Sie werden aus der Gött. Note ersehen haben, warum mich Ihre Beispiele noch nicht von der Unrichtigkeit des $\alpha\mu + \beta\nu$ überzeugt haben, und ich fasste meine Note hauptsächlich deshalb ab, um die Sache ordentlich, auch in Deutschland, zur Sprache zu bringen, damit schliesslich ~~durch~~ aus dem Widerstreit der Meinungen die Wahrheit Frieden stiftend sich erlebe. Darum thut es mir leid, dass Sie meiner Vertheidigung des $\alpha\mu + \beta\nu$ in Ihrer ausführlich Abh. keine Widerlegung folgen lassen können. Über die Sache selbst kann ich deshalb noch nicht sprechen, weil ich Ihre Note noch nicht habe.

Für das aus Strahl und darauf liegendem Punkte, und das aus Ebene und darauf liegendem Punkte habe ich inzwischen auch Produktsätze erhalten. Ich bemerkte auch wie Sie, dass für höhere, aus einer endlichen Zahl von Hauptelementen zusammengesetzte Gebilde die Produktsätze bald aufhören, wenigstens, insofern man bloss von fundamentalen Bedingungen abhängig darstellen will.

~~Das~~ Den an Ohrtmann durch Ihre gütige Vermittelung geschickten Heft Bd.I des Bull. habe ich, ebenso wie Bd.II, in der letzten Zeit zu Referaten viel benutzt. Über ihre Charakteristiken-Abhandlungen habe ich das Referat wegen Ihrer neuen Beschäftigung mit diesem Gegenstande noch verschoben bis zum nächsten Bande. Übrigens ~~ist~~ habe ich über Ihre Academie-Berichte über denselben Gegenstand schon früher referirt.

Wie kommt Lindemann jetzt nach Paris ? Ich vermuthete ihn in Würzburg. Grüßen Sie ihn, bitte, wenn er noch dort sein sollte. Ich freute mich, endlich die IIte Abh. seiner Vorl. v. Clebsch gedruckt zu sehen. Bitte, grüssen Sie auch Hirst, dessen Sachen ich meist kenne, den ich zwar nicht persönlich kenne, von dem ich aber wohl durch Sturm sehr viel gehört habe.

Meine Abh. über die Tangenten-Singularitäten, bei der es mir weniger auf einige neue Resultate, als auf die Methode (nur Punktepaar-Formeln) ankam, liegt schon bei Teubner

für die Math. Ann.

Sie sprechen von Saltel (*). [*in a footnote:*] (*) Seine Mittheil an die Acad. de Belgique sind mir hier unzugänglich.] Nun habe ich in den letzten Wochen mich viel mit Saltel's Erweiterung des Correspondenzprinzips beschäftigt. Ich habe dieselbe mit meiner in den Beitr. z. abz. Geom. niedergelegten Auffassung desselben zu einer grösseren Erweiterung verschmolzen. Dadurch ergibt sich z. B. die Zahl der fünfpunktigen Tangenten einer allgemeinen Ordnungsfläche F_n $5n(7n-12)(n-4)$ direct durch blosser Einsetzung der Zahl 5 und der Zahl n in eine der Formeln. Ich denke, Ich werde in dem Dankschreiben an die Soc. Math. ein Blatt beilegen, worauf ich einige kleine Resultate angebe. Vielleicht könnte ich als membre diese Dinge sogar in dem Bulletin veröffentlichen. Freilich müsste mein Französisch dann noch einer corrigirenden Hand unterbreitet werden. So sehr ich auch an Französisch lesen gewöhnt bin, so wage ich es doch nicht, französisch zu schreiben.

Für den Fall, dass Sie mich um Weihnachten mit einigen Zeilen zu erfreuen beabsichtigen, bemerke ich, dass ich in den Ferien, d.h. vom 23. Dec. Bis 7. Januar mit meiner Familie in dem deutschen Versailles, das heisst Potsdam, wohnen werde, und zwar

Hoditzstrasse 2.

Noch eine Bitte! Wenn ich die Ehre habe, mit Männern wie Sie zu correspondiren, so verbinde ich gern mit dem Adressaten einer räumliche Vorstellung, die sich am besten durch die Übersendung einer Photographie erreichen lässt. Wenn Sie mir also Ihre Photographie schicken wollten, so bin ich Ihnen sehr dankbar, und zu einem Gegendienste gern bereit.

In der Hoffnung, dass Sie mir Ihr Wohlwollen auch ferner bewahren werden, zeichne ich

hochachtungsvoll
Ihr ergebenster
Schubert

B.7 July 9th 1877

Hamburg, 9/7 77

Verehrter Herr Professor,

Es ist geraume Zeit her, dass ich Ihnen gegenüber etwas habe von mir hören lassen. Inzwischen habe ich durch ein genaueres Studium des Règlement administratif der Société math. de France entdeckt, dass ich wohl selbst daran schuld war, dass ich bisher noch kein Heft des Bulletin erhalten hatte, und habe meine Versäumnis nachgeholt

durch Übersendung des Eintrittsgeldes und der cotisation annuelle an den trésorier der Gesellschaft. Bis jetzt habe ich keine Antwort erhalten. Sie werden fragen, womit ich mich beschäftigt habe. Mit vielerlei. z.B. auch, untreu meiner Spezialität, mit der Frage der Nicht-Existenz ungerader vollkommener Zahlen. Können Sie vielleicht angeben, wo Herr Carvallo, welcher in den Comptes Rendus Bd.81, p.73 bis 75 einen Beweis dafür anbündigt, denselben publicirt hat ?

Nachstens werden 2 Abhandlungen von mir in den Math. Ann. Bd. 12, Heft 2 erscheinen. Die Correcturbogen habe ich eben gehabt. Die erste giebt die Correspondenzformeln für Gruppen von n Punkten, die auf einer beweglichen Geraden liegen, und das Analogue für n Strahlen, so dass das Princ. d. la Corresp. Analyt. des Herrn Saltel specieller Fall einer der Formeln wird. Die zweite Abhandlung wendet die Formeln für Strahlengruppen auf den Strahlencomplex an, und findet viele Singularitäten-Zahlen des Complexes n -ten Grades. Z.B. Die Zahl derjenigen ebenen Complex-Curven, welche einen fünffachen Punkt besitzen, ist:

$$\frac{1}{6}(n-5)(n-6)(n-7)(n-8)(n-9)(n-2)(n^3+8n^2+19n-12).$$

Endlich bin ich in letzter Zeit zu den Produktsätzen zurückgekehrt, und habe die Charakteristikentheorie einiger Gebilde erledigt, welche aus Punkten, Ebenen und Strahlen in endlichen Anzahl zusammengesetzt sind. Sie schrieben mir über diese Dinge einmal im Anschluss an meine Bemerkungen über Produktsätzen in den “Beiträgen z. abz. Geom.”. Ich habe die Fragen so gefasst, dass der Titel dieser letzten Arbeit ist : “Geometrische Verallgemeinerungen des Bezoutschen Fundamentalsatzes”. Die darin enthaltenen Formeln geben zu manchen kübischen Anwendungen Anlass. So ~~sind~~ gehen das bekannte $\phi \cdot \mu + \gamma \cdot \nu + \sigma \cdot \rho$ für die Berührung einer Fläche durch Flächen eines Systems, ferner die Resultate des Herrn Fouret (Comptes v.80, 805-809) etc. unmittelbar aus meinen Strahlbüschel “Characteristiken” Formeln hervor. Diese Resultate sind Sonnabend der Göttiger Societät vorgelegt, und werden also nächstens in den Gött. Nachr. erscheinen. Wenn ich Zeit finde, und die Soc. Math. es gestattet, so publicire ich die sich daran anschliessende ausführliche Abhandlung vielleicht durch das Bulletin, wie ich kürzlich auch Herrn Fouret schrieb, da ja das Tragengebiet der Zahlen der gemeinsamen Elemente gegebene Mannichfaltigkeiten grösstentheils französisches Terrain ist.

Ist Ihre Abhandlung “über die Reductionen des $\alpha\mu + \beta\nu$ bei Annahme gewisser Singularitäten des gegebenen ein- und vier-stufigen Kegelschnittssystems” noch nicht erschienen ? Wenn dies der Falle sein sollte, so würden Sie mich sehr verbinden, wenn Sie mir einen Abzug schickten, da mir von der französisch-mathem. Literatur nur die Comptes-Rendus zugänglich sind.

Mit grosser Freude habe ich die Zeuthensche Preisaufgabe begrüsst. Sie sind durch

Ihre bisherigen Arbeiten gewiss am meisten für die Lösung befähigt. Er fragt sich nun, ob Sie für acad. Preisschriften Zeit und Lust haben. Jedenfalls würde durch die Lösung eine wichtige Brücke von unserer Spezialität nach dem jenseitigen algebraischen Ufer geschlagen sein.

Ihr ergebenster
H.Schubert

B.8 August 7th 1877

Potsdam, d. 8. August 1877

Sehr geehrter Herr Halphen,

Besten Dank für Ihren liebenswürdigen Brief von 13.Juli, und die Zusage Ihrer Initiative, und das Bulletin des Soc. Math. in meine Hände gelangen zu lassen.

Inzwischen habe ich mich einige Wochen lang durch die Redaction der IIten Abhandlung der Beiträge absorbiren lassen. Ich stehe jetzt bei der Erzeugung der 13 fundamentalen Ausartungen der C_3^3 aus der allgemeinen C_3^3 . Eine Woche lang habe ich wieder Fusstouren auf dem Oberharz gemacht, die mich immer sehr erfrischen.

Ich fühle mich sehr geehrt durch die Bereitwilligkeit, mit welcher Sie meine Arbeit über die geometrischen Verallgemeinerung des Bezoutschen Satzes in das Bulletin aufnehmen, und Sich der Last des Stilpolirens unterziehen wollten. Doch war ich noch nicht so weit, um die ausführlichere Arbeit dem Druck übergehen zu können. Ich wollte auch erst die vorläufige Mittheilung der Resultate in der Gött. Nach. (Juliheft) erscheinen lassen. Dann aber möchte ich nun, dem berechtigten Drängen F. Klein's nachgebend, zunächst die Fortsetzung der Beiträge zum Druck bringen. Diese Dinge sind im wesentlichen vor nunmehr drei Jahren gefunden, und da so ist es rathsam, die Publication nicht noch weiter zu verschleppen. Die ~~Correcturbogen~~ Separatabzüge der "Verallg des Bez Satzes" erwarte ich nächstens. Sie finden dort das $\alpha\mu + \beta\nu$ für den Strahlbüschel, das Punktepaar u.s.w. Sie schrieben mir früher schon einmal von einer derartigen Ausdehnung der Produktsätze. Veröffentlicht haben Sie darüber nichts ?

Die ~~Corre~~ Separatabzüge des "Corr. Pr. für Gruppen" und der "Singul. des Liniencomplexes" muss ich auch nächstens bekommen.

Zeuthen's Preisthema verlangt die algebraische Ableitung der bekanntlich einen sechs Zifferigen Grad besitzenden Gleichung zwischen den 12 Tangenten von einem Punkte an die allgemeine Plancurve vierter Ordnung, wovon Sie mir auch einmal schrieben.

Über Carvallo's Arbeit habe ich noch nichts näheres erfahren. Ich muss es jedoch, weil ich einem ~~Herrn~~ Philologen, der einem Abriss über die Geschichte der "vollkommenen

Zahlen" verfasst, die neuste Literatur mittheilen soll. Ich schliesse etwas eilig, um nach Berlin zu fahren, wo ich mir die neueste math. Literatur ansehen will. In der Hoffnung, recht bald von Ihnen einige Zeilen zu erhalten,

Ihr ganz ergebenen
Schubert

B.9 December 3rd 1877

Montag, d. 3/12

Geehrter Herr,

Für heute belästige ich Sie nur mit einer dringenden Bitte, nämlich der, in der Société Mathématique gefälligst zur Sprache bringen zu wollen, dass ich, obwohl ich seit länger als einem Jahre durch Ihre Güte membre bin, und die cotisation annuelle statutenmässig geleistet habe, bis heute weder irgend ein Bulletin Heft noch das Diplom noch sonst ein officielles Lebentzeichen der Gesellschaft empfangen habe. Indem ich nochmal auf privaten Wege, diese Angelegenheit zu ordnen versuche, ersuche ich Sie, mit dem Secretär der Gesellschaft zu sprechen, und mir gefälligst mitzutheilen, was die Ursache dieser Verzögerung ist.

Endlich habe ich die IIte Abhandlung meiner "Beitr. Zur Abzähl. Geom." abgeschickt, worin die cubischen Plancurven behandelt sind, und, wie ich glaube, nur einige neue Gedanken ausgesprochen, ~~und~~ aber viele Zahlen berechnet sind, die man für weitere Studien brauchen wird. Haben Sie vielleicht meine "Geom Erweiterung des Bezoutschen Satzes" einmal angesehen. Ich habe kürzlich auch einmal die Fouret'schen und Saltel'schen Sachen über die Zahl der gemeins. endlichen Wurz. der Gleichungen gelesen. Ich glaube, da ist noch nicht alles erledigt.

Sehr in Eile, bitte ich Sie, meine schnelles Abzubrechen entschuldigen zu wollen.

Hochachtungsvoll,
Ihr Schubert

P.S. Sollte M.Brisse einen von Ohrtmann an ihn geschickten Brief nicht bekommen haben ? Ohrtm. wartet seit lange auf Antwort.

B.10 November 15th 1879

Hamburg, 15/11 79

Sehr geehrter Herr Halphen,

Ihr Brief und Ihre neueste Abhandlung, wofür besten Dank, traf gerade hier ein, als ich durch die Geburt einer Tochter erfreut wurde. Sie werden daher begreifen, dass ich in der letzten Zeit nicht viel zum Schreiben an meine verehrten Fachgenossen kam. Ausserdem hatte ich die Redaction einer französischen Note angefangen, und diese wollte ich erst vollendet haben, ehe ich Ihnen antworten.

Zunächst versichere ich Ihnen, dass ich Ihre frühere wie Ihre jetzige Abhandlung mit grossen Interesse gelesen habe, dass Sie mich ferner davon überzeugt haben, dass $\alpha\mu + \beta\nu$ in den und den Fällen die und die Reduction erleidet. Eigentümlich ist nun, dass ich von Ihnen studirten, dritten Kegelschnitt Ausartung schon vor Monaten von einer ganz anderen Seite her näher rückte, ohne dieses zu merken. Ich habe nämlich die Characteristikenformeln des Dreiecks aufgestellt, und hatte dabei fortwährend Rücksicht zu nehmen auf diejenige zweistufige Dreiecks-Ausartung, bei welcher die drei Ecken a, b, c einem und demselben Punkte unendlich nahe liegen, und zugleich die drei Seiten einer und derselben Geraden unendlich nahe liegen (Beispiel : das aus 3 unendlich nahen Punkten einer Curve bestehende Dreieck). Auch diese Ausartung ist durch den Punkt und die Gerade noch nicht vollkommen bestimmt, sondern es muss noch eine Bedingung hinzutreten, pour déterminer ce qu'on pourrait nommer ~~la courbure~~ du triangle infiniment petit. Analog beim unendlich kleinen Viereck muss eine zweifache Bedingung dazu treten. Inwiefern diess mit Ihren Resultaten in einigem Zusammenhang steht, ist hieraus vielleicht noch nicht deutlich, es wird aber ~~aus meiner Arbeit~~ aus der Fortsetzung meiner Untersuchungen klarer hervortreten.

Meine Abhandlung über Dreiecks-Characteristikenformeln werde ich nächstens an die Math. Ann. schicken. Eine vorläufige, kurze Mittheilung der Resultate habe ich in französischer Sprache (der Stil mag allerdings plus allemand que français sein) abgefasst, und gestern Abend an Chasles als den Alterspräsidenten der Characteristiken abgeschickt, mit der Bitte, die Note der Academie für die Comptes rendus vorzulegen. Sollte Chasles, was ich befürchte, diese meine Bitte nicht erfüllen können, so habe ich ihn ersucht, meine Note nach der Durchsicht, Ihnen und Herrn Fouret zuzustellen. Meine Resultate scheinen zwar sachlich sehr einfach, und die 3 Probleme, die Zahl der gemeinsamen Dreiecke eines i -stufigen und eines $(6-i)$ -stufigen Systems zu bestimmen, sehr leicht. Die Formeln enthalten aber doch immer in den 3 Fällen bezüglich 7, 17, 22 Anzahlen aus jedem der beiden gegebenen Systeme ; und aus einigen Anwendungen sieht man deutlich, dass die Formeln wirklich neue Wahrheiten enthalten.

Mein Buch ist inzwischen bei Teubner in Leipzig erschienen, ein Exemplar habe ich der Société Math. de France dedicirt.

Es war mir noch nicht möglich, Ihre Modificationen des $\alpha\mu + \beta\nu$ zu berücksichtigen,

da der VIte Abschnitt schon vor $1\frac{1}{2}$ Jahren von mir ausgearbeitet vorlag, und ich auch sie Sache noch nicht mit meinem Calcul beherrschen konnte. Im Literaturen habe ich Ihre Sachen natürlich erwähnt.

Mit bessten Grüssen und der Versicherung besonderer Hochachtung

Ihr ergebenster Schubert

B.11 November 23rd 1879

Hamburg, 23/11 79

Sehr geehrter Herr Halphen,

Ich empfangen soeben Ihren freundlichen Brief nebst Ihren Bemerkungen zu meinen Charakteristikenformeln im VIten Abschnitt meines Buchs. Ich bin sehr erfreut zu sehen, dass Sie und die Gesellschaft in Paris von meinem Buch Notiz genommen haben.

Ich werde sogleich meine Antwort auf Ihre Beispiele, und einige Winke über illusorisch werdende Anwendungen jener Formeln diesem Briefe beilegen. Ich ersuche Sie, meine Antwort auf Ihre Note der ~~Gesellseh~~ Société vorzulegen (★) und im Bulletin Ihrer Note folgen zu lassen im Fall Sie die Veröffentlichung Ihrer Note für nothwendig hatten [*in a footnote:*] (★) Etwaige Incorrectheiten des Stiles haben Sie wohl die Güte selbst zu corrigiren.]. Vielleicht wird dadurch der Mathematik ein guter Dienst geleistet, da durch die Entgegnung und die Vertheidigung die Sache klarer gestellt wird. Sollten Sie nicht davon überzeugt werden dass,

$$(\Sigma\Sigma') = 2 \text{ und nicht } = 1, \quad (\text{B.1})$$

so ersuche ich Sie umgefällige, briefliche Mittheilung Ihrer Gründe.

Was sagen Sie zu pag.14 meines Buches ? Herr Zeuthen hält die Ableitung für ebenso einfach wie exact, und baut nach derselben Methode mehrere Zahlen über doppelte und über dreifache Berührung auf, die ich durch meine Dreiecks-Characteristiken auch erhalten kann. Herr Zeuthen ist bekanntlich sehr geübt in Abzählen zusammenfallender Lösungen.

Dass Herr Chasles meine Dreiecksformeln für die Comptes Rendus nicht gebrauchen kann, wie ich aus Ihrem Briefe vermuthen muss (★), thut mir sehr leid, da ich der Meinung war, dass neue Resultate der Charakteristikentheorie in dem Geburtslande dieses Zweiges nicht verschmäht würden. [*in a footnote:*] (★) Eine Antwort habe ich von H.Chasles überhaupt nicht erhalten.] Freilich lassen sich Beispiele erfinden, bei denen man meine Dreieck-Formeln ohne Weiteres nicht anwenden kann. Daran sind aber die Formeln nicht

schuld, sondern das Missverständniss ihres Inhalts. Die detaillirte Ausführung der Modificationen, (★) die in vereinzeltten Fällen vorzunehmen sind, gehört wohl in die ausführlich Abhandlung. [(in a footnote:) (★) Zum Beispiel werden schon die Formeln für Φ illusorisch bei dem zweistufigen Systeme aller Dreiecke, welche aus zwei Tangenten einer Curve und der Verbindung der Berührungspunkte gebildet werden, weil es unendlich viele Φ sind. Diese sind aber nur als Dreiecke ϑ aufzufassen, und bei einiger Aufmerksamkeit kommt man nicht zu Unsinn, sondern zu den Plückerschen Formeln.]

