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and the making of macroeconometric modeling:  
1939-1959**

Erich Pinzón Fuchs

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UNIVERSITÉ PARIS I PANTHÉON-SORBONNE

# THÈSE

en vue d'obtenir le grade de Docteur  
en « Sciences Économiques »

présentée par

Erich Pinzón-Fuchs

## ECONOMICS AS A “TOOLED” DISCIPLINE: LAWRENCE R. KLEIN AND THE MAKING OF MACROECONOMETRIC MODELING, 1939-1959

Directeur de recherche :

Annie L. Cot

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Richard ARENA	Université Nice Sophia-Antipolis (président)
Michel ARMATTE	Université Paris-Dauphine
Marcel BOUMANS	Utrecht University
Annie L. COT	Université Paris 1 Panthéon-Sorbonne
Ariane DUPONT-KIEFFER	Université Paris 1 Panthéon-Sorbonne
Pedro GARCIA DUARTE	University of São Paulo (rapporteur)
Kevin D. HOOVER	Duke University (rapporteur)



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# Economics as a “tooled” discipline

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Lawrence R. Klein and the making of  
macroeconomic modeling, 1939-1959

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*Este es el hombre moderno. Conoce las fuerzas que gobiernan el mundo, las tiene a su servicio, es el dios de la tierra: es el diablo. Su lema es: todo puede hacerse. Sus armas son el oro y la inteligencia. Su procedimiento es el cálculo.*

*Ernesto Sábato, 1951*

*Boileau said that Kings, Gods, and Heroes only were fit subjects for literature. The writer can only write about what he admires. Present day kings aren't very inspiring, the gods are on vacation, and about the only heroes left are the scientists and the poor... And since our race admires gallantry, the writer will deal with it where he finds it.*

*John Steinbeck, 1939*





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# Economics as a “tooled” discipline





# *Preface and Acknowledgements*

Large-scale macroeconometric models have played a paramount role in the transformation of US-American macroeconomics in the political, academic, and intellectual spheres since the 1940s. On the one hand, in an era of progressive liberalism that pursued economic stability through the advocacy of government intervention, macroeconometric models provided powerful tools for economic planning and forecasting.<sup>1</sup> On the other hand, macroeconometric models also changed the way macroeconomic knowledge was produced, emphasizing empirical orientation and technical sophistication in the context of a growing computerization and fundamental transformation of scientific practices, which were increasingly relying on teamwork effort and on a new kind of expertise. The models by themselves, however, did not change the way macroeconomic knowledge was produced; instead, parallel to the models, a new scientific practice was also created that framed the way in which these models were built, maintained, understood, and used. This new scientific practice was macroeconometric modeling.

In this dissertation, I place macroeconometric modeling at the center of the history of twentieth century macroeconomics, i.e. as a history of *macroeconometrics*, and argue that the

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<sup>1</sup> David Plotke (1996) refers to the political order of the 1930s and 1960s as “progressive liberalism,” first because this term “emphasizes the basic liberal commitments of Democratic discourses. Second, [because] ‘progressive’ suggests the combination of modernizing and democratic themes that distinguished the Democratic reworking of American liberalism. It points to the Progressive movement two decades earlier, with its rationalizing drive, and to the subsequent use of that term to indicate a concern for democratic reform. Third, ‘progressive liberalism’ is not a familiar term, though its two words are – linking these words is meant to suggest the active process through which diverse thematic elements were integrated in building the Democratic order. Fourth, both parts of this term are mainly political in their referents, and direct attention to the crucially political character of Democratic efforts.” (163). Note that the notion “political order” can be related to the notion of *régime* developed by Alain Desrosières (2003).

work of Lawrence R. Klein was pioneering and decisive in the construction and consolidation of this new practice. The decades between 1939 and 1959 represent both the formative years of Klein, and those of a whole institutional design and configuration, which led to the establishment of macroeconomic modeling as the dominant practice in macroeconomics during a large part of the second half of the twentieth century.

The advent of this new scientific practice raises at least two central questions: (1) What exactly were the forces and the objectives behind the development of this new practice of macroeconomic modeling, and what kind of tools and institutions did macroeconomists build to observe, understand, and control the postwar economy? (2) What were the effects that the construction and use of these tools had on the production of macroeconomic knowledge? Answering these two central questions is my main motivation in this dissertation, which proposes a history of early macroeconomics that puts macroeconomic modeling at its center.

The kind of history I propose here can be read partially in three different ways. First, as a history of a discipline that responded to a political necessity to manage the economy in the specific historical context of the US-American postwar period. This response consisted on the adoption, adaptation, invention, and construction of new tools and techniques, which derived in new ways of providing scientific expertise, and of understanding the world; in a word, economists' response to the postwar period resulted in the development of specific tools that were embedded within specific institutions, and that allowed them to get closer to political and economic powers through a new scientific practice. Consequently, I specifically address the following questions: What did macroeconomic modelers seek to achieve with their new tools? And what were they actually able to achieve during these formative and enthusiastic years? Second, my dissertation can be also read as a historical account of the conditions that allowed macroeconomic modeling to rise. To this purpose, I examine the state of the

economics discipline, and of statistical and mathematical knowledge, the scientific institutional configuration, the financial support available, the political demand, and the type of scientists that made possible the construction and adoption of this new practice. A third reading of my dissertation consists on an account of Klein’s life-story, a path-breaking figure who was convinced of the necessity, utility, and power of using macroeconomic models as tools for economic planning and for social reform, and who forged this new macroeconomic modeling practice almost from scratch.