Ich kann kaum glauben, dass Sie meine Dreiecks-Formeln für würdig halten, in dem Bulletin zu erscheinen, sollte es aber doch der Fall sein, so bitte ich um gefällige Aufnahme.

Meine ausführliche Abhandlung werde ich nächstens an die Annalen schicken können. Seitdem ich die Ehre hatte, durch Ihre gütige Vermittelung in die Société gewählt zu werden, betrachte ich mich gewissermassen als Ihren Protégé. Um so mehr thut es mir leid, dass ich mit meinen Arbeiten (deren mehrfache Mängel ich nicht verkenne) gar keinen Anklang mehr bei Ihnen finde. Dieses sehr bedauernd, und im Gefühle besonderer Hochachtung für Sie, Zeiche ich, einer geneigten Antwort entgegensehend,

Ihr treuer
Schubert

[(in a footnote:) (★) Aus pag 344 unten geht wohl hervor, dass ich Ihre Verbesserung des $\alpha\mu + \beta\nu$ vollständig anerkenne, aber keine Zeit mehr hatte, dieses für das Buch zu verwerthen, das ja nicht aus plötzlichen wissenschaftlichen Mittheilungen besteht und in das wohl eine Formel hineinpassen kann, obwohl sie bei strenger Untersuchung noch die Ergänzung $-\Gamma$ bekommt.]

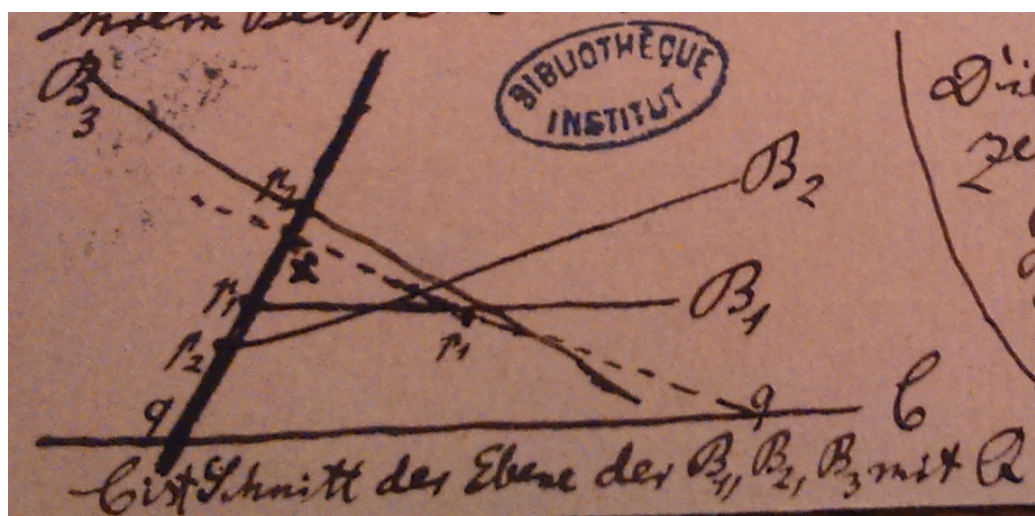
B.12 November 25th 1879 (postcard)

25/11 79

Geehrter Herr H.,

Ich habe in meiner Réponse den in der Ebene der drei Geraden B_1, B_2, B_3 gelegenen festen Punkt, durch welchen der Strahl g gehen soll, r genannt. Ich sehe eben, dass Sie ihn so x'' bezeichnet haben, also, wie mir scheint, mit x . Ich hiebt es für ein unterstrichenes v . Bitte, corrigiren Sie demgemäss. Vielleicht zersfört, wenn es noch nicht das Princip von der Erh. der Anzahl gethan hat, die folgende Figur, Ihre letzten Zweifel, dass die in Ihren Beispiele bestimmte Zahl nicht 1, sondern 2 ist.

Die eine Gerade ist dich gezeichnet, die von Ihnen übersehene gestrichelt. Das aus B_1, B_2, B_3 gebildete Dreieck ist im Begriff, in eins auszuarten, wo sich B_1, B_2, B_3 in einem Punkte schneiden.



C ist Schnitt der Ebene der B_1, B_2, B_3 mit Q

Mit besten Gruss

Ihr Scht.

B.13 November 31st 1879

Hamburg, 31/11 79

Sehr geehrter Herr Halphen,

So leid es mir thut, Sie wieder zu belästigen, mein mathematisches Gewissen zwingt mich dazu ; und ich ersuche Sie von neuen, die beiliegende Note über das Princip von der Erhaltung der Anzahl der verehrten Société vorzulegen, mit meiner Bitte, diese Note mit drucken zu lassen hinter Ihre "Nouvelles observations". Bei der Wichtigkeit welche die Sache durch unseren Streit für mich gewonnen hat, möchte ich Sie zu gleich bitten, für mich etwa 30 Exemplare alles 4 Noten als Separat-Abzüge (tirages à part) zu bestellen. Ferner bitte ich Sie, Germanismen des Stils und sprachliche Incorrectheiten oder ungenau gemachte Buchstaben selbst verbessern zu wollen. Z.B. schreibe ich für den zweiten Buchstaben des lateinischen Alphabets b, Sie aber b (beim Hyperboloid), oder ist dies ein anderer Buchstabe ? Dann bitte, gefälligst die ingemüss mein Manuscript zu ändern. Ich hege keinen Zweifel, dass die Société den Zweck bewilligen wird, der Sie ihn wünschen.

Gleichzeitig lege ich einen an die Société gerichteten Brief bei, in welchem ich dem Missverständniss stenne, wozu, wie ich gemerkt habe, der Umstand Veranlassung gegeben hat, dass ich Ihre Untersuchungen im Texte meines Buches nicht benutzt habe, und dass ich bloss historisch die Geschichte des $\alpha\mu + \beta\nu - \Gamma$ entwickelt habe, ohne auf die Sache selbst einzugehen, und dass ich dann dich mein Bedauern desüber ausgesprochen habe,

~~Ihre Untersuchungen~~ nicht im Hande gewesen zu sein, Ihre Untersuchungen verwerthen. Schon seit Ostern [wo ich] bin ich durch Sie überzeugt. Es liegt mir daran, dass Jeder weiss, wie hoch ich Ihre Arbeiten schätze. Vielleicht könnte sogar eine ?? Inhaltsangabe des Briefs am Bulletin erwähnt werden. Ich bitte Sie also, auch diesen Brief vorzulegen, dessen französische Abfassung mir nicht gelingen wollte.

Die Correcturbogen der beiden Noten werden mir wohl von Leiden der Redaction zum geschicht werden ?

Sollten sie eine nochmalige Antwort auf meine beilegende Note abfassen, so erwähnen Sie, bitte, im Bulletin, dass ich die Streitfrage in einer besonderen Abhandlung in den Math. Ann. weiter behandeln würde. Indem ich nochmal meine Note durchlese, fasse ich die Hoffnung, dass dieselbe Sie von der Richtigkeit meine Erörterungen vollständig überzeugt.

Was meinen Sie dazu, wenn wir die 4 Noten auch in die Math. Ann. brächten ? Ich würde es aus mathematischen Interessen sehr gern sehen. In der Erwartung baldiger Antwort,

Mit vorzüglicher Hochachtung,
Ihr ergebenster,
Schubert

[The following note was attached to the letter]

Sur le principe concernant la constance des nombres géométriques

Par M. Schubert

(Voir:

1. ~~Note de M.H~~ Les observations sur la théorie des caractéristiques par M. Halphen,
2. ~~Ré~~ La réponse à ces observations par M. Schubert,
3. Les nouvelles observations sur la théorie des caractéristiques par M. Halphen)

Je répète que la droite xs fait partie du système Σ' et satisfait à la condition Σ , et qu'elle donne lieu ainsi à la seconde solution du problème inventé par mon savant contradicteur. En disant le contraire, on dit que l'une des deux droites passant par un point donné x et rencontrant deux couples de droites quelconques harmoniquement, cesse d'exister au moment que trois des quatre droites vont ~~se couper~~ concourir en un même point, et en disant cela on nie le principe bien connu (Princip von der Erhaltung der Anzahl) auquel j'ai donné dans mon livre (pag. 12) quatre formes. La seconde forme ~~y~~ est peut être énoncée ainsi :

“Un nombre géométrique devient infini ou il conserve sa valeur, si l’on change la situation des figures données en sorte qu’elles deviennent situées plus spécialement.”

La troisième forme du principe s’énonce en ces termes:

“Un nombre géométrique devient infini ou il conserve sa valeur, si l’on place au lieu des figures données générales, des figures particulières ou dégénérées satisfaisant à la définition des figures générales.”

Pour prouver la fausseté de ce principe fécond dû à l’algèbre, M. Halphen a inventé un exemple dans ses “Nouvelles observations”. Cet exemple, loin de mettre le principe en défaut, en est une vérification très intéressante. Voici les raisons.

M. Halphen envisage trois directrices B_1, B_2, B_3 d’une certaine hyperboloïde H et définit un système Σ''' de figures Γ par les ∞^1 génératrices g rencontrant les directrices B_1, B_2, B_3 dans des points p_1, p_2, p_3 . Σ est la condition définie dans les notes précédentes. M. Halphen trouve la valeur 1 pour le nombre $(\Sigma\Sigma''')$, soit par ma formule, soit aussi bien que directement. Après cela, il envisage au lieu de l’hyperboloïde H un hyperboloïde dégénéré en deux plans en sorte que les directrices B_1, B_2, B_3 deviennent trois droites situées dans le plan xs et concourent au point s . Or, il s’agit de déterminer quelles sont maintenant les génératrices g . M. Halphen dit : le faisceau des droites passant au point x et situées dans le plan xs . Moi, je sais que les génératrices g se composent des droites de deux faisceaux. L’un est celui dont M. Halphen fait mention, l’autre est le faisceau ayant le sommet s et le plan xs (\star). Ainsi ~~chaque droite~~ toutes les droites joignant le point s avec les ∞^1 points où le plan xs rencontrent le plan Q , donnent lieu à des figures Γ' qui, faisant partie du système Σ''' , satisfont à la condition Σ . C’est pourquoi le nombre $(\Sigma\Sigma''')$ ayant la valeur 1 au cas de l’hyperboloïde général, devient infini au cas de l’hyperboloïde dégénéré en deux plans, résultat vérifiant la troisième forme ci-dessus citée du principe concernant la constance des nombres géométriques. D’autre part, on voit par cela que M. Halphen se trompe en disant que son système Σ le nombre $(\Sigma\Sigma''')$ défini dans la première note soit est un cas particulier du nombre $(\Sigma\Sigma''')$.

En résumant mes exposés, je dis que les exemples de M. Halphen vérifient mes formules caractéristiques ($\star\star$) aussi bien que le principe ci-dessus cité.

Hambourg, 31. Novembre 1879.

H. Schubert

[(In footnotes :)]

(\star) Si un hyperboloïde est dégénéré en deux plans e et e' , la droite d’intersection de ces deux plans contient deux points p et p' tels que les directrices de l’hyperboloïde dégénéré sont les droites de deux faisceaux, l’un composé du plan e et du sommet p , l’autre composé du plan e' et du sommet p' , tandis que alors les génératrices de l’hyperboloïde sont les

droites des deux autres faisceaux, l'un composé du plan p et du sommet e' , l'autre composé du plan p' et du sommet e . (Voir mon livre, pag. 70)

(★★) Je répète enfin que chacun qui veut employer mes formules caractéristiques sans en connaître les démonstrations et sans avoir lu les chapitres précédents III et V de mon livre, est obligé d'avoir égard aux remarques qui se trouvent à la fin de ma note précédente. J'espère de revenir à ces remarques ~~à l'occasion~~ dans un journal allemand quand je publie mes recherches actuelles.]

B.14 December 1879

Hamburg, /12 79

Sehr geehrter Herr,

Bessen Dank für ihre neuen Erörterungen im Briefform. Ich sehe jetzt deutlich, dass Ihre Beispiele daran schuld sind, warum ich nicht verstehen konnte, was Sie eigentlich meinten. Lesen Sie, bitte, vor allem noch einmal die zweite Hälfte meiner ersten Antwort. Hätten Sie das unglückliche Beispiel $\Sigma\Sigma' = 1$ oder 2 nicht gewählt, sondern bloss gesagt, was Sie jetzt sagen, nämlich : “Votre formule n'est valable que quand le système Σ' ne contient pas la figure dégénérée où les trois points coïncident.” Dann hätte ich sofort geantwortet : “Selbstverständlich, lesen Sie den Zusammenhang, dann werden Sie erkennen dass immer eine die Coincidenz von i Punkten als eine $(i-1)$ -fache Bedingung behandelt ist, und dass also der Formel sogar die Voraussetzung zu Grunde liegt, dass beide Systeme diese Eigenschaft haben. Andere Fälle, als solche stecken im ganzen Buche nicht.” Darauf hätten Sie höchstens noch den Ausdruck “Characteristikenformel” gestadelt, oder gewünscht, dass ich im Buche noch ~~einmal~~ besonders hervorgehoben hätte, dass auch Systeme möglich sind, bei denen die Formel illusorisch wird. Darin hätte ich Ihnen Recht gegeben, und wir wären einig gewesen.

In meiner ersten Antwort habe ich ja überhaupt nur Ihrem Beispiel Opposition gemacht. Lesen sie doch nach.

Ich habe nun 5 verschiedene Themata im Beiliegenden behandelt. Die Abfassung ist etwas eilig gewesen, aber ich glaube, Sie werden alles deutlich verstehen. Hiernach sind Sie wir in den wesentlichsten Punkten vollkommen einig. Trotzdem hatte ich jeden Buchstaben, den ich in meinen beiden Noten geschrieben habe, vollkome aufrecht. Ich bin mir keines Fehlers bewusst. Der Unterschied zwischen uns beiden besteht nur noch darin, dass ich gewisse Ihrer Definitionen anders verstanden werden. Die Differenz ist also mehr eine sprachliche geworden.

Sie fingen Ihren vorigen Brief mit den Worten an “Il me paraît effect. utile que nos observations sur vos formules soient publiées”, und überzeugten mich dadurch von den wissenschaftlichen Nothwendigkeit des Drückes unserer 4 Noten, um so mehr, als Sie mir Ihre erste Note als eine für den Druck bestimmte zuschickten. Warum sollen Sie also nicht gedruckt werden, ich sehe kein novum. Denn über die Nicht-Anwendbarkeit meiner Formeln in gewissen Fällen sind wir nie uneinig gewesen. Nun Sie sagten, die Formeln sind ungenau, weil sie in jenen Fällen nicht passen, ich sagte, die Formeln sind dann sogar sinnlos, und die Nicht Anwendbarkeit ist im Zusammenhange meines Buches selbstverständlich. Ich schlage Ihnen also vor, dass die 4 Noten, zur Belehrung anderer Mathematiker, gedruckt werden im Bulletin, und dass wir zusammen eine fünfte Note abfassen, welche Sie vielleicht auf Grund der beiliegenden Blätter abfassen, und mir dann vorschlagen. Ich glaube, dass dies möglich ist. Was meinen Sie dazu ? Es ist der würdigste Abschluss, da sonst noch Note auf Note folgen könnte, da indem durch neue Beispiele immer neue Fragen hineingezogen würden.

Ich halte die Sache im wesentlichen für erledigt. Das Hyperboloid und Ihr neues Beispiel können wir ja noch gelegentlich unter uns weiter besprechen.

Ich möchte Sie sehr ersuchen, die beiliegenden Blätter auch Herrn Zeuthen zu schicken, der sich ja für Ihre Anschauungen auch interessiert hat. Leider habe ich in diesen Tagen, antlicher Geschäfte wegen, gar keine Zeit, die Blätter zu copiren. Auch müsste ich Sie ersuchen, im Fall Sie auf einzelne Thesen mir noch antworten wollten, mir den ungefähren Inhalt und nicht bloss die Nummer anzudeuten, da ich auch keine Copie für mich behalten habe.

Was meinen Sie, dass ich in den Annalen veröffentliche ? Alle 4 Noten ? Oder bloss mein Zugeständniss über die Beschränkte Gültigkeit von $\alpha\mu + \beta\nu$, und mein Aufmerksam machen, dass für §42 und §44 Systeme denkbar sind, wo die Formeln sinnlos werden, also noch nicht alle Fälle erledigt sind ?

Warum habe Sie Bedenken getragen, die 4 Noten selbst gestern Abend vorzulegen ?

Sie schreiben mir, Sie hätten immer meine Antwort vorhergewusst. Warum haben Sie dann nicht die erwarteten Einwände selbst vorher entkräftet ? Beachten Sie wohl, dass meine Einwände sich immer nur gegen Ihre Beispiele, aber nicht gegen Ihre allgemeinen Behauptungen gerichtet haben.

Wenn Sie beabsichtigen, genaue Studien zu machen über die Grundbegriffe der abzähl. Geom, über Unabhängigkeit, Ausartung etc, so begrüsse ich diese Absicht mit grosser grosser Freude und hohen Erwartungen. Darf ich nur ein paar Bemerkungen hinzustreuen ? Eine Gleichung in Punktekoordinaten oder Liniencoordinaten zieht häufig in die Definition eines Gebildes oder einer Bedingung Dinge oder Anschauungen hinein, welche der rein geometrischen Auffassung der ausgesprochenen ~~Auffassung~~ Definition zunächst

fremd sind. Was verstehen Sie z.B. in der wahren Geometrie unter arbitraires ?

Noch eine Kleinigkeit. Ich halte der Ausdruck *solution étrangère* für eine *contradictio in adjecto*. Können Sie nicht einen andern Ausdruck dafür erfinden ?

Ich erwarte mit Spannung Ihren nächsten Brief, und hoffe sehr, dass Sie schliesslich[illegible] der noch nicht erledigten Fällen, in dem Abschnitt VI meines Buches die Basis zu einen intere?? neuen Theile der abzähl. Geom. erkennen werden. Sollte diesen Abschnitt auch bloss dazu beitragen haben, dass Sie Sich mit der Charac. Th. der aus einzelnen Hauptelementen bestehenden Gebilde beschäftigen, und Neues bringen, so würde der VIte Abschnitt schon immer einen grossen Erfolg hinter sich haben.

Sollte ich Ihren nächsten Brief nicht umgehen beantworten, so schreiben Sie dass, bitte, dem Zeitmangel zu, unter dem ich bis Weihnachten beiden werden. Auch um Weihnachten werde ich nicht viel arbeiten können, da uns Verwandte von ausserhalb besuchen werden. Dazu kommen die eben beginnende Correcturbögen meiner Abhandlung im Crelle über 1-2-deutig auf einander bezogene Punktreihen. Im di Math. Ann. habe ich einen sehr kurzen Beweis des Satzes von der Erhaltung des Geschlechts bei 1-1-deutig bezogenen Punktreihen, worüber ich soeben den Corrbg. bekam.

Was meine Dreiecke anbetrifft, so sind die gegebenen Formeln auch sinnlos wenn in einstufigen Systemen zweistufig genannte? Ausartungen vorkommen. Das versteht sich von selbst. Dass Wenn Sie deshalb die Formeln für interesslos halten sollen, so bedaure ich, die Zeit auf die Abfassung des für Frankreich (also hauptsächlich für Chasles, Sie und Fouret) bestimmten Résumés verwandt zu haben, und möchte Sie ersuchen, sie Herrn Fouret sehen zu lassen, und dann zu vernichten. Von Herrn Chasles habe ich überhaupt keine Antwort bekommen.

Einer gefälligen Antwort aber von Ihnen entgegensehend zeiche ich als Ihr

ergebenster, H.Schubert

[The five notes were attached to this letter]

Erstes Thema

Über die Anwendbarkeit der Charakteristikenformeln des §42 und §44 meines Buches

1) Wir haben beides stets darin übereingestimmt, dass die Formeln des §42 und §44 nur anwendbar sind, wenn jedes der beiden Systeme die Eigenschaft hat, dass in ihm die Forderung der Coincidenz von i Punkten eine $(i - 1)$ -fache Bedingung ist. (cf erste Réponse)

2) Ich halte es für selbstverständlich, dass die Formeln die in 1) erwähnte Eigenschaft bei den Systemen voraussetzen, und zwar deshalb für selbstverständlich, weil im ganzen Abschnitt V und VI die Coincidenz von i Punkten einer Geraden stets als eine $(i - 1)$ -fache Bedingung behandelt ist, und weil das Symbol der Bedingung der Coincidenz von i Punkten sogar in die Charakteristikenformel (pag 302) selbst als ein $(i - 1)$ -faches Symbol eingeführt ist.

3) Ich habe den wesentlichen Inhalt von 1) und 2) schon in meiner erster “Réponse aux observations.” ausgesprochen, und zwar in dem zweiten Theile, welches anfängt “Pour prévenir des emplois mal entendus.”

4) Ein Leser meines Abschnitts V und VI, der sich an die Bedingungssymbole gewöhnt hat, würde schwerlich die Formeln auf Systeme anwenden wollen, welche die erwähnte Eigenschaft nicht besitzen.

5) Mit Rücksicht darauf, dass man sich daran gewöhnt hat unter Charakteristikenformeln Formeln zu verstehen, welche für alle Systeme und alle Bedingungen Sinn haben, so wäre es zwar nicht nothwendig, aber zweckmässig gewesen, wenn ich namentlich bei der Formeln 3) (pag 308), welche die unbestimmten Coefficiente $\beta, \alpha_1, \alpha_2, \dots$ enthält, die durch den ganzen Zusammenhang vorausgesetzte Eigenschaft noch besonders hervorgehoben hätte, oder wenigstens angeführt hätte, dass Systeme denkbar sind, bei denen die Coincidenz von i Punkten eine Bedingung wird, die von niederer Dimension ist, als der $(i - 1)$ -ten. Unnöthig wäre das Hervorheben der Eigenschaft bei der Formel 4) auf pag 309 gewesen, weil da die Deutung der Symbole überhaupt unmöglich wird, sobald das System Σ die erwähnte Eigenschaft nicht hat.

6) Die Formeln werden dadurch, dass sie in gewissen Fällen keinen Sinn haben, noch durchaus nicht werthlos. Man kann nicht sagen, dass sie ungenau sind, man kann höchstens sagen, dass es keine Charakteristikenformel im strengsten Sinne des Wortes sind.

7) Systeme, welche die erwähnte Eigenschaft nicht haben, kommen in meinem Buche gar nicht vor.

8) Ohne dass dies es in der Ableitung beabsichtigt ist stimmt die Formel 3) auch, wenn ~~nun~~ die Σ' die erwähnte Eigenschaft hat, Σ aber nicht, bloss man muss dann die Coefficienten nicht nach Formel 4, die ja sinnlos wird, sondern auf anderen Wege bestimmen.

9) Eine ~~Untersuchung~~ Lösung des Charakteristikensproblem in den Fälle in denen meine Formeln sinnlos werden, wäre an sich eine dankenswerther Beitrag zur Charakteristiken-theorie.

Zweites Thema

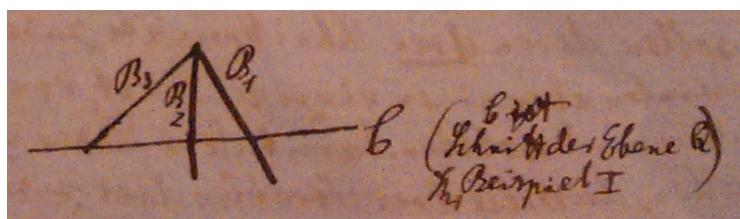
Über die Ungenauigkeit der Formel $\alpha\mu + \beta\nu$ (§38)

- 1) Die in §38 gegebene Ableitung der Formel $\alpha\mu + \beta\nu$ ist nur richtig, wenn das System die dritte Halphensche Ausartung (\star) nicht enthält. [*in a footnote :*] (\star) Die Natur dieser Ausartug habe ich erst während des Drückes des Buchs klar erkannt aus, und zwar aus der Abh. Halphen's in d. Math. Ann. Bd. 14.]
- 2) Dass dieses nicht besonders gesagt ist, is nicht bloss eine Unterlassungssünde, sondern geradezu ein Fehler, weil eine solche Voraussetzung über das System aus dem Zussammenhange nicht hervorgeht. Man muss also die Voraussetzung besonders hinzufügen.
- 3) Ist der Beweis bei Hinzufügung dieser Voraussetzung brauchbar ?
- 4) Ist mein Buch darum unbrauchbar, weil dasselbe in §38 ein tadelnswerthes Übersehen enthält ? (cf Fehler in Salmon, in Clebsch-Lindemann, an welche Fehler sich gewöhnlich geistreiche Untersuchungen angeschlossen haben)

Drittes Thema

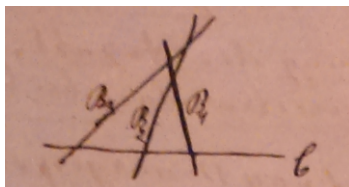
Über das Princip von der Erhaltung der Anzahl,
und seine Anwendung bei $(\Sigma, \Sigma') = \{1, 2\}$?

- 1) Das von mir auf pag 12 ausgesprochene Princip ist unbestreitbar, indem es aus eines fundamentalen, algebraischen Quelle stämmt. Es ist auch für geometrische Abzählungen äussert fruchtbar. Hierin stimmen Sie jetzt mit mir überein.
- 2) Bei der Benutzung des Princip kann man zu Irrthümern gelangen, wenn man nicht genau darauf achtet, ob die Definition des speciellen Gebildes auch präcis in der Definition des allgemeineren Gebildes enthalten ist.



C ist Schnitt der Ebebe Q Beispiel I

3) Man kann nicht ohne Weiteres aus der im allgemeineren Falle enthaltenen Zahl ableiten, dass diese Zahl in dem speciellen Falle auch noch existirt. Hängt zum Beispiel eine Zahl von einer so aussehenden Figur ab, so kann ich diese gesuchte Zahl nicht ohne Weiteres dadurch bestimmen, dass ich sage, sie ist gleich der Zahl, welche in derselben Weise von der folgenden Figur abhängt :



Wohl aber kann ich dies, wenn ich bei der ersten Figur definire, sie soll ~~aus 4 Strahlen bestehen~~ ein Vierseit sein, bei welchen drei Seiten sich in einem und demselben Punkte schneiden, und bei der zweiten Figur definire, sie soll ein Vierseit sein.

4) Ich kann aus Ihrer Definition des Systems Σ' auch jetzt noch nichts mehr oder nichts weniger herauslesen, als das B_1, B_2, B_3 und der Schnitt C mit der Ebene Q ein Vierseit sein sollen, und kann dabei nur immer sagen, aber warum sollen sich denn B_1, B_2, B_3 durchaus in einem Punkte schneiden ?

5) Sie ~~fragen~~ denken Sie also, wenn meine zweite Lösung nicht Lösung sein soll, dies gewissermassen schon in die Definition hineingefügt. Das konnte ich aber aus den Wortlaut nicht entnehmen.

6) Ich entnahm dem Wortlaut nur, dass B_1, B_2, B_3 drei Strahlen sein sollen, deren drei Schnittpunkte zusammenfallen.

7) Sie meinten aber einen einzigen Punkt, von dem drei Strahlen ausgehen, und wollten damit dem Leser verbieten, sich vorzustellen, dass die drei Strahlen drei zusammenfallende Schnittpunkte haben.

8) Bei Ihrer Auffassung bilden also allen Strahlen, welche B_1, B_2, B_3 in drei Punkten schneiden, nicht das ganze Strahlenfeld ihrer Ebene, sondern dieses vermindert um den Strahlbüschel, dessen Scheitel der gemeinsame Punkt von B_1, B_2, B_3 ist.

9) Habe ich Sie jetzt richtig verstanden ?

10) Wenn ich Sie verstanden habe, so können wir diesen Streitpunkt wohl nun fallen lassen, weil Sie meine Auffassung, und ich die Ihre verstanden habe, und der Streit jetzt nicht mehr mathematik sein würde, sondern sprachlich.

11) Wir sehen daraus beide, dass man bei der Definition geometrischer Gebilde sich leicht missverstehen kann. Das aus einem einzigen Punkte und aus drei von ihm ausgehenden Strahlen bestehende Gebilde ist ja für Sie etwas ganz anderes als das Gebilde welches aus drei Strahlen mit zusammenfallenden Schnittpunkten besteht.

Viertes Thema

Über die Regelschaar der génératrices welche drei Directrices B_1, B_2, B_3 eines in ein Ebenenpaar ausgearteten Hyperboloids schneidenden

1) Da Sie das Ebenenpaar als Hyperboloid ansehen, auch nicht von Schneiden der directrices, sondern von wirklichen génératrices und directrices dabei sprechen, so dürfen Sie, nach meiner Meinung, das im Thema III, These 3 ausgesprochenen nicht als Motiv dafür anführen, dass bei der Ausartung die Regelschaar der génératrices aufhört, zweiten Grades zu sein, und anfängt, erstes Grades zu sein.

2) Der Wortlaut Ihrer Definitionen führt sicher dazu, zwei Büschel von directrices und zwei Büschel von génératrices anzunehmen.

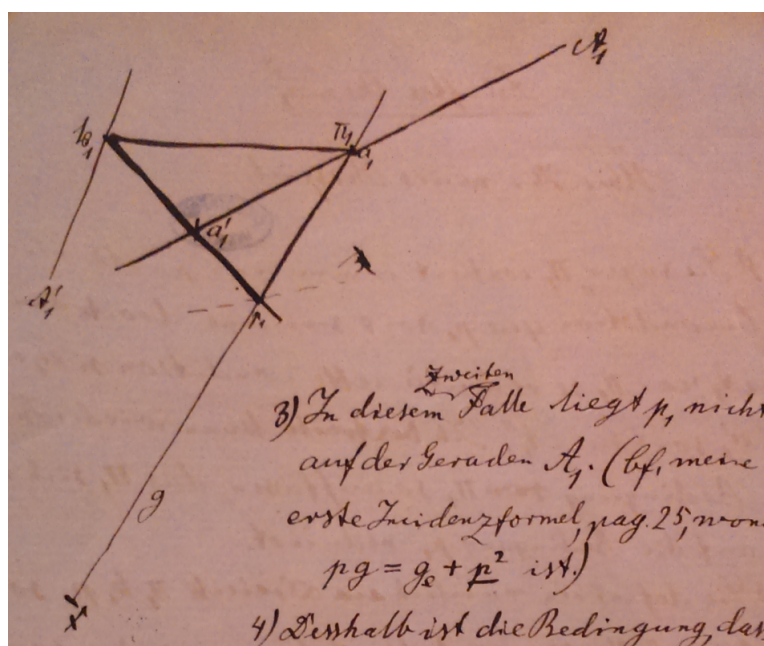
3) Es handelt sich nicht um ein punktgeometrisches, sondern um ein liniengeometrisches Problem dabei, und bei dieser Auffassung ergibt sich unendlich viel. Machen Sie Sich, bitte, die Sache doch durch homographische Abbildung klar.

Fünftes Thema (★)

Über Ihr neues Beispiel

1) Sie sagen " Π_1 contient comme cas particulier la condition que p_1 soit sur une droite donnée A_1 , car Π_1 se réduit à cette condition si a_1 et a'_1 sont sur A_1 ". Ich kann wieder Ihre Bedingung Π_1 so auffassen, dass Π_1 sich nicht auf die Bedingung p_1 reducirt.

2) Sie definiren nämlich ein Dreieck $\Pi_1 k_1 p_1$ so dass k_1 auf A'_1 , Π_1 auf A_1 liegt, $k_1 p_1$ durch a'_1 geht, $k_1 \Pi_1$ durch a_1 geht, $\Pi_1 p_1$ durch χ geht. Dann liegen auf einer beliebigen Geraden zwei Punkte p_1 . Richtig ! Nun legen Sie a_1 und a'_1 auf A_1 . Nun können, kann ich sagen, zwei Fälle von Dreiecken eintreten, erstens Ihr Fall, dass p_1 auf A_1 liegt, zweitens der Fall, dass Π_1 in den Punkt a_1 fällt, wie die folgende Figur zeigt :



3) In diesem zweiten Falle liegt p_1 nicht auf der Geraden A_1 . (Cf meine erste Incidenzformel, pag.25, wonach $pg = g_e + \underline{p}^2$ ist.)

4) Deshalb ist die Bedingung, dass p_1 auf der Geraden A_1 liegt, kein specieller Fall von Π_1 .

5) Ich gebe zu, dass Sie jetzt erwiedern können, dass Sie die Definition mit den Worten “On joint Π_1 et a_1 ” angefangen haben, und damit durch diese Definition selbst den Fall ausgeschlossen haben, dass a_1 mit einer Ecke zusammenfallen darf.

6) Wir kommen damit auf das IIIte Thema zurück.

[(in a footnote :) (*) worüber wohl kein besonderer Streit geführt zu werden braucht, wegen der Übereinstimmung, die wir wohl über das Thema III im wesentlichen haben.]

B.15 January 7th 1880

Hamburg, 7/1 80

Sehr geehrter Herr Halphen,

Erst jetzt, nachdem des Weinachts “und Neujahrs” Jubel vorüber ist, gewinne ich die Zeit, Ihren werthen Brief vom 24. December zu beantworten.

Sie schreiben auf der ersten Seite “D’autre part vous affirmez énergiquement que je me trompe sur tous les points, d’autre part vous seriez d’accord avec moi.”

Sie schreiben auf der letztzen Seite “Les observations jointes à votre dernière lettre sont pleines d’intérêt.”

Wenn Sie diese meine Bemerkungen im vorigen Briefe, die ich so ausführlich wie möglich und mit möglichster Trennung der verschieden Punkte angefertigt habe, um Missverständnisse zu beseitigen, durchgesehen haben, so müssten Sie doch deutlich eingesehen haben, inwiefern Sie ~~naeh~~ mit mir übereinstimmen, und inwiefern Sie andererseits, Sie nach meiner Ansicht, nicht mit mir übereinstimmen. Ich muss daher noch einmal das Wichtigste hervor heben.

1) Wir stimmen darin über, dass meine Formeln die Fälle ; wo ein System von ∞^i Geraden ∞^{i-1} Ausartungen mit 3 unendlich nahen Punkten enthält, nicht erledigen. Ich sehe dabei absichtlich ganz davon Ihren Beispiel ab. Es bedarf keines Beispiels, weil die Formel 4) die ausgeschlossen Fälle erkennen lässt.

2) Ich sage, dies die Einschränkung geht aus dem Zusammenhange meines Buches hervor. Sie geben dies zu durch den Text, den Sie mir in Ihren letzten Briefe für meine Antwort vorschlagen. Ich gebe Ihnen zu, dass es nützlich gewesen wäre, dies bei Formel 3) expressément zu sagen (Bei Formel 4) ist es ja unnöthig) Ich will auch gern zugeben, dass der Ausdruck “Charakteristikenformeln” zuviel versprechend ist ; Ich hätte vielleicht meinen alten Ausdruck “Produktensätze” (Math. Ann. Bd. 10) beibehalten resp. auch hier gebrauchen sollen.

3) Ich füge hinzu, dass nach meinem Buche keins der beiden Systeme Σ und Σ' die in 1) angedeuteten Ausartungen ∞^{i-1} -fach enthalten darf. Dass vielleicht noch eine Charakteristikenformel aufstellbar ist, wenn nur ein der beiden Systeme eine solche Ausartung enthält, geht aus meinem Buche nicht hervor. Das ist ein neuer Satz von Ihnen. Ich darf ihn also in meiner Antwort nicht aussprechen, um die Leser nicht zu verwirren.

Mit Rücksicht auf die eben geschriebenen Bemerkungen 1), 2), 3), mit Rücksicht auf meine erster mir zurückgeschickte Antwort, und mit Rücksicht auf Ihren Vorschlag einer Antwort, habe ich die beigelegte Antwort abgefasst, welche Sie gütigst der Société vorlegen und nach etwaigen Stil-Correctionen zum Druck übergeben mögen. Diese Antwort unterscheidet sich von meiner ersten durch zweierlei :

a) Ich lasse lieber ganz bei Seite, was sich für $(\Sigma\Sigma')$ ergibt. Das ist ja auch gleichgültig, weil die Anwendung, den Inhalte des Buches nach, nicht erlaubt ist, also zufällig ja auch etwas Richtiges hätte ergeben können.

b) Ich lasse ferner bei Seite, dass die Anwendung bei einer gewissen Auffassung (wenn nämlich meine zweite Lösung mitgezählt werden darf) ein doch noch ein richtiges Resultat giebt. Freilich ist dieser Antwort gegenüber meiner ersten etwas verbessert. Aber den Kern der Sache habe ich schon damals hervorgehoben, dass nämlich Formel 4) mit ihrem Symbolen, so wie schon §34 erkennen lassen, dass Systeme mit den erwähnten Ausartungen auszuschliessen sind.

Ich würde mich sehr freuen, wenn Sie mir zugestehen könnten, dass die beiliegende Antwort den Nagel auf den Kopf trifft und andererseits Ihnen genügt Sie schreiben ja :

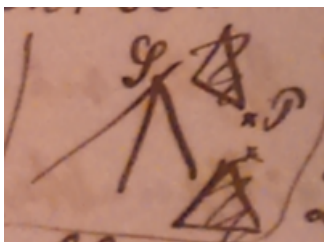
“Si vous dites que vos formules ne s’appliquent pas à tous les cas, cela me suffira parfaitement.”

Nun, das ist geschehen, ich habe nur, was Sie mir doch nicht übel nehmen können, hinzugefügt, dass woher dies “ne s’appliquent pas” kommt, und warum das Wort inexactitude die Sache nicht trifft. Ich hoffe also, diese Sache ist jetzt zu Ihrer Zufriedenheit erledigt.

Nun kommt etwas ganz anderer, was, wie ich jetzt mehr als früher hervorhebe, mit dem “ne s’appliquent pas” nichts zu thun haben soll.

4) Es fragt sich, ist $(\Sigma, \Sigma') = 2$ eine falsche Gleichung ? Ich meine, sie ist richtig für Jeden, welcher zugiebt in drei sich in einem Punkte schneidenden Geraden nichts anderes sieht, als drei Gerade deren 3 Schnittpunkte identisch sind (Volle Coincidenz der Schnittpunkte), welcher also darin nur einen wirklich speciellen Fall eines Dreiseits sieht.

Beispiel : Man habe ein einstufiges System von Dreiecke, in welchem sich eins befindet, bei dem die 3 Seiten, getrennt von einanders, sich in einem Punkte S schneiden, ausserdem einen Punkt P . Man zeichne zu jedem Dreieck des Systems denjenigen Strahl durch P , welcher die 3 Seiten a, b, c so schneidet, dass der Schnittpunkt auf a harmonisch conjugirt wird zu P in Bezug auf die Schnittpunkte mit b und mit c . Für jedes Dreieck giebt es einen Strahl, der leicht zu construiren ist. Für die Ausartung giebt liefert diese Construction die Verbindungslinie PS . Also ist PS eine Linie, welche die 3 concurrürenden Strahlen in der angegebenen Weise schneidet.