Taking Klein as a focal point and as a vehicle, I travel across the economics discipline of the 1940s and 1950s, and study the intersection between the history of macroeconomics and the history of econometrics, providing a new understanding of postwar economics as a “tooled” discipline, in which theory (economic and statistical), application, expertise, and policy become embedded within one scientific tool: a macroeconomic model. Indeed, this new understanding presents the history of macroeconomics not as the product of monolithic ideological and purely theoretical issues, but rather of divergent epistemological views and modeling strategies that go back to the debates between the US-Walrasian and US-Marshallian approaches to empirical macroeconomics, in which macroeconomic modeling forms the heart of macroeconomics.

My thesis is that Lawrence R. Klein was the most important figure in the creation of a new way of producing scientific knowledge that consisted in the construction and use of complex tools (macroeconomic models) within specific institutional configurations (econometric laboratories) and for explicit policy objectives, in which the roles of experts (scientific teams) were embodied within a new scientific practice (macroeconomic modeling).

\* \* \*

## *Preface and Acknowledgements*

Before I start, I would like to thank many important people who contributed to this project and who supported me in various ways during these years. I truly feel that this dissertation cannot be considered only the product of an individual's work, but the result of a teamwork effort constructed through direct and indirect collaboration, and inspired by the questions and themes that our academic community considers relevant in some way. This dissertation has grown from formal and informal discussions, spontaneous suggestions, detailed comments, and fascinating conversations I entertained with many people during these years (some of whom, I apologize, I might have forgotten to mention). If some of the following pages are original, this is only the product of these conversations, and it is my interlocutors who should be credited for this originality. The errors and inconsistencies of this work, however, are my own responsibility.

I would like to start by thanking my supervisor Annie L. Cot for her superlative support; for her capacity to ask relevant questions that turn scientific themes and objects upside-down, showing original aspects of old (and new) problems in a fresh (postmodern) perspective; for providing me with the liberty necessary to explore the subjects of my predilection; and for building, together with Jérôme Lallement (whom I also thank heartily), one of the best laboratories and “families” to do history of recent economics: the group *Recherche en Épistémologie et en Histoire de la Pensée Économique Récente (REhPERE)* at Université Paris 1 Panthéon-Sorbonne. Special thanks are also due to that amazing group of people and friends that form both *REHPeRE* and the Albert O. Hirschman doctoral seminar: Agnès Gramain, Judith Favereau, Matthieu Renault, Pierrick Dechaux, Niels Boissonnet, Cléo Chassonnery-Zaïgouche, Maxime Desmarais-Tremblay, Francesco Sergi, Aurélien Goutsmedt, Jean-Sébastien Lenfant, Juan Camilo Melo, Quentin Couix, Cristian Frasser, Thomas Delcey, Baxter Jephcott, Tom Juille, Dorian Julien, Raphaël Fèvre, James Johnston, Nicolas Brisset, José Edwards, Lauren Larrouy, Adrien Audinet, and Youssef Souidi.

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*A todos ustedes, ¡muchas gracias!*





# Introduction: Moving macroeconomic modeling to the center of the history of macroeconomics

As economics becomes the study of objective behavior, this breach between theory and the ‘practical’ subjects will be narrowed.

*Wesley C. Mitchell, 1925*

Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world.

*John M. Keynes, 1938*

When schools evolve and paradigms are born and die, it is forced upon you that *what ultimately shapes the verdicts of the scientist juries is an empirical reality out there.*

*Paul A. Samuelson, 1992*

### 1.1. Building a democratic progressive order in the US: the postwar need for macroeconomic planning

“Many people dread to think of what is coming. Businessmen, wage-earners, white-collar employees, professional people, farmers – all alike expect and fear a post-war collapse. Demobilization of armies, shut-downs in defense industries, unemployment, deflation, bankruptcy, hard times” (Hansen 1942, 1). This was the climate during the early 1940s in the United States when even the possibility of a military victory over the European totalitarian regimes was insufficient to overthrow the generalized pessimistic belief that the economy would fall into a slump similar to that of 1929. Indeed, the social disintegration and unrest, the mass unemployment, and the economic frustration left by the Great Depression, probably the most severe contraction in the whole of US history, remained vivid in the minds of US-Americans

even after World War II. Yet, there were good reasons to believe that the United States had learned from its past, and that it possessed the capacity to avoid the return of a new economic depression and of its consequent social distress.

In fact, in his 1947 economic report to the US Congress President Harry S. Truman was optimistic about the determination of US-Americans “to see to it that America is not ravaged by recurring depressions and long periods of unemployment, but that instead we build an economy so fruitful, so dynamic, so progressive that each citizen can count upon opportunity and security for himself and his family.” Truman vehemently rejected “the notion that we must have another depression,” proclaiming that “this need not happen again, and *must* not happen again” (vii).<sup>2</sup>

During the difficult years following the Great Depression and World War II, economists developed a sense of duty towards their country oriented to the prevention of a new collapse of the economy. For the most part, economists understood the idea and partook of the opinion that “if we do not plan for and try to build the ‘right’ kind of postwar world, the winning of the war will be of little avail and we should have not won the peace” (Harris 1943, 1). In short, independently of the military result after the war, if the catastrophic economic experience of the thirties was to be repeated, both capitalism and democracy would be doomed. Peace would not be achieved through military and political victory alone: history had demonstrated that “economic victory” was also essential to maintain the fragile social and democratic order of any country, including the United States. Avoiding a new economic collapse was considered a democratic necessity, which, together with the intervention programs put forward during the New Deal, created a strong consciousness and political will, sustaining that the state must have some agency over the economy, and that mechanisms of government intervention should be