Analog bei (Σ, Σ') . Ich meine also bloss, $(\Sigma, \Sigma') = 2$ ist, solange man aus Ihren 3 Geraden B_1, B_2, B_3 bestehende Gebilde als Dreieck ansieht. Sie meinen nun aber, wie ich Ihnen schon in den Bemerkungen meines letzter Briefes geschrieben habe, durch Ihre Definitionen von Γ, Σ, Σ' meine Auffassung ausgeschlossen zu haben. Ich habe dies darin nicht lesen können. Sie wollen durch Ihre Definition von B_1, B_2, B_3 eben ausdrücken, dass der Ort für die vierte harmonischen Punkte zufallen Strahlen, welche B_1, B_2, B_3 schneiden (bestimmt in Bezug auf die 3 Schnittpunkte) nur die einzige Polar polaire D ist. Ich habe die ganze Ebene als Ort aufgefasst, indem ich für jedem nicht auf D liegenden Punkt, die Verbindung mit S ziehe, und für der auf D liegenden Punkt unendlich oft dem Orte angehörig). Es handelt sich also bloss noch darum, ob es ein Fehler ist, wenn ich meine Auffassung aus Ihnen Definition der B_1, B_2, B_3 entnehme.



5) Unsere Debatte über die Anwendbarkeit des Princips von der Erhaltung der Anzahl hätten wir uns sparen können. Wir werden beide das Prinzip immer nur mit Verständniss anwenden, und auf seine algebraische Quelle bei allen zweifelhaften Fälle zurückgreifen. Meine Worte “on nie le principe..” waren damals vielleicht, wie Sie es nennen, quelque peu agressifs. Ich nehme sie zurück andererseits haben Sie durch ihre Worte “pour faire apprécier la valeur de ce raisonnement” meine raisonnement lückerlich gemacht. Sie hätten Sich doch aber wohl denken können, dass ich nicht ohne Weiteres aus jeder im allgemeinen richtigen Zahl dieselbe schliesse, dass dieselbe Zahl im speciellen richtig ist. Dass dies aber bei richtiger Deutung der Art der Specialisirung zu einem richtigen Resultate

führen kann, ist selbst verständlich. Es handelt sich eben bloss darum, ob die stattgefundenen[habte] Verallgemeinerung Veranlassung zu einer nachherigen Ausscheidung giebt oder nicht. Dass “concourant en un point” schien mir keine Veranlassung dazu zu geben, weil ich die Definition der B_1, B_2, B_3 nicht so auffasste, wie Sie es dadurch bezwecken wollten.

~~6) Ich bin immer noch der Meinung, dass~~ Jetzt kommt wieder etwas, was mit dem Vorigen nichts zu thun hat, das Ihr Hyperboloid Beispiel nicht nothwendig war, um mich über das Princip von der Erhaltung die Anzahl und seine missverstandenen Anwendung aufzuklären.

6) Ich bin noch immer der Meinung, dass (Σ, Σ') kein wirklich specieller Fall von (Σ, Σ''') ist, und zwar desswegen, weil beim Grenzübergang die sämtlichen Geraden x génératrices, welche B_1, B_2, B_3 als directrices haben, zwei Strahlbüschel bilden werden, deren einer (von Ihnen nur erwähnt) die Ebene xbs und den Scheitel x hat, deren zweiter die andere Ebene und den Scheitel s hat (bf die letzter Bemerkungen im letzten Briefe, Sie werden also doch zugeben, dass ich nicht mit Ihnen hierüber d'accord gewesen bin, und dass also die schon anfangs erwähnten Stelle Ihres Briefes ungenau ist.)

Wenn es Ihnen Recht ist, können wir über Vv 6) noch weiter gelegentlich disputiren, die übrigen Sachen können wir nun wohl als erledigt betrachten.

Ich ersuche Sie ferner die beigelegte zweite Tone über den §30 meines Buches der Société Math. vorzulegen, und abdrucken zu lassen. Ich ersuche Sie ferner, es meinem Mangel an Verständniss zuzuschreiben, dass ich nicht eher die Berechtigung Ihrer Beschränkung des $\alpha\mu + \beta\nu$ klar eingesehen habe. Ich lese mich übrigens allmählich mehr in Ihre Noten hinein. Ich muss aber eingestehen, dass immer noch Stellen darin sind, die mehr noch unklar sind. Ich hoffe aber, allmählich alles verstehen zu können.

Haben sie etwas dagegen, wenn ich Klein Ihre Observations, meine jetzt beigelegte Réponse und meine Note über nombre des coniques für die Annales schicke ?

Ich würde mich sehr freuen, wenn Sie mir bald, wenn auch nur per Postkarte, schreiben wollten, ab wann Sie der Société meine Réponse übergeben wollten, und ob Sie dieselbe für hinreichend halten.

Mit den besten Wünschen für das beginnende Jahr und der Versicherung besonderer Hochachtung,

Ihr ergebenster
Schubert

B.16 January 14th 1880 (postcard)

14/1 80

[(In the left margin, written vertically:) Morgen schicken ich Ihnen ein Exemplar meiner Abh. im Crelle.]

Besten Dank für Ihren werthen Brief, und im voraus dafür, dass Sie Freitag meine Noten vorlegen wollen. Sie würde Ihrer Güte die Krone aufsetzen, wenn Sie zugleich für mich 25 Separatabzüge Ihrer Observations und meiner Réponse bei der Redaction der Bulletin bestellen wollten.

Ich erkenne Ihre Gesichtspunkte bei $\alpha\mu + \beta\nu - \Gamma$ als berechtigt an, wenngleich ich glaube, dass sich die Sache noch übersichtlicher wird darstellen lassen. Sie werden aber auch zugeben müssen, dass die Darstellung der Anzahl der gemeinsamen ~~Anzahl~~ Gebilde von Systemen als algebraische Summe von Produkten von je zwei Anzahlen, die sich auf die einzelnen Systeme beziehen, an sich interessant genug ist, um um ihrer selbst willen studirt zu werden, selbst wenn, wie aus Ihren Untersuchungen ersichtlich ist, die gegebenen Systemen gewissen, natürlich anzugebenden Beschränkungen unterliegen. Nennen wir diese Analoga des Bezout'schen Satzes dann Productensätze, und bewahren wir den Ausdruck Characteristkensätsze für Ihren Sachen auf. Es thut mir leid, dass wir über das Hyperboloid nicht noch weiler discutiren können. Ihr specielles H , beidem die generatrices einen Strahlbüschel bilden, ist nach meinen Theilgebilde eines ausgeart. Hyp, aber erfüllt nicht die Definition des (Terminologie) Hyp.

Mit besten grüssen hochachtungsvoll

Ihr Scht.

B.17 January 25th 1880 (postcard)

25/1 80

Sehr geehrter Herr Halphen,

Indem ich Ihnen anbei eine kleine Note schicke, bemerke ich, dass ich sehr wohl weiss, dass darin die mehr als zweifachen Punkte mit zusammenfallenden Tangenten etc. noch nicht berücksichtigt sind. Ebenso wenig ist dies in Lindemann's Vorl. v. Clebsch bei den geom. Beweisen geschehen, sondern nur bei dem algebraischen Beweise. Ich glaube nicht, dass diese Versäumniss den Beweis werthlos macht. Ich glaube vielmehr, dass die Nicht-Berücksichtigung der höheren Singularitäten zunächst nothwendig ist, um das Verständniss des Beweis zu erleichtern. Ich würde mich sehr freuen, wenn Jemand durch Berücksicht der höh. Sing. den Beweis supplementirte. Ungenau ist der Beweis darum

noch nicht, weil er die übrigens üblicher Beschränkung auf die ursprünglichen Plückerschen Singular. aus sich selbst hervortreten lässt.

Wann wird meine Réponse im Bulletin etwa erscheinen ? Hatten Sie die Güte, mir Abzüge zu bestellen ? Augenblicklich habe ich das sehr interessante Problem den 1-1-1-Deutigkeit bei 3 geraden Punktreihen erledigt (Crelle, Bd.88, p.342 obnu)(Zusammenhang mit der klassen-allgemeinen Fläche 3ter Klasse) im Anschluss an das Erscheinen meiner Abh. in Crelle.

Mit der besten Grüßen

hochachtungsvoll
Dr.Schubert

B.18 February 20th 1880

20/2 80

Sehr geehrter Herr,

Umgehend schicke ich Ihnen selbst die Correctur zurück. Ich habe nichts zu Veränderndes gefunden, nur in der ersten Note fehlt noch die Angabe der Seite, auf welcher Ihre observations stehen, und welche Sie gefällt selbst hinzufügen werden. Der grosse Anfangsbuchstabe in "Livre" fiel mir auf, er ist aber wohl sprachlich gerechtfertigt.

Ich benutze Ihr freundliches Anerbieten, indem ich Sie bitte, mir zunächst tome IV des Bulletin bei der Société zu bestellen. Den Betrag dafür wie auch für die Separatabzüge kann ich dann wohl an M.Claude Lafontaine per Post schicken.

Von den Math. Ann. kostet jeder Band von Band VIII an 20 Reichsmark (die früheren kosteten erst 16, dann 18 Mark). Dies stimmt also mit dem von Ihnen angegebenen Betrage 26 fr, 75 cts überein, wenn die Reichsmark zu 1fr, 3375 gerechnet wird. Sie irren sich aber, indem Sie meinen, es erschienen 2 Bünde aller Jahr, das Erscheinen ist unabhängig von der Zeit, es sind bisweilen $1\frac{1}{2}$ Bünde, bisweilen 2 erschienen, ganz zwanglos, ebenso wie beim Crelle-Borch Journal. Die Math. Ann. sind in der That sehr theuer, viel theurer, als Crelle. Dies wührt zum Theil daher, weil der Verleger der Math. Ann. den Autoren Honorar zahlt (18 Mark pro Druckbogen, Crelle nicht), welches aber nicht in die Kasse der Autoren läuft, sondern auf die Erziehung der vielen, von Clebsch hinterlassenen, unmündigen Kinder verwandt wird.

Da wir nun einmal von den Kosten der mathematischen Nahrungsmittel sprechen, möchte ich noch anfragen, ob es wahr ist, dass die Comptes Rendus pro Jahr nur 18 bis 20 francs koste. In diesem Falle wollte nämlich die Bibliothek des Johanneums die C.R. halten, was mir sehr erwünscht wäre, ebenso wie den Naturwissenschaftlern.

Es freut mich sehr, von Ihnen zu hören, dass Sie meinen kleinen Beweis des Riemann'schen Satzes auf den Fall allgemeinsten Singularitäten ausdehnen wollen. Ich erwähnte den darauf bezüglichen Aufsatz mit Ungeduld, und fühle mich sehr geschmeichelt, dazu den Anstoss gegeben zu haben, und die Übersendung an die Math. Ann. vermitteln zu dürfen.

Ich selbst habe seit mehreren Woche die 1-1-1-Deutigkeit sowie die Charakteristiken-theorie und die Produktsätze (darunter wollen wir verstehen die Formeln, welche bei gewisser Beschränkung der Systeme die Zahl der gemeinsamen Gebilde als Summe von endlich vielen Produkten von Anzahlen der Systeme darstellen) liegen lassen müssen, theils wegen Krankheit in meiner Familie, theils wegen eines Vortrags über den neuen Seeweg-Integrator, theils wegen eines Besuchs den Prof. Oppermann aus Kopenhagen. Die 1-1-1-deutig bez. Planc. hoffe ich, bald wieder aufzunehmen, dann muss ich Ihre Joben über die Singul. auf Planc. sicher benutzen.

Mit den besten Grüßen

Ihr ergebenster
Schubert

Some letters from Zeuthen to Halphen

In what follows, we transcribe part of the Zeuthen-Halphen correspondence, preserved at the Paris Bibliothèque de l’Institut, in folder Ms 5624. We only produce the documents pertaining to enumerative geometry; however, there are about a dozen of letters left to transcribe in this folder. As Zeuthen wrote these letters in French, a language which he did not master fully, he made several grammatical mistakes, which are not corrected in what follows. However, we shall not indicate them systematically with a [sic], for the sake of clarity.

C.1 December 1st 1875

Copenhague 1 Décembre

Mon cher Monsieur Halphen,

Votre lettre, dont je vous remercie, contient une question à laquelle je n’ai pu vous répondre qu’à présent, celle sur la prorogation du concours sur la question des cubiques gauches. Il fallait attendre premièrement si au temps déjà fixé il y avait des réponses à la questions, et, lorsqu’il y en avait une, il fallait l’étudier et y appliquer beaucoup de preuves numériques pour voir si elle était digne de la prix. Elle s’en est montrée très-digne, selon moi et je suppose que les autres membres du comité seront du même avis, et alors une prorogation est impossible.

L’auteur fait des applications très-heureuses de ce qu’il appelle “la multiplication symbolique de M. Halphen”, et il vous cite aussi quant à la détermination des droites de l’espace dont il a besoin pour trouver les courbes singulières. Vous voyez donc que vous n’êtes pas absolument absent du concours ; mais, certes, j’aurais préféré de beaucoup, si vous auriez pu nous faire l’honneur d’y prendre part d’une manière plus directe. La révision de deux mémoires, dont l’un pourrait servir à éclaircir les points difficiles de l’autre,

m'aurait été, peut-être, plus facile que celle d'une seule.

Toutefois ce qui est le plus triste à cet égard c'est la circonstance elle-même qui a interrompu vos travaux dans cette direction. Je suis heureux de savoir que c'est un petit garçon que j'ai vu chez vous, premièrement parce que je sais alors que la petite fille n'était pas votre seul enfant, et ensuite parce qu'il me serait pénible d'entendre la perte de beau garçon ; mais je comprends bien et prends part à votre douleur de perdre un enfant chérie.

Je vous remercie de l'envoi de la démonstration de votre détermination du genre d'une courbe à singularités supérieures. Il m'intéressera d'y(?) voir aussi une fois la démonstration géométrique dont vous parlez ; après avoir lu votre note je m'en fait une, fondée sur les mêmes principes dont je me suis servi dans la démonstration géométrique de la conservation du genre d'une courbe à singularités ordinaires (Voir Salmon Higher pl. Curves) ; mais comme je n'ai formé cette démonstration (de votre extension) que dans la tête, il est possible qu'elle n'est pas complète.

Si vous verrez M. Fouret je vous prie de lui faire mon compliment pour son heureuse conception d'une théorie des implexes. Je n'en ai pas fait encore une connaissance plus profonde ; mais l'idée m'intéresse.

Et vous comprendrez, qu'ayant passé quatre semaines très agréables et utiles pour mon instruction à Paris, j'ai à vous [illegible] de me rappeler dans la mémoire de toutes ces personnes distinguées qui m'ont prouvé autant de complaisance et d'amitié. J'écirai bientôt à M. Chasles ; mais voulez vous bien à l'occasion porter mes remerciements pour des heures agréables et instructives à MM. Mannheim, Darboux, Painvin, Résal Moutard, Brisse En même temps je vous prie d'en garder une bonne partie pour vous même, et de croire que votre connaissance est une des très-bonnes parts(?) de mon séjour à Paris.

Votre très-dévoué

H G Zeuthen

Copenhague, Norrebro

C.2 May 13th 1876

Copenhague $\frac{13}{5}$ 1876

Cher Monsieur,

Je vous remercie de votre aimable lettre que je viens de recevoir et des intéressantes remarques qu'elle contient. J'attends toutefois, pour en profiter complètement, le cahier du Bulletin de la Soc. Math. que vous citez, et qui ne m'est pas arrivé encore ; mais je vais en écrire à M. Brisse.

Pour le moment je vous écris seulement à la hâte pour vous fournir l'adresse que vous demandez. M. Schubert vient d'obtenir une place à Hambourg en "Oberlehrer" à un lycée. H m'a indiqué l'adresse suivante :

Baumeisterstr. 1, Hambourg.

~~mais~~ Comme Hildesheim n'est pas une grande ville, il m'étonne qu'on n'en a pas envoyé votre lettre à Hambourg.

Je vous remercie d'avance de l'envoi que vous me permettez.

Votre très dévoué

H G Zeuthen

C.3 August 11th 1876

11 août 1876

Cher Monsieur,

je vous remercie beaucoup ~~pour~~ de la part que vous prenez à mon malheur, et pour la récréation que vous avez bien voulu me donner par vos intéressantes communications. C'est à celles-ci que j'essaierai ici de joindre quelques observations, qui ne sont pas le fruit de réflexions très-sérieuses, et dont par conséquent, la plupart ne vous disent peut-être rien de nouveau ; mais je vous les adresse pour le cas qu'il y resterait toutefois un peu qui ~~vous~~ puisse vous être utile à la continuation de vos recherches.

Premièrement, vous êtes, à ~~un égard~~ du moins, injuste envers Clebsch, qui expressément à la p.II ligne 3 que μ est la valeur maximum (grösste) de $\alpha + \beta + \delta$. J'avoue du reste que la démonstration de Clebsch m'a semblé toujours extrêmement difficile ; par conséquent, quand même je n'ai pas trouvé des objections à faire aux détails, je n'aurais pas osé adopter le théorème à la seule base de cette démonstration ; et à présent il ne m'est pas entièrement clair pourquoi vous ~~voulez~~ substituez $\beta + 2\gamma + \delta$ à $\beta + \gamma + \delta$, quand même je comprends parfaitement votre transformation corrélative.

Quant à l'exactitude du théorème de Chasles, l'exemple dans votre première lettre est parfaitement clair : il faut ~~y~~ soit joindre au théorème une restriction – vous l'indiquiez dans votre seconde lettre, soit donner d'autres définitions aux mots de solution propre et solution impropre, que celles de Clebsch et, si je ne me trompe pas, Lindemann (mon exemplaire de son livre est à Copenhague, mais je suis à la campagne). En effet, selon mes remarques à la p.7 du mémoire de Clebsch (lignes 8 – 10) les solutions impropres dont le nombre ne fait pas partir des $\mu a + \nu b$, sont celles qui sont indépendantes des paramètres donnés : il faut donc, selon cette définition dans votre exemple regarder ~~les solutions~~

comme impropre la solution du $\frac{n-n'}{R-R'} = n\omega$ nombre donné, qui résulte de la circonstance que $n - n' = 0$ et $R - R' = 0$ et le théorème de Chasles ne sera plus applicable au nombre des solutions propres.

On pourrait peut être garder le théorème de Chasles en prenant un autre point de vue⁽¹⁾, d'où cette solution – qu'on trouve si le système contient une conique inf. aplatie à sommets coïncidents – n'est plus impropre ; mais alors il faudrait fixer (préciser) clairement ce point de vue. Cela n'étant pas fait, vos remarques serviront en tout cas à compléter le théorème de Chasles en précisant le sens qu'il faut y attribuer. Malheureusement, je ne sais pas encore obtenir cette précision de la représentation du point de vue d'où le théorème reste vrai, et je me bornerai pour cette raison à indiquer à peu près les points de vue possibles. [*In a footnote :*] (1) Vous verrez qu'à la fin de la lettre j'ai altéré un peu de cette opinion.]

1° Le premier point de vue est celui de Bischoff-Jonquières. Les coniques (courbes) sont ici définies exclusivement par leurs propriétés ponctuelles. Une tangente est une droite qui rencontre les coniques en deux points coïncidents, les cas de coniques ordinaires avec leurs tangentes se présentent donc à côté de ceux où la conique se réduit à une droite double, et où par conséquent – du point de vue actuel – il faut regarder la conique comme tangente à toute droite (courbe) du plan. Le nombre total de coniques d'un système satisfaisant à une condition donnée sera égal au produit $\alpha\mu$ de deux nombres qui dépendent l'un du système, l'autre de la condition.