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<sup>2</sup> The Economic Report of the President, transmitted to the Congress, January 8, 1947. Emphasis in the original.

established.<sup>3</sup>

An example of these mechanisms was the Employment Act of 1946, which declared it to be “the continuing policy and responsibility of the federal government to use all practicable means [...] to promote maximum employment, production, and purchasing power.”<sup>4</sup> But political will alone was not sufficient to achieve the maximum level of performance of the economy. Concrete actions of economic policy had to be undertaken in a reasoned, effective, and scientific way. Given the international political context and the rise of totalitarian regimes at both extremes of the political spectrum, however, US-Americans feared that the difference between democratic intervention and totalitarian rule at home could be transgressed too easily. And so, both economists and politicians made great efforts to demonstrate that “there [was] nothing anti-democratic in an active national antidepression policy,” and that, on the contrary, the promotion of sound intervention was the most democratic way to go (Marschak 1941, 370-371).

This had been, in fact, the sense in which President Franklin D. Roosevelt and other New Deal figures had thought of their political and economic systems. “Democratic leaders liked to consider themselves as realistic and flexible, as nonformalist regarding institutional and legal arrangements, and as open to revising policies in the light of practical experience” (Plotke 1996, 164). Roosevelt’s rejection of the “platonic distinction between ‘capitalism’ and ‘socialism’ [...] led the way toward a new society which took elements from each and rendered both obsolescent. It was his freedom of dogma which outraged the angry, logical men who saw everything with dazzling certitude” (Schlesinger 1960, 651). At one side of the spectrum, former President Herbert Hoover alleged that Roosevelt’s illusion was “that any economic system

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<sup>3</sup> The Democratic political order implemented in the 1930s and 1940s sought to fuse “democratic and modernizing themes in a progressive liberalism that advocated government action to achieve economic stability, enhance social security, and expand political representation” (Plotke 1996, 1).

<sup>4</sup> Public Law 304 – 79<sup>th</sup> Congress S. 380: Employment Act of 1946, 1. For a contemporary history of the Employment Act of 1946 see Bailey (1950). See also Santoni (1986).

would work in a mixture of others,” and that “no greater illusions ever mesmerized the American people.” At the other side of the spectrum, Roosevelt’s “freedom of dogma” produced rejection as well, leading Leon Trotsky to say with contempt that in abhorring “systems” and “generalities,” Roosevelt had developed a “philosophic method [...] even more antiquated than [the US] economic system” (quoted in Schlesinger 1960, 651).

But Roosevelt “resisted ideological commitment. His determination was to keep options open within the general frame of a humanized democracy; and his belief was that the very diversity of systems strengthened the basis for freedom” (651).<sup>5</sup> This “middle-of-the-road” alternative, made part of the determination of New Deal economists too, which coexisted in a context of “interwar pluralism” represented not only in the dissimilar voices among different economists, but also in the kind of “inner pluralism” of ideas, methods and practices promoted by each economist (Morgan and Rutherford 1998, 5). Although no consensus over a theoretical or methodological agenda existed, economists converged “around a particular view of science and a conviction of the inadequacy of the unregulated market” (2). In addition, economic planning was considered a technical tool and a “politically neutral concept [to] pursue and achieve economic and political democracy simultaneously” (Balsciano 1998, 156).

Consequently, the 1940s inherited important tools of intervention developed during the 1930s as a result of the New Deal, which set the ground for interventionist ideas and for the establishment of “a greatly expanded public sector [as a] fact of life” (Sweezy 1972, 123). This public sector had been justified in a systematic theoretical way both by the work of John Maynard Keynes (1936) and of many “New Dealers” after whom political intervention in a market economy was no longer taboo.<sup>6</sup> Macroeconomic planning, which had survived the test

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<sup>5</sup> Schlesinger’s (1960) characterization of Roosevelt’s attitude might be overoptimistic. Yet, the point here is to illustrate the kind of intellectual spirit that reigned in the United States during the years of the New Deal and World War II, not only in the mind of the President, but also in some progressive academic sectors of the society, including that of some economists.

<sup>6</sup> New Deal economists like Leon H. Keyserling (1972), however, remembered that they had “been unable to discover much reasonable evidence that the New Deal would have been greatly different if he

of economic and political feasibility after the 1930s, stood up as the most suitable way both to manage the economy and to avoid economic collapse (Balisciano 1998, 156).<sup>7</sup> Yet, as expressed by Leon H. Keyserling (1972), it was economic practitioners (in governmental institutions) and Congressmen rather than academic economists, who were to be held responsible for the “major elements in the enduring achievements of the New Deal.” And so, “academic economists inside or outside of government” could not “claim much creative influence in [their] connections” to the achievements (or failures) of the New Deal (135).<sup>8</sup>

Despite Keyserling’s recollections, however, US American economists, saw themselves as scientists with professional skills and techniques that should be used to solve practical problems (Fourcade 2009, 8). This self-image, together with the sense of duty to serve their country, moved US economists towards the development of new scientific methods of macroeconomic planning and intervention during the 1940s.<sup>9</sup> Indeed, these years “witnessed [the] emergence

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[Keynes] had never lived” (135). This goes well in line also with Klein’s (1947a) definition of what he thought had been the *Keynesian Revolution*: “The Keynesian theory is viewed in the following pages as a revolutionary doctrine in the sense that it produces theoretical results entirely different from the body of economic thought existing at the time of its development. The ‘Revolution’ discussed here is a revolution in thought, not in the economic policies of governments” (vii). On the anticipations in terms of economic policy by the institutionalist movement see also Malcolm Rutherford (2011).

<sup>7</sup> Apart from macroeconomic planning, Márcia L. Balsciano (1998) identifies three other types of economic planning during the 1930s: social management planning, technical-industrial planning, and business economy planning.

<sup>8</sup> Leon H. Keyserling (1909-1987) was a very influential economist during the 1930s and 1940s, not only because of his close relation to practical politicians like Senator Robert F. Wagner of New York or President Franklin D. Roosevelt, but also of his affiliation to important economic institutions in agricultural and housing agencies. Keyserling obtained a BA in economics from Columbia University in 1928, and a law degree from Harvard in 1933. He never finished his Ph.D., although he worked on it at Columbia “absorbing the liberal economics tradition for several years under Rexford Guy Tugwell,” the prominent New Deal advisor at Columbia (Brazelton 1997, 189). He influenced as well the draft of the Employment Act of 1946, which created the Council of Economic Advisors (CEA) established in 1947. The CEA can be considered one of the institutions through which the economics profession has exerted important influence in matters of economic policy. Keyserling became the second chair of the CEA in 1950, appointed by President Truman. He is also remembered as the economist providing the “economic philosophy” of the Truman administration, and as the intellectual of the Monday Night Group, and as a “respected but refreshingly controversial economist” (190). The Monday Night Group was composed by Truman’s inner circle of advisors, and met every Monday in the Wardman Park Hotel (190). For a more detailed account of Keyserling’s career see Brazelton (1997) and Saulnier (1988).

<sup>9</sup> The standard narrative in the history of economics sustains that the move towards a more mathematically and statistically sophisticated economics was, in part, the product of the economists’ strategy to hide their political ideologies and to counteract the attacks from right-wing movements such as McCarthyism (see Goodwin 1998; Morgan 2003). Yet, the idea of producing tools that would allow

[of macroeconomics] as a technique of government (symbolized by the twin innovations of national accounting and macroeconomic modeling) and, more generally, as a tool for the exercise of public expertise” (2).

### 1.2. Finding a reasoned and rigorous way to control the economy

In this context of fear for a postwar slump, economists embarked in various projects to produce the kind of economic tools that would contribute both to the understanding of the economy and to the process of decision making. One of the most important examples was Jacob Marschak’s ambitious econometric project at the Cowles Commission for Research in Economics.<sup>10</sup> According to Marschak, the United States needed a fresher approach to Tinbergen’s macroeconomic model (Klein and Mariano 1987, 412).<sup>11</sup> This was, at least,

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for economic planning was not new and was rather related with the self-image that US-American economists had, as practically-oriented and problem-solving scientists (Fourcade 2009). To be sure, however, the Cold War had important effects on the kind of tools that economists adopted, but, as Weintraub (2016) reminds us, the Cold War and McCarthyism were not the same historical episodes.

<sup>10</sup> The Cowles Commission for Economic Research was founded by Alfred H. Cowles (1891-1984) in Colorado Springs in 1932. Cowles, who was raised in a family that owned various newspapers in the United States, was a business man and stock market journalist who regularly published a newsletter analyzing the state of the stock market and recommending the public to buy or sell different stocks. Yet, after the 1929 crash, Cowles started to feel that neither his or any other’s forecasts were accurate, and that forecasters were just guessing. At that point, Cowles wanted to use his resources to improve the understanding of the economy, and with the help of H. T. Davis, created a computing laboratory in Colorado Springs. With the move to Chicago in 1939, and with the recruitment of important economists, the Cowles Commission became a very influential institution in the economics profession, which was reinforced in 1955 with its move to Yale and the renaming of the Cowles Foundation. For a detailed history of the origins of the Cowles Commission see Grier (2005). See Christ (1956; 1994), and Hildreth (1985) for a discussion of the influence of the Cowles Commission in economics.

<sup>11</sup> Jakob Marschak (Jacob since 1933) was born in Kiev in 1898. His fascinating life both in terms of his political and scientific influences can be understood only considering the convoluted twentieth century (Hagemann 1997, 219). His life was marked by three important emigrations, first from Ukraine to Germany in 1919, then to England in 1933, and finally to the United States in 1939. In his early years in Ukraine, Marschak was very engaged Russian Revolution, ending up as Minister of labor in a regional government in the Caucasus region at the age of 19, and as a member of the Mensheviks faction of the Russian socialist movement. After spending some months in prison, Marschak arrived to Berlin in 1919 where he started his economics studies under Ladislaus von Bortkiewicz who taught him the meaning of mathematical and statistical methods for economic analysis (228). Briefly after that, he moved to Heidelberg where he continued his studies under the influence of Emil Lederer, Alfred Weber, and Karl Jaspers, who count as some of the most important figures responsible for the progressive spirit that reigned at the *Institut für Sozial- und Staatswissenschaften*, at *Universität Heidelberg*. In 1928, he moved to Kiel to the *Institut für Weltwirtschaft*, which had been recently created by Adolf Löwe, where he embarked in an innovative empirical project of economic analysis. There, he also met people