Ce point de vue a l'avantage d'être simple et parfaitement clair. La distinction du “général” et du “particulier” qui cause des difficultés au point de vue suivant, se fait donc ici immédiatement. Mais à cet avantage se joignent plusieurs faiblesses. Premièrement, il s'altère par les application du principe de dualité, qui conduit à la définition des coniques par leurs propriétés tangentielles (de ce point de vue un point “se trouve sur” une conique si les deux tangentes menées par lui coïncident etc.) Cette duplicité cause des difficultés (paradoxes apparents) analogues à celles qu'on éloigne dans la théorie des courbes algébriques par les formules de Plücker. Plus grave est peut-être la circonstance que l'introduction successive des conditions par la formule $\alpha\mu$ conduira, sitôt qu'on a dépassé certaines limites – au delà desquelles on devrait trouver, du point de vue actuel, une infinité de solutions – à des résultats sans aucun sens immédiat. On trouve par exemple 8 coniques passant par un point et tangentes à 4 droites au lieu de toutes les droites doubles par le point.

2° Ces difficultés conduisent au point de vue de Chasles, où l'on ~~regarde comme~~ a égard d'une manière égale aux propriétés ponctuelles et tangentielles. ~~On ne définit donc plus entièrement~~ Une conique qui se réduit à une droite double ne sera donc plus entièrement déterminée si l'on connaît la position de la droite qui contient ses points, il

faut encore connaître les deux sommets des faisceaux de ses tangentes. Pour ce point de vue on aura le théorème de Chasles qui en est peut-être une conséquence si intime que sa démonstration se présenterait d'elle-même, si l'on savait bien définir précisément ce point de vue. Les difficultés à cet égard résultent du point de vue de départ double qui empêche par exemple de distinguer clairement les catégories du "général" et particulier : A peut être un cas particulier de B pour la définition ponctuelle, pendant que B est un cas particulier de A pour la définition tangentielle. On rencontre les mêmes difficultés dans les autres cas où l'on fait usage du même point de vue, notamment dans la théorie des singularités dites ordinaires des courbes planes (formules de Plücker) des courbes gauches et des surfaces, et de celles des systèmes de courbes d'ordre supérieur. M'étant occupé beaucoup de toutes ces questions, j'ai eu lieu de connaître assez bien ces difficultés qui ont rapport toutefois plus à l'énoncé complet et exact des vérités qu'à leur découverte et démonstration.

Votre exemple me montre qu'on peut encore ici trouver des cas où l'introduction successive des conditions souffre de la même faiblesse que nous avons indiquée pour le point de vue de Jonquières. Cherchons par le théorème et les substitutions géométriques de M.Chasles le nombre de coniques ayant des contacts quadruponctuels (du 3^{eme} ordre) avec une courbe donnée (en des points inconnus), et satisfaisant à deux conditions de la nature que vous m'avez indiquée. Alors on trouvera un nombre fini ; mais, selon notre point de vue qui nous fait regarder comme exacte l'application du théorème de M.Chasles au cas où les coniques sont déterminées par une de ces conditions par une condition quadruple qui admet des coniques inf. aplaties à sommets coïncidents, toutes les coniques aplaties qui se trouvent dans les tangentes de la courbe et dont les deux sommets coïncident avec les points de contact devraient se présenter en solutions. Je ne doute pas que le nombre fini de solutions ait une signification, mais non pas celle qui est exprimée par l'énoncé de la question et par le point de vue où nous nous trouvons.

Ce dernier exemple, que je viens de former d'après vos indications, me persuade, plus que je n'en étais persuadé au commencement de cette lettre, de l'insuffisance de la théorie de Chasles, qui regagnera tout son intérêt et importance par le supplément que vous y ajoutez. Votre point de vue, qui est aussi celui où se veillent placer Clebsch et Lindemann, est entièrement clair et bien défini. Vous ne voulez indiquer que le nombre des solutions propres, celles qui sont indépendantes des valeurs données étant regardées comme impropres. Si vous êtes bien sûr d'éviter par vos restrictions tous les cas où il y en a des solutions impropres qui ne sont pas déjà exclues de la formule $\alpha\mu + \beta\nu$, votre théorème sera absolument exact complet. Permettez moi seulement la question s'il ne serait pas possible de lui donner une forme positive en remplaçant la restriction par un nouveau terme ajouté à l'expression $\alpha\mu + \beta\nu$.

Vous me pardonnerez d'avoir écrit autant d'une question dont vous n'avez pas encore fini vos recherches personnelles ; mais je ne prétends de dire rien qui ne soit présenté à nous-même. Peut-être mes remarques vous feront observer les contre-objections que ceux qui sont moins unanimes avec vous que moi pesant à votre critique ou supplément du théorème de Chasles et des démonstrations de ce théorème, et d'y avoir égard dès à la publications de vos remarques. En tout cas je vous les adresse ici.

Votre très dévoué H G Zeuthen

October 5th 1876

5 octbr 1876

Cher Monsieur,

Répondant soit tout de suite, soit avec un retard tout-à-fait indéfini, aux lettres que je reçois, je choisis pour le moment la première alternatifs.

Je vous remercie donc premièrement de votre défense envers M.Bertrand. Vous avez raison. Je n'ai pas pris une part active aux démonstrations du théorème $\alpha\mu + \beta\nu$, quant même j'avais, je l'avoue une entière confiance en ce théorème. J'avais cette confiance avant les démonstrations qui me semblaient trop difficiles pour l'augmenter essentiellement. J'ai toutefois un seul reproche à me faire à cet égard : c'est pour mes remarques à la fin de mon analyse du livre de Lindemann (Darb. Bull.). Heureusement j'ajoute les mots suivants : "il est très difficile d'affirmer qu'en des démonstrations de cette espèce il ne reste plus aucun point faible etc."

Les géomètres ont aussi à se demander s'ils n'auraient pas fait usage du théorème incomplet, et, dans ce cas, à discuter si leurs démonstrations restent en vigueur avec vos modifications de ce théorème. A cet égard je ne me trouve guère gêné ; car mes démonstrations ont été ordinairement indépendantes de ce théorème. Dans un seul travail (Nouvelles Annales 2^e série t.VII) je me suis servi de son analogue pour les surfaces du second ordre, ou plutôt de l'analogue du théorème de Cremona ; mais c'était avant les démonstrations de ces théorèmes et avec une complète conscience de l'insuffisance de la base de ma déduction. Je me suis persuadé de la justesse des résultats que j'y obtiens, et je crois qu'ils seront faciles à démontrer lorsque vous aurez reconstruit la théorie des formules

$$\alpha\mu + \beta\nu + \text{Suppl.} ; \alpha\mu + \beta\nu + \gamma\rho + \text{Suppl.} ; , \alpha\mu^2 + \text{etc.}$$

mais alors ~~seulement~~ une partie essentielle de la démonstration de mes résultats n'appartiendra à ~~moi~~ pas à moi.

Je suppose que les reproches de M.Bertrand repose sur un malentendu. Dans le t.III des “Mathematische Annalen” j’ai démontré que le théorème de M.Chasles $n'\mu + n\nu$ a lieu pour les systèmes de courbes de tous les ordres la détermination du nombre des courbes d’un système (μ, ν) d’ordre quelconque qui sont tangentes à une courbe indépendante du système d’ordre n et de la classe n' et à singularités ordinaires.

Cette dernière restriction ne sera pas, je suppose, difficile à éloigner. ~~Plus tard M. Brill~~ a M.Chasles n’avait pas démontré complètement ce théorème ; plus tard M.Brill a donné une démonstration plus simple de ce théorème et de son analogue pour l’espace.

Mais j’aperçois que j’ai ici l’air de laver mes mains de la manière la plus phariséenne devant vous qui vous accusez vous même de la faute dont je parle. Soyez donc assuré que je n’ai pu parler ainsi que parce qu’il me semble que vos études dans cette direction méritent notre admiration entière. Vous avez fait, premièrement, dans nos camps(?) extrêmement difficiles, des recherches qui conserveront, sans doute, leur importance si vous y appliquez les modifications rendues nécessaires par le défaut que vous venez de découvrir – à moins que vous ne saurez y substituer des démonstrations plus simples. Ensuite en découvrant, vous-même, ce défaut vous réussirez à jeter une lumière tout à fait nouvelle sur cette théorie, et à substituer la sureté aux hypothèses résultant d’inductions. En même temps vous expliquez un fait qui m’a semblé toujours étrange, celui que ce théorème si simple (qu’il était dans la forme de M.Chasles) demandait des démonstrations si compliquées que, même en en saisissant les détails, on n’était pas sûr de suivre la marche entière. Espérant une démonstration plus facile, je croyais (hélas pour moi) en trouver dans le livre de M.Lindemann, où je trouvais bien quelques conclusions difficiles ou peut-être douteuses mais aucune qui me semblât fausse⁽¹⁾. Le lemme que je cite dans mon analyse, est il peu juste injuste aussi ? [*in a footnote :*] (1) Je ne parle pas ici de ~~la~~ sa démonstration du théorème de Cremona, où j’ai signalé une faute. M.Schubert m’a fait remarquer que celle-ci est plus grande que je ne supposais d’abord.]

A un égard je suis très disposé à défendre M.Lindemann envers vous. Vous dites ~~que~~ ~~toutes~~ (“entre nous”) que tout ce que l’on voit dans son livre sur cette théorie vous paraît être un pillage éhonté, augmenté des erreurs les plus grossières. En le défendant à cet égard, je me défends en quelque sorte moi même ; car, selon moi, un “pillage éhonté” est un crime (mon dictionnaire me dit que “éhonté” est une expression plus forte que “effronté”), et en rendant compte de son livre sans ~~apperee~~ apercevoir ce crime ~~je montrerais~~ j’aurais montré une légèreté qui était près de me rendre son complice.

Mais quel est donc ce crime (musal(?), car je ne parle pas ici des erreurs qu’il y ajoute) : Il cite loyalement vous et Clebsch, en essayant de substituer à vos démonstrations des démonstrations plus simples. Il fait donc savoir à tout le monde qu’il ne construit pas ses démonstrations indépendamment des vôtres, quant même il ne rend pas compte en

détail des emprunts qu'il fait à l'un et à l'autre⁽¹⁾. Il n'aurait donc violé les droits de personne, et il aurait rendu un service réel à la science, si, en profitant des travaux des prédécesseurs qu'il cite, il avait construit des démonstrations plus courtes et plus simples, ce qu'il a cru faire. S'il n'a pas réussi à cet essai, si la simplicité est obtenu par l'omission de déductions nécessaires, si, même, il y ajoute des fautes positives, tant pis pour lui (et pour moi qui ne l'avais pas découvert avec mon compte rendu) ; mais il n'a pas fait une action immorale. [*In a footnote :*] (1) Il avait été à souhaiter qu'il aurait dit expressément que, dans la démonstrations du théorème de Cremona, vous avez, le premier, fait usage d'une conique fixe rencontrée aux mêmes points par des coniques satisfaisant aux deux conditions multiples. J'y ai fait une allusion, peut être trop faible, dans mon analyse.]

Il vous semble peut-être que je prends ici une hyperbole plaisante de votre part trop au sérieux ; alors je vous prie de croire que, devant me servir ici d'une langue étrangère, j'ai malgré moi une mine plus sérieuse que je ne montrerais, si je n'avais pas cette difficulté à surmonter.

Vous me demandez si j'ai écrit quelque chose sur les singularités des surfaces gauches. Non. Seulement mes discussions des singularités des courbes gauches (Annali di Matematica 2 ser. III) m'ont conduit à établir quelques résultats qui ont égard aux surfaces gauches dont une courbe donnée est 1, 2 ou 3 fois directrice ou qui ont d'autres relations avec les courbes gauches. Plusieurs de ces résultats étaient du reste établis par M. Cayley dans ses deux mémoires sur les surfaces gauches (otherwise scrolls)

Je saisis l'occasion pour vous rappeler que vous m'avez promis ~~une fois~~ de m'envoyer votre portrait.

Votre très dévoué
H G Zeuthen

Mon adresse sera dorénavant

Niels Ebbesens Vej
Copenhagen
V

C.4 December 1st 1877

Copenhagen V., Niels Ebbesens Vej 20
1 décembre 1877

Cher Monsieur,

Plus je considère les résultats que vous me communiquez dans votre lettre, plus ils m'intéressent. Une vérité mathématique a pour moi quelque chose d'effrayant lorsque je n'entrevois pas sa connexion avec d'autres vérités ou une voie qui pourrait y conduire, et c'est cette entrevue qui commence à s'ouvrir. Pour la connexion avec d'autres vérités je n'ai pas besoin toutefois de me contenter d'une entrevue, pouvant essayer d'appliquer vos résultats généraux à des cas particuliers notamment à une courbe cuspidale $n = 2, v = 1^{(*)}$. Quant à la déduction, je me contente à me dire que si je devais démontrer les résultats que vous venez de trouver, je voulais essayer de me servir d'un procédé analogue à celui que j'emploie dans mon mémoire sur les surfaces réciproques et que j'ai exposé aux pages 462-469 ; mais je vois bien que j'aurais à surmonter plusieurs difficultés avant de parvenir à une démonstration rigoureuse. Je suis assez curieux de savoir si votre procédé est bien différent de celui-ci. *[(In a footnote :) (*) Je vois aussi, en ce moment, que le résultat le plus simple ayant égard à votre sous-groupe “(V, V) indicatrice non parabolique” est renfermé au résultat principal de mon mémoire : “Sur une classe de points singuliers de surfaces” Math. Annalen IX.]*

Vous me priez de vous prévenir de mes objections. Je n'en ai pas pour les beaux résultats communiqués, mais seulement pour votre hésitation d'assaillir les points singuliers dont la discussion fait la suite naturelle de votre discussion actuelle. Dans mon mémoire je ne m'occupe que des premières singularités ; comme on ne sait faire de celles-ci des substitutions analogues à celles des équivalents pluckériens dans le plan, on a un besoin d'autant plus grand de recherches générales comprenant des singularités de tous les degrés de multiplicité (Je regarde bien des points μ -tuples, mais je suppose que leur cône tangent n'a que des singularités pluckeriennes). Quant même vous auriez besoin de séries contenant deux variables, vous pourriez achever cette étude mieux qu'aucun autre mathématicien.

Je suis aussi curieux de connaître votre autre mémoire sur les courbes gauches. Je ne reçois pas très-régulièrement le bulletin de la Société Mathématique ; il faut écrire parfois à M. Brisse quel n^o est le dernier que j'ai reçu. Si je résous un mois de lui écrire, j'espère que votre mémoire sera alors déjà publié de façon que je puisse avoir ainsi le cahier qui le contient. Je suis du reste bien content de pouvoir – quant même avec ces retards – obtenir le Bulletin en échange de mon “Tidsskrift for Mathematik” qui ne peut intéresser beaucoup des membres de votre Société.

C'est assez bon que j'ai pu vous écrire de vos Travaux ; car si je devais me tenir aux miens, je devrais me taire, selon les principes énoncés dans votre lettre. Je n'ai pas négligé de travailler, mais les fruits ont presque manqué. J'espère toutefois que j'ai gagné plusieurs connaissances qui me seront utiles en ma qualité d'un des représentants peu nombreux des mathématiques dans un petit pays, où il n'est pas permis de se borner à

ses études de prédilection. Aux objets dont j'ai fait ainsi la connaissance appartient la statique graphique ~~dont j'ai~~ que je ne connaissais presque pas il y a un an, et dont j'ai été assez hardi de faire un cours et d'écrire un petit mémoire – très-innovant du reste – dans mon Tidsskrift. Sans doute, mes pensées ne se sont pas abstenues pendant ce long temps de la théorie des courbes et des surfaces, mais je n'ai pas été heureux à cet égard : parfois je me suis occupé en vain de problèmes trop difficiles, parfois la matière a été si simple que mes petits résultats ne pouvaient intéresser. Pour le moment toutefois j'ai trouvé une matière où il y aura, j'espère, quelque chose à faire pour moi. J'ai indiqué, il y a 3 ou 4 ans, dans les C^{tes} rendus, une détermination des nombres pluckeriens de l'enveloppe d'un système donné de courbes. A présent j'ai commencé de les appliquer à une étude plus soigneuse de systèmes de coniques et de surfaces de second ordre et de leurs enveloppes. La richesse de la matière se montre au premier pas où j'ai dû m'arrêter dès à l'étude des systèmes aux caractéristiques $\mu = 2$, quant même cette première étude appartient encore à celles dont la simplicité rend les résultats moins intéressants. Les systèmes de coniques et quadriques à la caractéristique $\mu = 2$ sont "unicursales" ($p = 0$), de façon qu'on les peut représenter analytiquement sans difficulté et les étudier au moyen de leurs équations ; mais, par gout j'évite les calculs où je peux.

Un de mes amis plus jeunes M. Crom vient de construire quelques modèles de surfaces développables que j'ai accompagné d'une préface ; mais je n'en ai pas eu des exemplaires à ma disposition. Le libraire en a envoyé des exemplaires à plusieurs professeurs ; je crois aussi qu'il en a envoyé un à la Société mathématique où peut-être vous les avez vues ; mais leur but est plutôt d'être instructifs ~~et~~ aux élèves que de montrer quelque chose de nouveau aux savants.

Mais j'oublie de vous demander où devient votre mémoire sur les caractéristiques. Depuis avril je l'ai cherché en vain dans le Journal de Mathématiques. Etait-ce peut être tout le mémoire que vous avez publié dans les C^{ptes} Rendus il y a plus d'un an. J'attendais que vous nous exposiez encore de ~~systèmes~~ la correction analogue pour les systèmes de quadriques. – Il y a longtemps que je n'ai eu des nouvelles de M. Schubert.

Vous jurez sur les caractéristiques de me répondre. J'espère qu'il n'est pas sur le théorème inexact de $\alpha\mu + \beta\nu$.

Votre très dévoué H G Zeuthen

C.5 December 15th 1877

15 décembre 1877

Cher Monsieur,

Je ne hais pas le calcul du tout, seulement pour moi-même je veux bien être exempt de faire les calculs. J'avoue toujours franchement que les déterminations des nombres de solutions dont je m'occupe et les applications de ces nombres ont pour base l'analyse algébrique. La question de notre Société, que je vous ai adressée trop tard, montre que je serai bien content de voir d'autres surmonter les difficultés algébriques qui seraient insurmontables à moi. Voilà aussi pourquoi je vous engage à cultiver les points singuliers des surfaces ; car ce qui est à faire dans cette matière demande, à côté d'un œil ouvert à toutes les propriétés géométriques, une habileté analytique qui n'est pas à ma disposition : et vous, Monsieur, disposer de toutes ces deux conditions.

Néanmoins je crois trouver une faute de calcul dans votre réponse à mon invitation à cet égard. Vous dites que votre mémoire actuel est trop long bien qu'il contienne seulement la moitié de la théorie des courbes singulières, et que vous ne [illegible] songer à y mettre aussi les points singuliers. Mais la moitié qui manque n'est elle pas la théorie des courbes singulières planes, où les nappes de la surface sont tangentes en même plan: et vous avez remarqué, dans votre première lettre, que ces plans correspondent aux points singuliers de la surface corrélative.

Vous attribuez au calcul l'honneur de vous avoir remis dans le droit chemin quant à $\alpha\mu + \beta\nu$. Sans doute vous devez connaître les moyens qui vous ont fait découvrir la vérité à cet égard, et je sais que dans les démonstrations vous vous êtes servi du calcul. Il me semble néanmoins notable que c'est vous, Monsieur, qui êtes le plus géométrique de ceux qui se sont occupé de cette question – qui avez réussi à l'éclairer complètement (Chasles n'avait fondé son hypothèse que sur une pure induction).