what he said to the young Lawrence R. Klein when they first met at the annual meeting of the Econometric Society in Cleveland, in September of 1944 (see chapter 2). Marschak, who was research director of Cowles at the time, was referring, of course, to the macroeconomic model of the US economy that Jan Tinbergen (1939) had developed for the League of Nations in the second half of the 1930s. Like in his former Dutch macrodynamic model, Tinbergen’s objective was to describe and explain the past behavior of the US economy, and to provide a method to judge the effects that different policies might have on the economy.<sup>12</sup> Marschak assembled “an unusual group of independent scholars but wanted to guide all together in teamwork” to develop a tool that “would permit state intervention and guidance for policy” (Klein 1991a, 109-112). With the institutional support of the Cowles Commission, Marschak thought that he would be able to reach that goal in just a few years. In the end, however, more than a few years were necessary to build not only the models but most importantly a complex scientific practice around these models: *macroeconomic modeling*. Macroeconomic modeling consisted not only in the construction of large-scale macroeconomic models to pursue policy and scientific objectives to intervene and understand the economy; it also represented a new way of producing knowledge through the intervention of organized and large teams of experts, who

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like Gerhard Colm, Hans Neisser, Fritz Burchardt, and Wassily Leontief. After being discharged from his position in Kiel because of his non-Arian descent in 1933, he moved to Oxford in 1935, where he became the director of the newly created *Oxford Institute for Statistics*, funded by the Rockefeller foundation. Finally, in 1939, Marschak emigrated to the United States. There too, he made part of several important institutions: first, he became chair of economics at the New School for Social Research in New York replacing Colm from 1939 to 1942; then, he moved to Chicago as research director of the Cowles Commission from 1942 to 1947 and as professor of economics at the University of Chicago. Under Marschak’s directorship, the Commission developed the ambitious econometric program that defined Klein’s image of econometrics, and where he picked up important concepts about economic planning, teamwork, and statistical analysis. Marschak stayed as a research associate at Cowles until 1960, following the Commission in its move to Yale in 1955, under James Tobin’s directorship. In 1960, however, Marschak made a final move to the University of California in Los Angeles. He died suddenly from a heart attack in July 1977, acting as President elect of the American Economic Association (AEA). For a detailed account of Marschak’s fascinating life see Hagemann (1997).

<sup>12</sup> One of the objectifs set by the League of Nations on Tinbergen’s report was that it should be concerned with the testing of the hypotheses and theories that had been collected by Gottfried Haberler (1937). For an account of Tinbergen’s work see Morgan (1990, chapter 4). See also Boumans (1999) for an account of Tinbergen’s modeling strategy.



maintained, adjusted, and used these models within an institutional framework to provide contextualized judgements and interpretations on policy and scientific matters. It was Klein, and not Marschak, who with his life-long commitment to the project, ended up consolidating a macroeconomic program useful to understand the economy and to evaluate the effects of different economic policies.

Yet, apart from Klein's enthusiasm, the development of macroeconomic modeling did require other important conditions that made the project possible and that resulted from an amalgam of important developments dating back to the 1920s and 1930s. The foundation of the *Econometric Society* in 1930 and of *Econometrica* in 1933, as well as the active participation of the Cowles Commission in the further development of econometrics, for instance, had opened a place for that discipline within the economics landscape. The work developed at Cowles in the 1940s owed much of its "inspiration" to the "contribution of Trygve Haavelmo" and his probability approach to econometrics (Klein 1991a, 113). As Alain Desrosières (1993, chapter 9) put it, the econometric work at Cowles allowed for the integration of disciplines that economists had considered incompatible until the 1930s: mathematical economics, descriptive statistics, statistical analysis, and probability calculus.

Furthermore, as Thomas A. Stapleford (2009) put it, "given both the widespread conviction that only planning of some form could overcome the depression and the common reference back to wartime mobilization as a model for planning success, vastly expanded and improved statistics seemed a prerequisite for long-term economic recovery" (146). The efforts of institutions like the National Bureau of Economic Research (NBER) founded by Wesley C. Mitchell in 1920, and Simon Kuznets's pioneering work on the production of national accounts at the same institution were the sources of the blooming of economic data necessary to feed the models.<sup>13</sup> Another element of importance to understand the rise of technical and

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<sup>13</sup> The problems of the way in which data enters the practice of macroeconomic modeling and of the construction of statistical data are treated only tangentially in this dissertation. Dealing with these

professionalized institutions in economics was the expansion of funding sources for economic research stemming from philanthropic initiatives (at least since the 1920s) and, most importantly, from “the vast increase in federal support through the National Science Foundation’s social science program and the systematic contractual use of economic research by military and civilian agencies” (Fourcade 2009, 67).<sup>14</sup> Significant examples of these were the provision of funding by philanthropic individuals and institutions like Alfred Cowles, the Rockefeller Foundation, and the Ford Foundation, which contributed to build the material conditions for this type of teamwork-based research to be developed in the rise of the era of “big science.”<sup>15</sup>

During 1944 and 1947, the Cowles Commission developed the first macroeconomic models of the United States that combined both Tinbergen’s pioneering effort with Haavelmo’s (1941; 1943; 1944) probabilistic approach to econometrics and the effort of a selected group of economists, statisticians, and mathematicians who worked together as a team to solve a particular problem (see chapter 4). The publication of Klein’s (1950) *Economic Fluctuations in the United States, 1921-1941*, was the outcome of this teamwork effort, providing three models “to describe the way in which the [economic] system actually work” as well as detailed descriptions of how to build these kinds of models, at least on paper (1).<sup>16</sup> The result was the construction of an economic system described by a set of *simultaneous* equations that expressed *all* the

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important problems in a systematic way would add too many elements to an already complex story. In any case, these omissions must be recognized as important limitations to my dissertation. For a history of the construction of different data in the US, and in particular of the Cost of Living Index see Stapleford (2009). See also Desrosières (1993; 2002) for a history of the construction of statistics combined with the construction of the state, and the sciences.