A propos de $\alpha\mu + \beta\nu$, avez vous vu qu'un auteur italien, j'ai oublié son nom, a entrepris – immédiatement avant votre découverte – de démonstration au moyen du théorème $\alpha\mu + \beta\nu$ les nombreux théorèmes exposés par Chasles en 1864. Il dit que Chasles les a énoncés sans démonstration ; mais, selon moi, l'énoncé du principe de correspondance, l'exécution de quelques exemples, et l'ordre des théorèmes indique suffisamment sa démonstration, pendant que vous avez fait celle de l'auteur italien illusoire.

Je continue l'étude dont je vous ai écrit, et je trouve beaucoup de choses mais – hélas – ordinairement ce que je trouve de bon n'est pas nouveau, et ce que je trouve de nouveau n'est pas bon. Je ne croyais pas de vous faire une intéressante communication en vous faisant que le genre des courbes et surfaces à la caractéristique $\mu = 2$ est égal à zéro, ce que j'établis de la même manière que vous ; j'en [illegible] pour vous faire remarquer la simplicité de la représentation algébrique de ces systèmes.

La lenteur de ceux qui devraient se faire un plaisir à s'empresse de publier vos mémoires est tout-à-fait intolérable. Il devient par exemple difficile de publier quelque chose sur les caractéristiques avant de connaître toute la portée de vos découvertes à cet égard.

Je vous remercie de votre prompte réponse à ma précédente lettre, et je reste

Votre très dévoué

H G Zeuthen

J'ai reçu le Bulletin de la Société Mathématique jusqu'au troisième cahier du Tome IV inclusivement ; je n'écris pas cela par impatience, mais seulement pour le cas qu'il vous dérangerait d'aller le voir dans les registres de la Société. J'espère que la Société a reçu régulièrement les cahiers de mon Tidsskrift, que j'ai envoyés toujours à M. Brisse, à qui j'ai causé ainsi de la peine dans cette année. 5 cahiers de l'année 1877 lui ont été adressés. J'adresserai le sixième et dernier de la présente année directement à la Société, 7, Rue des Grands Augustins, et je ferai de même pour l'année suivante.

H Z.

C.6 September 5th 1878

Niels Ebbesens Vej 20, Copenhagen V

5 Septbre. 1878

Cher ami

C'était bien aimable d'ajouter à la série de vos autres obligeances celle de suppléer, dans une lettre, à mon défaut de présence d'esprit lorsqu'il s'agirait de saisir des idées mathématiques exposées oralement. En lisant votre lettre, je vois que vos raisonnements sont assez simples pour devoir être saisis tout de suite ; mais en même temps j'y trouve aussi des remarques sur lesquelles j'étais bien aise de pouvoir réfléchir chez moi. J'avais besoin par exemple, d'une réflexion pour m'assurer du fait dont vous faites usage, et qui me paraissait plausible, seulement, à la première lecture, que l'ordre m d'une conique dégénérée est égal à son degré de multiplicité dans la formule $2\mu - \nu$ (Vous ne définissez expressément dans votre lettre que le rapport $\frac{m}{n}$; mais je suppose que, pour définir explicitement m ou n , vous faites, dans le voisinage d'une conique singulière, le système ou la "branche" du système dont il s'agit, dépendre d'une manière rationnelle d'un paramètre variable qui peut être zéro pour la conique singulière : m est alors le double de l'ordre du segment infiniment petit intercepté sur une droite quelconque par la conique correspondante à une valeur infiniment petite du 1^{er} ordre du paramètre).

Dans cette question je suis de votre avis si je l'ai bien compris ; mais il n'est pas de même pour une autre remarque que vous faites en passant et qui est – comme vous me faites remarquer – sans influence à vos résultats. Vous dites que le système $P^2(\mu q)D$

ne contient qu'une seule conique dégénérée (3^{me} esp.), qui doit être, par conséquent, de l'ordre 8μ et de la classe $8q$.

Il est vrai que toutes les coniques dégénérées (3^{me} esp) de ces systèmes doivent coïncider ; mais, je vous ai bien compris, vous voulez dire qu'une conique variable du système ne peut que par une seule voie tendre à coïncider avec cette conique singulière, et ensuite s'en éloigner (le sens de la voie étant indifférente), ou bien qu'une seule "branche" du système "passe par" la conique singulière. Dans ce cas, sans avoir discuté en détail les branches du système qui y passent, j'ai une objection qui me semble essentielle. Deux point étant donnés, une conique variable du système peut tendre à coïncider avec la conique singulière de deux manières visiblement différentes : celle où les points se trouvent du même côté de l'axe de la conique "pénultième", et celle où ils se trouvent de différents côtés (alors la même chose aura lieu après la coïncidence)*. ~~Vous pourriez dire que~~ Si vous avez raison, seulement une de ces voies conduit à une véritable conique limite du système, mais quelle donc ? Et pourquoi l'une plus que l'autre ? [(In a footnote :) ★ Vous savez que la même circonstance est la cause ~~que~~ pourquoi le système (P^2D^2) contient deux fois sa conique applatie, qui est toutes les deux fois de l'ordre 2. Dans ce cas, non seulement cette conique singulière appartient à deux branches, mais le système se décompose en deux ; il est du genre $\div 2$.]

Je croirais donc, a priori et sans avoir des raisons complètes, que le nombre de coniques dégénérées est 2 ou un multiple de 2, et si vous avez raison je verrais ici un des paradoxes apparents, utiles à étudier pour éviter dorénavant des conclusions incertaines dont j'aurais alors connaissance.

Mes remarques vous auront montré que j'ai compris la difficulté à la détermination des équivalentes d'une condition et l'analogie avec la difficulté de la distinction des branches formant un point singulier d'une courbe. C'est à cause de cette difficulté, sans doute, que vous appelez votre moyen de cette détermination, théorique. Il sera aussi en beaucoup de cas difficile à employer ; mais je crois que la difficulté appartient alors à la question elle-même, et que vous avez indiqué aussi le véritable moyen dont il faut se servir en pratique pour des conditions données.

Rentré chez moi j'ai essayé de me rendre, le mieux possible, maître des différentes impressions de mon excellent voyage, et de faire un plan pour profiter le plus possible des observations mathématiques que j'ai entendues, et qui m'ont indiqué de nouvelles voies, ou qui m'ont fait sentir le besoin de réparer mes connaissances faibles en beaucoup d'endroits de parts des mathématiques. J'ai ramené beaucoup de renseignements sur des livres à étudier, et beaucoup de mémoires offerts par les auteurs. Trouvant en même temps beaucoup à faire ici j'ai éprouvé un peu du même sentiment de peur de noyer qu'à l'Exposition.

J'ai essayé de me tirer d'affaire en commençant – à côté de travaux obligatoires et personnels assez indépendants de mon voyage – par étudier vos invariants différentiels, et en profitant de l'occasion pour connaître la manière de M. Hurwitz de traiter des fonctions doublement périodiques. Ne faisant que commencer, je n'ai du reste aucune question ou remarque à vous dresser à cet égard.

Encore une fois, je vous remercie de tout ce que vous avez contribué à faire mon séjour à Paris si agréable et si utile pour moi, soit en me parlant de mathématiques de toute autre chose, soit en me conduisant à la campagne chez vous et votre famille, soit en me conduisant au Louvre. Je vous prie de faire mes compliments à Madame Halphen et à sa famille.

Votre très dévoué

HG Zeuthen

C.7 December 14th 1878

Le 14 Décembre 1878

Cher ami,

Me voilà qui vous fasse attendre déjà long temps à la réponse de deux aimables lettres. Quant même mes remords ne se sont pas manifestés de la seule manière pratique je vous assure qu'ils ~~ont~~ existent. En tout cas je vous prie de ne juger pas de la satisfaction que m'ont causé vos lettres d'après mon silence. Au contraire, si les explications complètes que vous avez bien voulu me donné sur les multiplicités en question ne m'avaient pas satisfait, je me serais hâté sans doute de vous montrer la finesse de ma critique. Il n'était pas, du reste, dans l'espoir de trouver un autre objet de ma critique, qu'ayant reçu la première partie de votre mémoire dans les "Proceedings", j'attendais la seconde avant de vous répondre – et que je me contentais de vous avertir par une carte de poste de mon indiscretion dans le "Tidsskrift" –, mais plutôt pour vous pouvoir faire des questions s'il en serait besoin. En vous remerciant de cette notable contribution – à laquelle je préfère pourtant les considérations plus simples et directes exposées au congrès et dans les très intéressants mémoires écrits pour moi dans vos lettres – à une théorie qui renferme les mots de beaucoup d'énigmes algébriques et géométriques, je n'ai ni des critiques ni des questions à vous diriger à cet égard, mais seulement un à propos.

Vous indiquez un critère, très-bon pour les coniques, des solutions propres et impropres, les unes ~~étant exprimées par~~ dépendant exclusivement des covariants les autres aussi d'invariants, mais serait-il bon d'appliquer le même critère aux courbes d'ordre supérieur,

où toute courbe ordinaire a des caractères projectifs et indépendants de tout autre partie de figure du plan, qui la distinguent d'autres courbes ordinaires. Quel est donc le critère pour les courbe de tous les ordres ? Je n'essaierai pas de l'énoncer complètement, mais je sais qu'il faut le chercher à la circonstance qu'on obtient les solutions impropres en donnant à une quantité qui se présente sous la forme de $\frac{0}{0}$ une valeur différente de la valeur-limite de cette quantité. Sans doute, on élimine de cette façon du moins une détermination dépendant de parties de figure extérieures, et une réflexion venant pendant l'écriture est près de me faire croire que la courbe sera alors déterminée par les autres conditions extérieures et une condition intérieure – de même que pour les coniques.

Je n'ose pas décider sur cette pensée de ce moment sans des réflexions ultérieures ; mais votre pensée a-t-elle peut être déjà parcouru des réflexions semblables – quant même les coniques ont été l'objet particulier de vos recherches –. Alors j'aurais pourtant cette question à vous faire, “si votre critère s'applique aussi avec peu de modifications à des courbes d'ordre supérieur”, et j'espère que vous m'y répondrez ~~après-avoir~~ sans punir trop long temps mon silence.

Mon intérêt est fort occupé d'un petit travail ~~qui m'occupe~~ sur les surfaces du quatrième ordre à conique double. L'occasion de cette étude était premièrement le projet de profiter de la connaissance des figures des courbes planes du quatrième ordre pour connaître celles de ces surfaces, qui ont (le centre de projection étant pris sur la conique double) pour contours apparents des courbes générales du quatrième ordre, et d'étendre ainsi nos connaissances assez faibles des figures d'espace définies algébriquement (les connexions des nappes etc.). Dans le cours du travail je suis conduit à y appliquer aussi d'autres considérations, et à chercher aussi de nouvelles déductions des propriétés géométriques des surfaces. J'ai saisi aussi une occasion pour noter un cas particulier de notre problème sur les 12 tangentes – du quel aucune solution n'est venu au terme fixé –. Le cas particulier, qui dépend du reste de constructions connues depuis long temps, est celui-ci : Si l'on mène d'un point les droites aux quatre points d'intersection de deux coniques, et les deux couples de tangents aux mêmes coniques, on peut construire une infinité triple de courbes du quatrième ordre tangentes aux quatre premières droites, et ayant l'une des deux couples pour tangentes doubles, et ayant des points doubles sur l'autre.

Le théorème réciproque est vrai pour toute quartique plane à deux points doubles, mais seulement pour quatre des points d'intersection de ses tangentes doubles.

J'ai invité un de mes plus jeunes amis ici M.Csane(?) à rechercher des cas particuliers analogues, ce qui ne serait pas peut-être sans intérêt.

Je regrette beaucoup de ne me pas me rappeler les résultats que vous m'avez fait connaître sur le même problème ; car j'espère que je ne viens pas de les rappeler ici trop bien en oubliant d'où cette idée me serait venue alors ?

Le bon souvenir de la famille de Madame Halphen m'a vivement touché, et je vous prie de porter mes félicitations sincères aux jeunes mariés si vous les verrez en Normandie ou à Paris, et à Monsieur et Madame Aron. Je suis heureux de pouvoir ajouter après mon dernier visite à Paris à ma lettre mon compliment à Madame Halphen. Mes meilleurs souhaits pour votre famille.

Votre H G Zeuthen

C.8 November 5th 1879

Citadelsvej 9
Copenhague φ
5 Novembre 1879

Cher Monsieur,

J'ai peur que ce soit moi qui suis coupable cette fois de la longue interruption de notre correspondance. Dans ce cas, vous n'êtes pas, du moins, sans nouvelles dans le long intervalle. Une carte vous aura annoncé un évènement très heureux pour moi, et une brochure, qui va vous être suivie d'une autre, vous aura dit que l'amour ne m'a pas empêché de travailler. Au contraire j'espère que le bonheur de famille sera bon pour mes travaux scientifiques.

Vos deux brochures viennent de me porter aussi de vos nouvelles ; mais cela ne m'empêche pas de désirer de voir bientôt une de vos lettres amicales, qui me causent toujours beaucoup de plaisir. J'essaye de m'en procurer en saisissant la plume dans ce moment où mes pensées s'occupent de deux de vos découvertes mathématiques. J'essaie dans le semestre actuel de faire un cours sur la "abzählende Geometrie*". Je profite à beaucoup d'égards du nouveau livre de Schubert, mais plus que lui qui expose le "calcul" der abz. Geometrie je cherche à exposer les méthodes de cette science et à les appliquer à beaucoup d'exemples. Notamment mes études des formes limites me fournissent des exemples de la méthode bien connue à laquelle M.Schubert a donné le nom très expressif du principe de la conservation des nombres. Je dois avouer, à ~~cette occasion~~ cet égard, que ce nom et quelques exemples bien choisis de M.Schubert m'ont été l'occasion d'en chercher d'autres, ~~je vais vous en exposer~~ à côté de ceux que je possédais avant. Je vais vous exposer deux exemples de cette méthode que j'ai trouvés aujourd'hui. [(In a footnote :) ★ Comment traduire ce mot ?]

I. Soit donnée une congruence de droites quelconque, et soit n le nombre de droites par un point, n' celui des droites d'un plan. Si l'on cherche les droites de cette congruence satisfaisant à de nouvelles conditions indépendantes de celles de congruence, il est commode

de la regarder comme cas particulier d'une série de congruence résultant de la première par des transformations homographiques. Le nombre cherché sera le même pour toutes ces congruences si seulement dans les cas particuliers on détermine exactement les nombres de solutions coïncidentes : il faut ~~seulement~~ regarder les cas particuliers comme des cas limites. On peut donc trouver le nombre cherché en ~~regardant~~ considérant un autre cas particulier. Pour définir celui-ci nous repérons la congruence donnée à un tétraèdre de coordonnées : soit x, y, z, u les coordonnées d'un point. Alors on se sert des formules $x : y : z : u = \frac{x'}{\varepsilon} : \frac{y'}{\varepsilon} : z' : u'$, où ε est une quantité infiniment petite. On obtient alors une congruence de droites rencontrant ~~le~~ l'arête $x = 0, y = 0$. Les droites d'un plan par cette arête envelopperont une courbe de la classe n , et celles qui passent par un point de l'arête formeront un cône de l'ordre n' [Si l'arête est tangente α -tuple de l'enveloppe trouvée et génératrice α' -tuple du cône on aura $n - \alpha = n' - \alpha'$].

Cherchons à présent les droites communes à ~~cette~~ deux congruences (n, n') et (n_1, n'_1) indépendantes. A cause de l'indépendance on peut substituer à l'une et à l'autre la forme limite que nous venons de décrire rapportée à deux droites singulières ($x = 0, y = 0$) différentes. Supposons que ces deux droites se rencontrent. Alors on trouve sans difficulté les droites communes qui sont 1° les génératrices de rencontre de deux cônes, des ordres n' et n'_1 et à sommet commun, et 2° les tangentes communes à deux courbes des classes n, n_1 dans le même plan. On trouve ainsi votre formule

$$nn_1 + n'n'_1$$

7 Novembre. Je suis venu jusque là avant hier, et hier je n'avais pas le temps de continuer. La congruence particulière à laquelle j'ai réduit une congruence quelconque est moins simple que je n'avais voulu en commençant la description. Voici une autre réduction qui conduit à un cas particulier plus commode. Si l'on substitue ~~aux coordonnées~~ au point $x : y : z : u$ le point $\frac{x}{\varepsilon} : y : z : u$ où ε est infiniment petite (on construit alors une figure homologique à la figure donnée) on aura une congruence contenant n' fois toute droite du plan $x = 0$ et encore les droites par n points fixes du même plan. Alors on obtient immédiatement votre formule sans appliquer aucune transformation à l'autre congruence.

II. On peut se servir d'un procédé analogue pour étudier les systèmes de courbes (surfaces) ; je me bornerai ici à parler de coniques dans un plan fixe. Soient μ et ν les caractéristiques d'un système.

Nous substituerons à un système donné quelconque la figure homologique où tous les points du plan – à l'exception de ceux qui étaient infiniment près du centre d'homologie – sont transportés à se trouver sur l'axe d'homologie ($x : y : z = \frac{x'}{\varepsilon} : y' : z ; \varepsilon$ étant inf. pet.). Alors le système se décomposera en deux : l'un contient une série de coniques

infiniment aplaties, coïncidant avec une droite fixe, et ayant pour sommet deux points de cette droite dont les abscisses satisfont à une équation symétrique du degré ν en chaque abscisse ; l'autre contient des coniques composées de la même droite fixe et d'une droite passant par un de μ points fixes de la même droite. Les 2ν points qu'on obtient en égalant, pour la première série, les deux abscisses seront 1° les μ points fixes de la seconde série, 2° les ω centres des coniques à points doubles appartenant à la première série.

On trouve les coniques du premier système partiel qui satisfont à une nouvelle condition en combinant l'équation entre les deux abscisses à celle qui exprime, pour la conique dégénérée, la nouvelle condition. Le nombre qu'on trouve sera un multiple de ν , dont il faut soustraire, parfois, un nombre de solutions particulières étrangères au nombre de μ ou de $\bar{\omega} = 2\nu - \mu$.

La détermination d'une conique du second système partiel dépend de la détermination d'une droite par un des μ points. La condition donnée, étant indépendante de celles du système, fournit une courbe à laquelle la droite doit être tangente. Le nombre de coniques trouvés ainsi devient donc un multiple de μ .

[NB : En relisant la lettre je vois que vous pouvez très bien laisser la lecture de cette page]

[On voit que le nombre de coniques du système total sera bien de la forme $\alpha\mu + \beta\nu$ si – bien entendu – il est possible de le déterminer de la manière indiquée. Cela n'a pas lieu si la condition est satisfaite par toute conique infiniment aplatie, parce que la dégénération a effacé la différence entre celles et les autres coniques du système (si l'on ne distingue pas les infiniment petites de différents ordres, et alors il aurait été plus simple de garder le système donné où les quantités données inf. petites du 1^{er} ordre étaient finies.) On peut appliquer dans ce cas la méthode corrélatrice, à moins que en même temps la condition soit satisfaite par toutes les coniques à point double. On voit ainsi qu'il y a des cas* où la formule $\alpha\mu + \beta\nu$ n'est pas prouvée ici ; mais comme vous ne vous êtes pas arrêté à ce résultat très négatif, mais y avez ~~seulement~~ substitué, non seulement ~~celui~~ une démonstration du fait qu'il existe des cas où la formule est incomplète, mais aussi la détermination positive du supplément nécessaire, ~~cette~~ ma déduction actuelle de cas où la formule $\alpha\mu + \beta\nu$ est juste – il y aurait sans doute encore quelques précautions à avoir – n'a aucun intérêt.]

[(In a footnote :) ★ Il y en a peut être d'autres que celui que j'ai nommés]

Mais pour les déterminations pratiques je crois la méthode assez bonne, et en beaucoup de cas elle serait à préférer à l'application ordinaire du principe de correspondance.