<sup>14</sup> See also Erickson *et al.* (2013), and Mirowski (2002).

<sup>15</sup> For a detailed account of the phenomenon of big science, the establishment of entire scientific and calculation laboratories both in the private and in the public sector see Grier (2005, especially Parts II and III).

<sup>16</sup> Although the protagonists in the construction of the model were conscious of the importance of the institutional setting of the project, and of the fact that his endeavor could only be fulfilled on a teamwork basis, there is no description of these features in the whole monograph apart from the brief acknowledgements.

*interrelationships* among the “measurable economic magnitudes which guide economic behavior” (2). The approach was Walrasian in the sense that its objective was to model the economy *as a whole*, and to “hope that [eventually] we may develop a complete social theory” that can be expressed in a comprehensive model (2).<sup>17</sup> The first step in the construction of these models was to discover the economic structure through the specification of “structural equations” (see chapter 3). Then, once the system was complete, sophisticated statistical methods like the maximum likelihood method of estimation should be ideally used both to estimate the parameters of the model, and to provide a picture of the way the economy worked.

Klein’s Model III, published only in 1950, had been used already in 1945 to provide the Bureau of Budget, the Department of Commerce, and the Federal Reserve Board with the first projections of the performance of the US economy using this type of macroeconomic models. Albert G. Hart, a collaborator of the Cowles Commission and research associate at the Committee for Economic Development (CED) at the time, convinced Marschak and Klein of presenting the results of the model before these governmental institutions in Washington D.C. “The Cowles-CED projections,” which showed optimistic results on the performance of the postwar US economy, “were not taken seriously, [however]; the response in all cases was that we should wait until mid year 1946, when we would find 6 million unemployed again and return to conditions of the Great Depression” (Klein 1991a, 114-115). The pessimistic answer of the institutions in Washington D.C. was hardly surprising, given the generalized belief that the postwar economy was at the edge of collapsing.

For a 25-year-old economist like Klein, this generalized belief might have represented an important obstacle to convince the experts in Washington that no crisis was ahead of the US economy. Klein’s projections, “though premature,” turned out to be quite accurate, building

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<sup>17</sup> See chapters 5 and 6. In the 1960s, with the availability of new data and the improvement of computers, macroeconomic models became “large-scale,” integrating dozens, hundreds and even thousands of equations.

his reputation as “the economist who predicted America’s economic boom after World War II” (Rifkin 2013). Yet, “the senior researchers at the Commission were not satisfied with the performance” of these models either, and were probably disappointed for losing the opportunity to initiate a closer collaboration with the government agencies in Washington.<sup>18</sup> These results led to a decline in the pursuit of empirical work at Cowles, which little by little changed the focus of its research program passing from emphasizing econometric research to activity analysis and pure economic theory (Klein 1991a). Klein, however, never abandoned his enthusiasm about what he thought were powerful tools, dedicating his whole life to this endeavor and becoming the central figure of twentieth century large-scale macroeconomic modeling (see chapter 2). Yet, it was clear to Klein that the tools alone were not to be considered powerful. Rather, it was the scientific practice around the tools that would render them informative, useful, and powerful.

### 1.3. The path-breaking economics of Lawrence R. Klein

Klein’s macroeconomic work was path-breaking because it marked a milestone in the way that macroeconomic knowledge was produced. With the development and consolidation of the macroeconometricians’ project, and particularly of Klein’s project, it was not possible to make a clear separation in macroeconomics between theory (economic or statistical), application, and policy anymore. As Gerd Gigerenzer (1992, 331) has argued elsewhere, both theory and data (but also policy) were “tool-laden.” Scientific tools or instruments allow for producing data, but they also allow for processing information, and to some extent, for the implementation of

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<sup>18</sup> To be sure, not all the responses by the Washington officials were dismissive of the Cowles-CED projections presented by Klein. Arthur Smithies, who worked for the Bureau of the Budget from 1942 until 1948, exchanged correspondence with Klein during December of 1945, inquiring about the Cowles’s macroeconomic methods and modeling. Lloyd A. Metzler, who worked for the Board of Governors of the Federal Reserve system also exchanged some correspondence with Marschak at the end of November 1945, where he thanked Marschak for the “stimulating talk before [their] seminar” and assured that he had “since received a number of calls from economists who heard the talk and were interested in pursuing the subject further” (Metzler to Marschak, November 30, 1945, I thank Olav Bjerkholt for sending me this correspondence).

policies. In any case, the nature of macroeconomic knowledge changed and these three dimensions came to be understood within the complex framework of a new scientific practice: macroeconomic modeling.<sup>19</sup> To be sure, Klein's objective was not to build a comprehensive and abstract macroeconomic theory that he would then test and apply in his econometric model. Instead, macroeconomic modeling would include a whole new scientific practice where theory, application, and policy issues would all find a place within the same "system of reasoning."<sup>20</sup>