On trouve par exemple sans aucune difficulté le nombre $\mu_1\nu + \nu_1\mu$ des couples de coniques de deux systèmes (μ, ν) et (μ_1, ν_1) qui ont entre elles des contacts doubles, en appliquant à l'un des deux systèmes la dégénération que je viens de décrire. Le même

procédé fournit le nombre $3(\mu\mu_1 + \nu\nu_1)$ de couples de coniques de deux systèmes qui ont entre elles des contacts du second ordre ; seulement la détermination du coefficient 3 demande une recherche particulière ou un emprunt à des recherches analogues. ~~De manière analog~~ Je vois, à présent, que ~~dans toutes les deux déterminations on peut substituer des courbes quelconques aux coniques (non pas, toutefois des droites)~~ le même procédé est applicable à la résolution des problèmes analogues pour des systèmes de courbes d'ordre quelconque. Soient m et m_1 les ordres, n et n_1 les classes des courbes, b et b_1 , c et c_1 les ordres des lieux des points doubles ou stationnaires des courbes du stationnaires, b' et b'_1 , c' et c'_1 les classes des enveloppes des tangentes stationnaires. Alors il existe

$$\begin{aligned} & \mu_1\nu(m_1-1)(n-1) + \mu\nu_1(m-1)(n_1-1) + \\ & \mu\mu_1(n-2)n_1 + 2\mu\mu_1(n_1-2) + \nu\nu_1(m-2)n + 2\nu\nu_1(m_1-2) \\ & + b_1\nu + b\nu_1 + b'_1\nu + b'\nu_1 \end{aligned}$$

de contacts doubles entre les courbes des deux systèmes, et

$$3\nu_1\nu + 3\mu\mu_1 + c_1\nu + c\nu_1 + c'_1\mu + c'\mu_1$$

contacts stationnaires. Je présente les termes dans l'ordre où ils se présentent à ma déduction. [*in a footnote :*] 4 cas d'exceptions : $m_1 = 1; n_1 = 1; m = 1; n = 1$

Vous voyez sans difficulté comment on peut multiplier ces exemples (nombre de points de contact de courbes de trois systèmes ; Fouret), et je dois mettre une fin à mes causeries. Je vois, en effet, qu'au lieu de vous donner des résultats prêts – dont je possède quelques uns que j'espère d'énoncer bientôt dans les Comptes Rendus (p.ex. sur le nombre de courbes d'un système de ∞^2 courbes qui ont des contacts doubles avec des courbes fixes) – j'ai profité de l'occasion pour éclairer et développer mes propres pensées que je voulais exposer. Est-ce abuser trop de votre patience ?

J'ai aussi parlé exclusivement sur mes propres recherches (qui auraient toutefois des rapports avec vos découvertes). Permettez moi donc d'ajouter sur vos derniers travaux – ou sur celui d'eux qui traite des caractéristiques – que je trouve vos exemples de la théorie que vous avez bien voulu me développer près du pont des arts et, à cause de ma lenteur, répéter dans une lettre, excellents pour éloigner le malentendu que vos nouvelles conditions ~~seraient pas élémentaires~~ seraient presque les seules qui demandent votre complément de la théorie de M. Chasles.

J'espère que votre famille se porte bien et je vous prie de faire mes compliments à Madame Halphen.

Votre très-dévoué H G Zeuthen

C.9 November 14th 1879

$\frac{14}{11}$ 1879

Mon cher ami

Je suis très fâché d'apprendre que la poste a dévoré et ma carte et ma brochure, qui sont expédiées à des époques différentes. Je me console en voyant qu'une liste des brochures expédiées consent avec ma conscience à confirmer mon innocence ; pour la carte je n'ai aucune liste ; mais je ne crois pas qu'il ne soit possible de vous avoir oublié.

Je trouverai encore un exemplaire de la brochure, qui traite des surfaces quartiques à une conique double, et qui contient notamment des recherches sur la réalité de leurs droites et [illegible] enveloppes de plans tangents doubles. Malheureusement elle est écrite en danois ! Vous n'aurez donc pour la connaître qu'un résumé très succinct dans le bulletin de Darboux. Un autre mémoire que je vous enverrai en même temps est, du moins, suivi d'un résumé en français.

Vous exprimez, un peu ironiquement – non pas envers moi mais envers l'objet de mon cours – le désir de venir suivre ce cours. Je vous réponds que vous n'y gagneriez pas grand chose. La plupart des résultats, ~~du moins~~ excepté ceux qui servent seulement d'exemples d'applications, vous sont bien connus (formules de Plücker, formules analogues dans l'espace, théorie des caractéristiques, y compris détermination des courbes qui satisfont à des conditions données avec l'indication expresse qu'il ne faut pas croire que leurs nombres aient toujours l'expression $\alpha\mu + \beta\nu$ etc*). Les méthodes sont celles dont on doit les germes à Poncelet, Steiner et Chasles (pour le principe de correspondance bien plus que la germe à Chasles) et qui sont développées et fertilisées et appliquées par MM. Jonquières et Cremona, puis MM. Halphen et Zeuthen et – je l'avoue – aussi par M. Schubert – bref les méthodes dont je me sers dans mes recherches personnelles. Vers la fin du cours je pense donner une esquisse ~~du~~ calc, illustrée par des exemples, du calcul symbolique introduit par vous, et dont le développement que M. Schubert ne semble peut-être trop exécuté pour ce que cette forme contient encore, mais qui a beaucoup de bon (exemple : formule VII de la page 32 dont la traduction géométrique est un théorème, facile à déduire, mais non pas immédiatement évident). [*In a footnote :*] * Le temps et les connaissances de mes élèves mettent certaines bornes à la délicatesse des matières dont je peux traiter.]

J'espère donc qu'en venant suivre mon cours vous trouverez les mots que je sers aux jeunes gens assez sains et du moins innocents, quant même vous n'y trouveriez pas trop de gout et de force.

Mais pourquoi, me demandez vous, leur donner ce nom que évidemment vous ne goûtez pas. C'est parce que je n'ai pas d'autre pour comprendre les méthodes que j'ai besoin de présenter en connexion, [illegible] et qu'elles m'ont été utiles pour le peu de chose que j'ai

pu faire moi-même, et que je les crois utiles pour atteindre beaucoup de plus. Je veux apprendre aux jeunes gens à ne faire pas de calculs inutiles pour pourvoir à un but où l'on peut parvenir sans calcul*. Prenons un exemple très élémentaire : cherchons le lieu de sommets d'angles de grandeur donnée circonscrits à une conique. Le lieu rencontre une tangente de la conique en quatre points, il est donc du quatrième ordre. Pour un point du plan passe un seul des lieux qui correspondent aux différentes valeurs de l'angle v , ces lieux forment donc un faisceau. Celui ci est déterminé par deux de ses courbes, par celles qui correspondent à $v = \frac{\pi}{2}$, et $v = 0$. On trouvera donc l'équation suivant (pour l'ellipse)¹

$$(x^2 + y^2 \circ a^2 b^2)^2 - \lambda(b^2 x^2 + a^2 y^2) = 0$$

[(In a footnote :) ★ Quant même il faut pour être exacte développer les principes de ces procédés sur base analytique.]

Un seul point du lieu suffit ensuite pour donner $\lambda = 4 \cot^2 v$. Le calcul évité ici est extrêmement simple, mais par des procédés analogues on évite souvent des calculs compliqués. Dans le cas actuel je ne regarde pas le procédé comme moins bon pour une conique parce qu'il n'est applicable qu'aux ~~tangentes d'une~~ coniques. En d'autres cas, il faut appliquer soit de nouvelles formes du même principe (principe de la conservation des nombres) soit un nouveau principe. J'applique ~~par~~ ordinairement le principe de correspondance aux cas où il s'agit de démêler les nombres des solutions coïncidentes et les ordres des infiniment petites – cas que vous regardez avec raison comme étant des plus difficiles et des plus intéressantes, mais qui ne sont nullement les seuls cas d'intérêt*.

[(In a footnote :) ★ Pour moi ils ont, non seulement l'intérêt résultat de ce qu'il faut les étudier pour appliquer les lois générales aux cas qui présentent les singularités mais aussi celui qui résulte de ce qu'il faut connaître l'influence des singularités pour faire des conclusions exactes du particulier – où les lois se présentent souvent d'elles-mêmes – au général (loi de la conservation des nombres).]

Si vous me faites l'objection que les méthodes de dénombrement sont dangereuses aux commençants à appliquer, je vous répondrai qu'alors précisément il faut leur apprendre à éviter les dangers.

Mais finissons cette apologie, presque superflue j'espère. J'ai encore quelques remarques à ajouter sur $\alpha\mu + \beta\nu$ et notre ami commun à Hambourg, ou plutôt, je n'ai pas ~~besoin de~~ beaucoup à ajouter ; car ~~à cet égard~~ je ne le comprends pas M. Schubert à cet égard. Après sa première publication de la démonstration de Hurwitz – qui a je crois des ressemblances notables avec votre ancienne démonstration – je lui ai écrit sur votre découverte. Sa réponse me faisait croire de l'avoir persuadé de la nécessité d'exclure dans une théorie complète les solutions $\frac{0}{0}$ provenant de la troisième dégénération, c'est à dire de

¹An exponent 2 is missing from the second term on the left-hand side.

l'inexactitude, prouvée par vous, de la formule $\alpha\mu + \beta\nu$. Et à présent il vient la démontrer de nouveau ! même sans s'entourer de réserves et de définitions précises qui serviraient à l'explication de ce qu'il semble regarder comme un fait, que la formule pourrait être en même temps vraie et fausse ; car je ~~suis persuadé~~ ne sais pas qu'il ~~ne~~ veut ~~pas~~ nier votre découverte. Sans doute il ne la comprend pas du tout ; au cas contraire il ne nommerait pas au même instant les objections insignifiantes de M. Saltel, et il aurait la prudence de chercher – grâce à l'aide qu'offre vos premiers exemples – la faute de sa démonstration, qui n'est pas extrêmement difficile à trouver*. Il me semble à cet égard très naïf ; mais tant pis pour lui ; vous n'y perdrez rien. Du reste je comprends très bien que vous souhaitez à cette occasion faire comprendre mieux votre découverte, quant même vous auriez le soin de faire usage de répétitions, qui est la mère de la science. Mais selon votre lettre j'attends vos communications ultérieures sur cette matière qui exciteront peut-être de nouvelles remarques de ma part ~~publique~~.

Votre très-dévoué

H G Zeuthen

[(*In a footnote :*) * Je crois même qu'on pourrait en déduire une nouvelle démonstration de votre théorie ; peut-être mais je ne m'occuperai pas de cette question avant de voir votre propre démonstration géométrique.]

C.10 November 22nd 1879

22 novembre 1879

Cher ami,

me voici dans une position pénible. Certainement j'avais tort en appelant M.Schubert notre ami commun ; mais moi je suis l'ami commun de vous et M.Schubert. Avant l'arrivée de votre lettre, M.Sch m'avait écrit une lettre sur les caractéristiques triangulaires, à laquelle je dois répondre, ce que je n'ai pas fait encore. Le plus facile serait de lui envoyer votre exemple aussi simple que clair pour démontrer l'inexactitude de sa formule ; mais je ne voulais pas le faire sans vous citer, et je ne dois pas lui raconter que M.Chasles vous a fait juge de son travail, et peut-être vous aimez aussi le mieux garder votre exemple et vos arguments à une autre occasion.

Heureusement vous me montrer la voie de me tirer d'affaire en me rappelant nos conversations à Paris l'année précédente. Je ~~pourrais~~ veux lui écrire que vous m'avez fait observer l'impossibilité de représenter par un nombre fini de caractéristiques ces formations géométriques, qui peuvent contenir des formes dégénérées dépendant de deux quantités

infiniment petites. En effet, une telle forme-limite jouera un rôle différent d'après la valeur différente du rapport des ordres des infiniment petites ~~et représente ainsi un~~. N'est pas un juste énoncé de votre argument ?

~~En même temps~~ Pendant que je ne devrais pas montrer à M.Schubert vos expressions sur lui, M. Schubert parle ~~de vous~~ dans ses lettres avec respect de vous et de vos découvertes. Quant au calcul symbolique il me semble qu'il ait dit aussi hautement dans ses écrits publiés qu'il développe une idée due à vous. Dans la lettre dont je viens de vous parler, il ajoute dans une note (avec quelques réservations insignifiantes sur la ~~façon~~ manière dont vous faites valoir la fausseté de $\alpha\mu + \beta\nu$):

“Ich bin jetzt von der Berechtigung der Modification des $\alpha\mu + \beta\nu$ durch Halphen überzeugt”

Voilà ce que – jusqu'à l'étude de la sixième partie de son livre – je croyais qu'il était depuis ~~2 ans~~ trois ans. Au cas contraire j'aurais publié alors les remarques que je lui avais envoyées dans une lettre, afin de lui donner l'occasion de répondre aussi publiquement. Heureusement pour moi je suis alors exempt de me mêler publiquement à cette affaire à une époque où je ne connaissais que le côté négatif de votre découverte. Vous me demandez si à présent je ne veux pas entrer dans cette discussion au lieu de vous. Pour le moment j'y refuse ; car quand même je vois le point faible de la démonstration de Hurwitz-Schubert je ne le vois que grâce à la connaissance de votre théorie. Si toutefois le litige durerait, je ne manquerais pas de saisir une occasion de me placer de votre côté – pour ce que vous avez dit jusqu'à présent, et peut-être avec quelque autre considération sur la valeur relative des théories antérieures de Jonquières et de Chasles – ; j'aurais besoin alors moi-même de rendre ma position claire, et, comme la plupart des mathématiciens n'étudient pas directement les questions mais se contentent d'un dénombrement des partisans des deux opinions, il serait peut-être utile aussi pour répandre la juste opinion. Cela sera superflu toutefois si M.Schubert reconnaît publiquement non seulement la “Berechtigung” comme il l'écrit dans sa lettre, mais aussi la “nécessité” de la modification de $\alpha\mu + \beta\nu$.

Mais j'ai peur de vous surcharger de mes lettres.

Votre très-dévoué
H G Zeuthen

C.11 November 23rd 1879

23 novbre 1879

Cher ami,

Si je veux conserver votre confiance, il faut que je me hâte de révoquer mes éloges hier de votre ~~démonstration~~ exemple qui ne me satisfait plus après quelques réflexions que j'aurais dû faire avant de vous répondre ~~d'autant plus que~~. Certainement ma seule objection ne vous ~~est~~ sera pas échappée : dans le cas actuel le résultat de M.Schubert $2m - \nu$ sera exact si l'on regarde aussi les $\nu_2 + \mu_3$ triangles à sommets coïncidents comme des solutions. Il s'agit donc seulement de montrer ici qu'en se servant des progrès de M.Schubert on doit savoir a priori que ces solutions feront partie du nombre trouvé, car alors on pourra les éloigner ou garder à son gré. La formule sera bonne (pour l'exemple actuel, bien entendu) si l'on connaît parfaitement le sens du nombre qu'elle donne. Alors, du moins elle ne trompe pas. Permettez-moi donc premièrement la remarque qu'il n'est pas permis de demander à la formule qu'en tous les cas elle donne seulement des triangles propres. Alors il faudrait dire aussi que la caractéristique μ d'un système de coniques est le nombre de coniques propres qui passent par un point qui ne se trouve pas sur une conique singulière etc. Vous ~~réponderez donc~~ me répliquerez peut-être que dans la formule $2m - \mu$, on a déjà éloigné certains triangles singuliers et qu'il sera inconséquent d'en garder autres. J'essaierai donc d'en montrer la différence, en indiquant deux différentes manières dont on peut poser la question aux formules de Schubert, et les deux réponses qu'elles vont donner.

1° On peut demander tous les triangles satisfaisant aux conditions données y compris tous ceux dont le sommet a coïncide avec b , où le lieu \mathcal{C} de a a un point μ -tuple. Alors la question si elle aura du sens ne peut être qu'un cas limite de la question plus générale où la courbe \mathcal{C} ne passe pas en b . On trouve alors immédiatement $(\varepsilon') = m$

$$(a') = (s') = (\gamma') = (\varepsilon') = m, (b') = (c') = (\alpha') = 0$$

$$X(\Sigma', \Sigma) = Am$$

où, pour votre condition Σ , A sera $= 2$.

2° Le système Σ' que nous venons de considérer contient une infinité de triangles dont le sommet a coïncide avec b ; car si l'on impose aux triangles du système cette condition, la direction du côté ab (γ) reste indéterminé. On peut donc décomposer le système Σ' en deux : celui ~~qui conti~~ où tous les points a coïncident avec b , et celui où cela n'a pas lieu en général, mais qui contient pourtant μ triangles singuliers où a coïncide avec b mais où les côtés γ sont entièrement déterminés comme tangentes à \mathcal{C} en b . Ces triangles sont aussi bien triangles du dernier système que tous les autres, et si un de ces triangles satisfait à une nouvelle condition Σ , on y aura une aussi bonne solution du $X(\Sigma', \Sigma)$ que toutes les autres*. [illegible] pour ce dernier système, qui est un système complet, qu'on a les

nombre indiqués par vous

$$(a') = (s') = m, (\gamma') = (\varepsilon') = m - \mu, (b') = (c') = (\alpha') = 0,$$

et pour votre système Σ on aura

$$X(\Sigma', \Sigma) = 2m - \mu.$$

Si bc est une des tangentes à la courbe \mathcal{C} au point b , ce nombre contient des triangles où a coïncide avec b . La formule n'indique pas et n'éloigne pas le nombre de ceux-ci, mais elle ne le prétend ni non plus.

[(In a footnote :) ★ Si l'on demanderait : combien de points d'intersections a une courbe donnée d'ordre m avec une courbe dont on ne connaît que l'ordre n , non-compris les intersections qui se trouvent en des points d'inflexion de la courbe donnée, la géométrie énumérative répondrait "Les courbes ont mn points d'intersections. Si l'on ne veut pas ceux qui se trouvent aux points d'inflexions, il faut dans tous les cas particuliers les chercher et les éloigner s'il y en a". De même il n'est pas permis de demander à la géométrie énumérative qu'elle éloigne a priori tout triangle dégénéré.]

Dans mon usage actuel de la formule de Schubert je me suis contenté à donner aux quantités dépendant de la condition Σ des valeurs convenables. Essayant ensuite de déterminer directement les quantités $(\varepsilon b^2 c^2)$ etc. Je vois qu'on trouve trouverait $X(\Sigma', \Sigma)$ égal, respectivement, à $\underline{4m}$ et à $\underline{4m - 2\mu}$. Il était aussi à prévoir qu'à cause des deux intersections coïncidentes de la conique Ω avec la droite bc^* la formule donnerait chaque solution deux fois - ce qui n'est pas une faute de la formule si l'on a des moyens de découvrir la multiplicité des solutions. Je communique toutefois ces dernières remarques, qui sont sans importance pour la question dont il s'agit, avec toute réserve afin de n'avoir pas besoin de venir faire des corrections après avoir y réfléchi plus qu'en ce moment.

[(In a footnote :) ★ ou plutôt à cause de deux coniques coïncidentes Ω qui passent, dans le cas actuel, par le point donné b et sont tangentes à bc .]

C'était après avoir fini ma lettre à M.Schubert, où j'exprimais mes regrets, pour son livre et pour ses lecteurs et pour ceux de son nouveau mémoire, de la manque d'une indication de la portée des formules de sa partie 6^{me} ($\alpha\mu + \beta\nu$ etc.) et de ses nouvelles formules, que j'ai commencé mes nouvelles réflexions sur votre exemple. Ces regrets restent encore les mêmes ; car rien n'est altéré aux objections, et une formule dont on ne connaît pas la portée - car ne nous connaissons celle de $\alpha\mu + \beta\nu$ par vos recherches, et comme M. Schubert la pourrait faire connaître à ses lecteurs - est fausse ou du moins dangereuse. Cependant je suis bien aise de n'avoir pas encore expédié la lettre à M.Schubert, de façon que j'y puis substituer une autre où je donne aux mêmes pensées des expressions

moins fortes que celles que me dictait la supposition que les formules n'étaient pas même applicables à un si simple exemple.