This system of reasoning was not something that happened "in the mind" of the economists, but had necessarily a kind of materiality expressed in the model itself; in the physical facilities of an institution like the Cowles Commission or the Wharton School; in the individuals and their specific division of labor within a scientific team; in its calculation machines and computers; and in the internal and external seminars designed to discuss about the construction of the models; in short, in the practice of macroeconomic modeling. The type of knowledge that this system of reasoning produced was not only of forecasting and estimation results, but also of training and understanding. This system produced flexible tools that needed to be regularly tinkered, discussed, reconceived, and adjusted, for which the intervention of "experts" was necessary. These experts, who were needed at every stage of the construction of the system, were not external specialists or authorities, but stemmed from the modeling practice itself. Pretty much in the way described by Mary S. Morgan and Margaret Morrison (1999), these experts "learn[ed] [...] from building and from manipulating" the

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<sup>19</sup> Klein's macroeconomic program was not the only program in economics that changed the way of producing economic knowledge and the relation between theory, application and policy issues, of course. His program must be understood within a larger context in which economics was becoming a model-based science (see Morgan 2012).

<sup>20</sup> In the same way that Hacking (2002) prefers to use "style of reasoning" rather than "style of thinking," I also prefer to use the word *reasoning* over *thinking* to characterize the kind of "system" I am talking about. In fact, thinking seems to be more related to ideas and to something that happens inside the minds of the researchers. On the contrary, reasoning is more inclusive, since it is related to discussion, practices, and of course, thinking.

model, rather than just from looking at the model (12). By constructing models, by intervening at every stage of the modeling practice, and by being part of a team, these experts were trained to understand not only the functioning of the model, but also the functioning of the economy in a learning-by-doing process, providing as well context-dependent expert opinions. And so, no necessary attachment to any specific economic theory or ideology existed in this construction, maintenance, and development process of the system.

In the same vein as Malcolm Rutherford (2011) defined institutionalism as a “movement,” Klein’s influence can be also seen as a movement that was eclectic and that does not subscribe to any specific “school of economic thought.” Indeed, the empirical and real-world oriented nature of his project, combined with the context-dependent analysis of expert knowledge, cannot be bounded to any specific economic theory or ideology, but to a particular way of doing macroeconomics and of producing macroeconomic knowledge. Traditionally, Klein has been classified as a Keynesian, in part because of the title of his PhD dissertation (1944) and first book (1947a) *The Keynesian Revolution*. Yet, this characterization is more problematic and limiting than it is enlightening to understand Klein’s work. In fact, *The Keynesian Revolution* was a critical assessment of Keynes’s methodology, theory, and policy objectives from a Marxian perspective. While Klein might have been sympathetic to certain Keynesian viewpoints, this is probably a consequence of the contemporary *zeitgeist* described before, rather than a sign of profound convictions and convergences between Klein and Keynes (see chapter 2). Indeed, Keynes’s methodological approach is more distant from Klein’s Walrasianism, than from Milton Friedman’s Marshallianism (see chapters 3, 4, and 5). To be sure, Klein’s influence was on the construction of a scientific practice, which was pluralistic, teamwork-based, highly argumentative, painstaking, flexible, and open to other arguments and ideas; a practice that was also “market-oriented” in the sense that it responded to a particular demand by both

government and private institutions to (partially) plan the economy.<sup>21</sup> Klein developed this whole new scientific practice and discipline almost from scratch, and yet, he was not alone in his quest, benefiting not only from the collaboration of the whole Cowles's team, and from the economists' community of the 1940s and 1950s, but from important epistemological views and modeling strategies inherited from the 1920s and 1930s.<sup>22</sup>

Too often, historians of economics have argued that "American institutionalism was displaced because its model of quantification was made obsolete by the combined rise of mathematical economics and econometrics, which associated empiricism with the explicit formulation and testing of economic theory" (Fourcade 2009, 84). Yet, on the one hand, not all the influences of the institutionalist movement were completely displaced or forgotten; on the other hand, the model of quantification was not an attempt to explicitly formulate and test economic theory either. In fact, a closer study of Klein's work shows that important elements of the institutionalist approach to empirical work in macroeconomics remained, and are reflected not only in Klein's admiration for the work of the NBER, but also in the careful and painstaking attention that he conferred to the analysis of data, and that is not necessarily related with an exclusive conviction of testing theories (see chapters 2, 5, and 7). Another important institutionalist aspect of Klein's approach was his pluralistic and pragmatic flavor, as well as his determination to solve concrete and real-world economic and social issues (see chapter 3).<sup>23</sup>

This is not to say that Klein was an institutionalist, of course, but rather that important elements of the institutionalist movement remained in US-American academia and public sphere, shaping the economics discipline of the second half of the twentieth century.<sup>24</sup> Again,

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<sup>21</sup> See Fourcade (2009) for a discussion about US economists as "market-oriented science."

<sup>22</sup> I have already mentioned that there had been important initiatives to build macroeconometric models that inspired Klein, including Tinbergen's and Frisch's.

<sup>23</sup> Another resemblance between Klein's approach and the nature of institutionalism was "a shared notion of science as involving some form of empirical 'realism,' and [...] a view of economic and social arrangements or institutions as in need of significant reform" (Rutherford 2011, 8).