Votre très-dévoué HG Zeuthen

C.12 November 24th 1879

24 novembre.

Je viens d'écrire une nouvelle lettre à M. Schubert, au lieu de celle que j'ai retenue. Pensant que, dans les circonstances actuelles je devais lui communiquer le moins possible de ce que je dois à vos communications personnelles, je me suis contenté à ajouter aux remarques sur $\alpha\mu + \beta\nu$ – où je pouvais renvoyer à vos publications –, sur les parties suivantes de sa sixième section, que celles-ci ne m'inspiraient pas la confiance nécessaire pour en faire usage : “Je crois, en effet, ou bien je sais par des communications que M. Halphen m'a faites l'année précédente, que plusieurs de ces formules à la Bézout ne sont possibles qu'avec des limitations de la portée semblables à celles dont on a besoin pour la formule $\alpha\mu + \beta\nu$.” Je l'invite donc en particulier à entourer ses nouvelles formules sur les triangles de précautions semblables (en citant alors vos remarques analogues sur $\alpha\mu + \beta\nu$).

J'espère avoir évité ainsi toute indiscretion.

Votre
H Z

C.13 November 30th 1879

30 novembre 1879

Cher ami,

Peccavi. Même sans me rappeler vos définitions précises de solutions étrangères j'aurais dû voir qu'on peut poser la question ainsi que, dans le cas actuel, les triangles limites sont exclus et que le nombre des autres solutions doit avoir ainsi selon le théorème de Sch. la forme prétendue être applicable toujours. J'ai vu plus immédiatement la même chose pour vos exemples présentés à la Société Mathématique, que M. Sch. m'a communiqué avec sa réponse. La généralisation de la question pour laquelle il obtient les deux solutions de la question, annoncées par son théorème, est arbitraire, et par une autre généralisation il aurait pu accommoder le nombre de solutions à une autre forme générale (quelconque ?).

Selon moi, il dépend de la manière dont on formule la question si une solution est propre ou étrangère*. Il faut donc demander à une formule caractéristique (devant être indépendante de la formulation de la question), qu'elle exprime le nombre de solutions de toute question (du genre demandé) qu'on peut exprimer par une équation algébrique complète, aussi de celles où l'équation algébrique résulterait d'une décomposition d'une équation réductible. En effet, cette dernière circonstance, qui se montre par les opérations énumératives avec le degré des équations algébriques qui correspondent aux équations algébriques, peut dépendre de la voie choisie pour résoudre le problème.

[(In a footnote :) ★ Je ne proteste nullement à vos définitions précises et conséquentes, mais cherche seulement un caractère applicable partout dans la géométrie. Peut-être coïncide il avec le vôtre.]

Considérons pour illustrer ces remarques par un exemple, la fameuse expression $\alpha\mu + \beta\nu$. Selon l'usage ordinaire et légitime de la forme des théorèmes généraux dans la géométrie, on peut dire (voir pourtant la note à la page 5.)

Dans un système de coniques aux nombres (caractéristiques)⁽¹⁾ μ et ν , le nombre de coniques satisfaisant à une nouvelle condition indépendante est égal à

$$\mu \cdot \left(\frac{1}{2} \mu'^2 \nu'^2 - \mu'^4 \right) + \nu \left(\mu'^4 - \frac{1}{4} \mu'^2 \nu'^2 \right)$$

(notations de M.Schubert, ayant prêté le ~~premier~~ 3^{me} cahier du I vol du Bulletin (pour votre introduction) à un jeune homme je ne puis citer vous-même);

[(In a footnote :) (1) Les caractéristiques ne sont pas regardées ici comme caractéristiques propres, mais indiquant seulement les nombres de coniques passant par un point ou tangentes à une droite.]

car en définissant le système par μ et ν , on demande la solution, qui est générale lorsque ces nombres comprennent tout ce qui est donné sur le système. Alors le cas où il y a des singularités de la 3^{me} espèce est particulier, et on n'a pas besoin d'exclure ce cas de l'énoncé général, qui est applicable à tout cas particulier, à condition qu'on le regarde comme cas particulier, ou bien comme cas limite. Pour cette formulation ci les coniques de la 3^{me} espèce ne sont pas des solutions singulières, quand même elles seraient indépendantes des arbitraires de la condition. On a même ici une voie de trouver aussi le nombre des solutions qui seront les seules propres lorsqu'on considère isolément le cas qui était particulier ici à cause de la formulation générale de la question, mais qui n'est pas du tout plus particulier en lui-même [illegible] que ceux où on rencontrent les autres coniques singulières parce ~~qu'il dépend~~ que toutes les paires de coniques singulières de 3^{me} espèce dépendent aussi de quatre arbitraires ; aussi, bien entendu, l'énoncé de la formule, regardée comme générale ici, n'est qu'une partie mince de cette voie, la difficulté, que vous seul avez surmontée, se présentant ~~seulement~~ à l'exclusion des solutions qui deviennent

étrangères par la nouvelle formulation de la question.

On a aussi un théorème $\alpha\mu + \beta\nu$ logique (? voir p.5) ; mais alors il faut se rappeler qu'avec autant de raison on aurait un théorème $\alpha\mu$; car les systèmes définis par le seul nombre μ , ne contiennent pas en général des coniques aplaties. Dans une théorie des caractéristiques⁽²⁾, telle que vous l'entendez, et telle que M.Schubert dit qu'il faut l'entendre, ces deux théorèmes⁽³⁾ et les théorèmes de M.Schubert sur plus de 2 points d'une droite etc sont absolument faux. Pour en savoir quelque chose il lui faut donc les transformer en théorèmes énumératifs généraux ordinaires.

[(In footnotes :) (2) où les théorèmes énoncent que tel nombre a toujours une expression de telle forme (3) ayant alors un autre énoncé]

Peut-être que je ne fais que vous rendre des pensées que vous avez déjà émises. Vous remarquez pourtant la différence de votre note du 13 nov^{bre} 1876 : je n'excepte pas des énoncés généraux les cas particuliers où la solution générale se décompose, mais demande seulement qu'on les regarde – dans les applications du théorème général – comme cas-limites. Ici comme partout j'accentue que la différence du général et du particulier n'est pas absolu mais dépend de la formulation (l'énoncé) du théorème ou des problèmes, et que dire “en général” sans formuler exactement l'énoncé c'est rendre l'énoncé encore plus inclair.

Recevant l'appel de M.Schubert je pensais de nouveau à me prononcer publiquement afin de ne me trouver pas entre les deux adversaires ; mais en apprenant par votre lettre que votre débat avec M. Schubert paraît même “devoir être courtois” je crois qu'il vous vaut le mieux de vous passer de mon intervention. Votre bon droit et le bon sens, dont M.Schubert a donné des preuves en d'autres matières, va finir bientôt le combat – malgré la grande difficulté que M.Schubert aura à abandonner un chapitre d'un livre imprimé – et m'étant tû jusqu'à présent je ne dois pas me mettre à côté de vous au moment où vous allez gagner tout seul la victoire.

M. Schubert prononce dans sa lettre ses regrets de voir troublées ses relations amicales avec vous, et me prie de vous parler en sa faveur. Je le ferai en vous disant – entre nous – que ses pêchés contre vous, qui se dressent contre lui-même, et en particulier sa négligence totale de vos objections à $\alpha\mu + \beta\nu$ n'ont leur racine ni à malice ni à “mépris” mais à une naïveté et un défaut de tact qui n'est pas rare en Allemagne. J'ajoute que je lui ai répondu que la seule voie de vous réconcilier serait de reconnaître publiquement la justesse de vos recherches sur les caractéristiques ou de soutenir publiquement sa théorie des caractéristiques, en particulier sa le théorème de $\alpha\mu + \beta\nu$ contre les objections que vous aviez y opposées depuis long temps.

A propos de ce long temps je dois vous dire que je reconnais la patience dont vous vous louez dans votre lettre avant dernière ; elle n'a nui qu'à M.Schubert, ~~donnant~~ dont

les nouvelles méprises donnent du relief à vos découvertes sur ce terrain.

Votre très-dévoué

H G Zeuthen

En relisant cette lettre pour voir si je pourrais vous permettre de faire au besoin – mais vous n’en aurez pas besoin – l’usage qui bon vous semble de sa partie non-personnelle, je vois qu’alors il faudrait y ajouter une restriction. Le Un système de coniques est loin d’être défini par les deux nombres μ et ν d’une manière si claire que par le seul nombre μ ; en effet, celui-ci s’attache, non seulement par les systèmes unicursales, à une la représentation algébrique par des coordonnées ponctuelles, pendant qu’il pourrait rester douteux pour les systèmes (μ, ν) si la réduction de 2μ à ν et de 2ν à μ se fait en général par des coniques singulières des deux premières espèces. Le théorème $\alpha\mu$ avec la petite portée pratique est donc plus clair et logique que le théorème $\alpha\mu + \beta\nu$, dont la grande portée pratique a fait croire à son universalité. Celui-ci aura pour devenir juste besoin d’être entouré de plus de restrictions qui, sans excepter expressément les coniques singulières de 3e espèce, ce qui serait contraire au caractère que je veux attribuer à un théorème énumératif général (mais non pas caractéristique), fait des systèmes contenant ces singularités, des cas particuliers.

Néanmoins je ne transcris pas ma lettre parce que la nécessité le défaut dans mon exemple, étant signalé ici, montre plus clairement les demandes qu’on doit parvenir poser à une formule un théorème énumératif général pour qu’il soit applicable à tout cas particulier, et auxquelles j’essaie à observer dans mes propres énoncés de théorèmes ; en même temps il montre de nouveau combien vos corrections de la théorie ont été nécessaires ; mais le théorème $\alpha\mu$ reste le seul bon exemple dans cette lettre de théorèmes énumératifs généraux.

Vous faites des excuses de vos raies et de votre écriture. Et moi, qui vous présente “en général” des brouillons !

Votre

HZ

C.14 December 18th 1879

$\frac{18}{12}$ 79

Mon cher ami,

En lisant votre lettre j’étais près de croire que j’avais commis une faute analogue à celle de dire que les courbes gauches du quatrième ordre doivent être “en général” de la même classe, avoir le même nombre de points doubles apparents etc parce qu’elles sont des cas

particuliers de “la courbe générale du quatrième ordre”, être qui n’existe pas. Mais vous devez vous rappeler que je n’ai parlé que de coniques satisfaisant à une condition indépendante du système et vous me conviendrez que le nombre de points d’une courbe d’espace d’ordre m qui satisfont à une condition indépendante de la courbe est en général égal à Am . ~~Il est possible que~~ La courbe et la condition peuvent avoir à la fois des spécialités d’où résultent des décompositions en nombres d’autres formes, mais ces spécialités ~~résultent~~ cesseront, non seulement, comme vous me faites observer, par une généralisation de la condition, mais aussi par une généralisation de la courbe qu’on peut regarder comme cas particulier, non pas de la courbe générale d’ordre m , qui n’existe pas en singulier, mais de la courbe générale de l’espèce à laquelle elle appartient. En effet la division en espèces se rapporte à la définition ponctuelle de courbes.

Mon théorème “général” $\alpha\mu$ ne diffère pas du reste essentiellement du vôtre (qui est plus clair), ce qui se montre aussi à la circonstance, que aussi pour moi le théorème analogue sur les systèmes d’une infinité double cesserait d’être vrai parce qu’on y trouve en général des droites doubles (mot qui comprend la dégénérescence de la seconde espèce et toutes celles de la troisième espèce, qui sont de même réunies dans la formule ordinaire et toujours juste $\lambda = 2\mu - \nu$). J’ai seulement défendu moi-même ; mais j’avoue que votre lettre m’a rapporté une circonstance qu’on est trop disposé à oublier, et à laquelle je ne pensais pas expressément en écrivant ma dernière lettre.

Vous dites que votre théorème véritable est plus “général” que ces théorèmes énumératifs généraux. Je préférerais de dire que votre théorème est absolu, de façon qu’on n’y ait pas besoin de dire ou de sous-entendre “en général”, mots qui désignent qu’un théorème est aussi vrai dans tout cas particulier, à condition qu’on le regarde comme cas particulier. Pour le faire il faut définir ~~d’une manière~~ sans ambiguïté le cas général, ce que M. Schubert oublie, et ce qu’on oublie beaucoup de fois dans la géométrie. Vous voyez que j’attribue aux théorèmes généraux, dont on ne peut se passer partout, une valeur inférieure à celle des théorèmes absolus.

Mais je crois que sur ce point notre accord est si parfait, qu’il n’y a aucune raison d’y insister.

Quand même vous ne semblez pas insister à la circonstance que j’avais oublié votre solution antérieure du problème sur le contact du second ordre, je le regrette moi-même, et je voudrais que votre lettre aurait pu venir d’assez bonne heure pour me permettre d’ajouter une note relative au fait que vous me rappelez, à ma seconde communication. A présent, je serai borné à saisir la première occasion pour mentionner vos deux déductions du même résultat, ce qui intéressera d’autant plus que ~~les applications~~ chacune de vos déductions s’appliquent à des problèmes qui ne se résolvent pas ~~immédiatement~~ par la mienne. Je ne crois pas qu’il serait difficile d’étendre celle-ci à des courbes à singularités

quelconques ; mais elle sera inapplicable, immédiatement du moins, aux contacts d'une courbe fixe avec un système d'une infinité triple, parce que ce système contiendra un certain nombre de courbes dont la droite, limite de la courbe fixe, fait partie (on peut définir le système de manière que ce nombre devienne "en général" égal à zéro, mais sans définition expresse il n'est pas permis de négliger la possibilité de ces courbes du système.) Des circonstances analogues vous empêcheront peut-être (?) d'appliquer les résultats que vous trouverez par vos procédés à des courbes dont les ordres sont au dessous de certaines limites, ce qui est sans importance. Je ne prévois pas a priori d'autres difficultés – je ne parle pas des grandes difficultés pratiques que peut causer la réalisation détaillée - ; ~~mais~~ cependant je n'ai pas discuté si le nombre de coïncidences inconnues différentes ne dépassera jamais celui des équations qu'on obtient par le principe. Dans tous les cas, je vous remercie de m'avoir rappelé ces recherches intéressantes, que j'espère de n'oublier pas après d'y être revenu de la manière actuelle.

Votre énoncé complet de mon extension du théorème du genre, qui exempte de toute question difficile sur solutions multiples, et qui marque ainsi un progrès très stable au delà de ma formule, avait(?) été du reste dans mes mains avant l'arrivée de votre lettre. J'en ai fait usage dans mes leçons ; mais ayant choisi d'avance mes applications, j'ai négligé de consulter les vôtres. J'applique la formule à l'étude énumérative d'un lieu de points ou d'une enveloppe de droites. Dans le premier cas on aura à déterminer directement l'ordre n de la courbe et les degrés de multiplicité des systèmes* circulaires d'un ordre n supérieur à l'unité. Ma formule, rendue plus facile à appliquer par vous, sert à déterminer le genre, et ensuite l'expression du genre et les formules de Plücker feront le reste. En y représentant les singularités connues d'avance par les équivalents, qui les remplacent ici, on obtient les autres par des soustractions.

[(*In a footnote :*) Grâce à vos théorèmes il ne sera jamais douteux quelles branches appartiennent au même système ~~eirculaire eyelique~~...]

L'exemple que j'ai donné avec le plus de détail c'est l'étude de la développée, où l'on emploie toutefois seulement le théorème simple du genre. Je n'ai fait ici que retrouver les résultats que vous avez donnés dans votre mémoire sur les points singuliers, ~~mais~~ que je suis bien aise d'avoir eu ainsi une occasion de connaître mieux qu'à la rapide lecture, dont les meilleurs mémoires sont sujets lorsqu'ils me trouvent occupé d'autres travaux ; je l'avoue, et j'ai déjà parlé ici de la conséquence de cette rapide première lecture d'un autre de vos travaux.

Mais il est temps de finir ; je crois que c'est mon âge avancé (au delà des 40 ans) qui me fait bavard, et qui me porte à dire beaucoup de choses que vous saurez mieux que moi (~~sur les limites de l'emploi des équivalents Plückeriens~~), seulement pour vous montrer que je les sais aussi moi. Dans tous les cas je reste

votre très dévoué

HG Zeuthen

C.15 August 19th 1886

Copenhague le 19 août 1886

Mon cher ami.

A mon retour de la campagne je viens de trouver votre premier volume sur les fonctions elliptiques. Curieux de voir la voie que vous choisissiez pour introduire ces fonctions et pour en fonder les principales propriétés, et ayant besoin non seulement de connaissances que m'ouvriront les parties suivantes mais aussi d'une bonne répétition des éléments, j'en ai commencé aujourd'hui la lecture par l'étude du premier chapitre. Les moyens que vous y employez, ainsi que la manière dont vous les employez, sont d'une simplicité et facilité qui correspond entièrement au caractère élémentaire que vous promettez pour cette partie de votre travail. Je ne doute pas qu'aussi vos autres promesses seront tenues, non seulement celles de la préface, mais aussi celle que contient le nom de l'auteur.

Pour m'inspirer confiance ce nom n'a nullement besoin de l'addition de "Membre de l'Institut"; mais néanmoins je suis très-content d'avoir appris votre nomination, il y a quelque temps. Vous savez quelles circonstances m'ont empêché de l'observer immédiatement et de penser à vous en féliciter ; mais je vous prie d'agréer à présent mes félicitations retardées.

Je pensais du reste à vous écrire, il y a 1-2 mois, en lisant un article de M. Study dans les *Mathematische Annalen* sur $\alpha\mu + \beta\nu$. Il reconnaît la justesse de vos arguments contre cette formule, à la condition que vous adoptiez les définitions des coniques singulières d'un système de Clebsch. Il y substitue une autre qui selon lui rendrait la formule juste. Cette autre définition consiste essentiellement à demander que les courbes dégénérées soient des limites de coniques satisfaisant aux conditions données. Sa considération de ces courbes ne diffère donc pas de celle dont nous auteurs antérieurs à ~~Clebsch~~ – qu'il traite d'assez naïfs – nous sommes servi. Il se rend donc aussi coupable de la même faute que tout le monde avant vous, et n'a pas compris que le fait qu'aussi la troisième classe de coniques dégénérées peuvent dépendre de quatre conditions.

Je voudrais que vous auriez assez de Temps et de patience pour lire le mémoire dont il s'agit et pour envoyer quelques lignes à M. Klein ; mais j'ai peur qu'après avoir tué le théorème vous dédaigniez d'en combattre le spectre.

J'ai fort peu à dire de moi-même. Après une perte cruelle, suivie d'une pneumonie – dont je suis du reste parfaitement restitué – je n'ai pu encore concentrer mes forces sur un nouveau Travail scientifique devant remplacement mes recherches sur les coniques

de l'antiquité. Je ne suis donc pas mécontent de ce que ma vacance finit le premier septembre : c'est plus facile d'avoir un travail prescrit que de s'en donner, et les leçons laisseront assez de Temps pour les petites recherches où se trouvent les germes d'études plus sérieuses.

Mes renseignements sur moi-même seraient incomplets si je n'ajoutais pas que mon garçon se trouve très-bien.

J'espère que vous-même et votre famille vous portez bien.

Votre très-dévoué

H G Zeuthen

Je pense que l'éditeur vous a envoyé de ma part le premier cahier de l'édition allemande de mes "coniques de l'antiquité", et que dans peu de jours vous en recevez le second cahier.

Je vous remercie de votre réponse amicale à mon avis dans cet hiver. J'ai aussi à vous remercier de plusieurs envoies scientifiques.

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