<sup>24</sup> In this sense, and contrarily to what the standard view tells about the place of institutionalism within the history of economics, this movement cannot be considered "an aside to the main story" (Rutherford

to inscribe Klein within a school of economic thought is not only far from being the objective of this dissertation, but this would also do no justice to the complexity of Klein’s economic practice. Besides relating Klein to Keynesianism, the *standard vision* in the history of macroeconomics has often related Klein with the Neoclassical Synthesis, a program that attempted the reconciliation of Walras’s and Keynes’s theories.<sup>25</sup> As Michel De Vroey and Pedro Duarte (2013) have shown, the term “Neoclassical Synthesis” is an “elusive notion,” however, from which at least two major understandings appear: first, one in which the authors would attempt at reconciling the Keynesian short-term with the Walrasian long-term worlds. Second, another in which the Neoclassical Synthesis would appear as “a pluralistic macroeconomics wherein Keynesian and Walrasian models exist side-by-side” (26). According to De Vroey and Duarte (2013) and to De Vroey (2015, chapter 3), Klein’s work was inscribed within the first understanding of the synthesis, i.e. as an attempt to reconcile Keynesian short-term and Walrasian long-term analyses. I do not think, however, that Klein’s work should be inscribed in that way within the Neoclassical Synthesis. On the contrary, I argue that if there was a sort of synthesis in Klein’s work, it was first because of Klein’s pluralistic approach, and second, because of a special methodological way to integrate Walrasianism with Keynesian, Marxian, and Neoclassical theory within the macroeconometric modeling framework (see chapters 4 and 5).

Methodologically speaking, Klein was a very special kind of Walrasian. He was not, of course, a pure Walrasian in the sense of being a follower of Léon Walras’s work itself, and of

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2011, 3). Also, another interesting parallel between the institutionalist movement and Klein regarding their places in the history of economics, is the idea that neither the institutionalist movement, nor Klein produced a well-specified theory. And so, the standard history of macroeconomics that considers the history of the discipline as a success of ideas and theories, does not find a place Klein (see section 1.6 above).

<sup>25</sup> I will come back to the definition of the *standard vision* in the history of macroeconomics (see section 1.6).



his impressive program of social, applied, and pure economics.<sup>26</sup> Neither was he a Walrasian in the sense of Vilfredo Pareto, i.e. a follower of Walras's "pure economics," of abstract and perfect relations.<sup>27</sup> Klein's Walrasianism must be understood as a label, which is built around an "epistemological credo" of general equilibrium and that keeps only the idea of simultaneity from the original work of Walras. Klein's Walrasianism becomes tangible only in terms of his modeling strategy, which consisted in representing the economy in all its complexity, and which emphasized the concept of simultaneity, and which is based on Haavelmo (1944) (see chapter 5).

Structural macroeconometricians like Klein were particularly bound to this credo of general equilibrium, which brought both a framework for the development of models and certain limitations. The macroeconometric "tool" itself allowed for the conception of the economy only as a set of simultaneous equations, always expressing the relationships between the variables in a general equilibrium framework. Econometrics did not establish only a method from which to produce a particular model system allowing for understanding and intervening the economy, but as Rutledge Vining (1947) put it, econometrics also put a "strait jacket" to the possibilities of explaining phenomena. In short, if the researcher sticks to a particular way of producing a model system in order to produce knowledge from a reasoned and logical procedure, then he also restricts himself to a number of possible explanations he can find only in that particular system. Other explanations that are not conceivable within the general equilibrium framework will then just be excluded, or they will not even be possibly conceived within the rules of this particular model system.

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<sup>26</sup> In fact, Walras's whole works only became available in English in 1954 with William Jaffé's 1954 translation, and so, apart from John Hicks's (1934), there were not many English sources where Klein could look up for Walras's work (see chapter 5).

<sup>27</sup> As I show in chapter 2, Klein's mathematical influences would never have pointed to a type of abstract mathematical economics. Quite on the contrary, Evans, Wilson, and Samuelson, taught Klein that the type mathematical economics that he should adopt would be one oriented towards the understanding of real-world and concrete problems. See also Part II of this dissertation for a comparison with Friedman's idea about macroeconomic modeling and mathematization.

1.4. Postwar macroeconomics: a combination of empirical models and expertise

Mid-twentieth century macroeconomics, like any empirically-oriented scientific discipline, was about observing the world and about trying to make sense out of its complexity and messiness. It was also about trying to exert control over specific parts of the economy to reach desired levels of economic activity, following specific methodological, theoretical and political principles. It was macroeconomists, of course, who, following these principles, observed this complex and messy world and who tried to exert control over particular parts of the economy. Sheltered by academic, governmental, or research institutions, they gathered together into creative and organized communities, from which they made their best efforts to observe, measure, explain, and control their surrounding reality. To this end, they invented and developed scientific tools, especially models, which provided them with “a practical form of reasoning,” and with a “method of exploration [and] of enquiry, into both their ideas and their world” (Morgan 2012, 38).

The mere adoption and use of these tools, however, was not sufficient to accomplish their goals of observation, understanding, and control. Macroeconomists needed to follow certain rules for their “tooled” observations, measurements and explanations to be validated as “scientific.”<sup>28</sup> These rules, were not explicitly written or defined, however. Rather, they should be understood in the same vein of Thomas Kuhn’s (1962) *paradigms*, according to which scientists “never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications” (46), resulting from an amalgam of practices, which follow an intricate process of collective development, negotiation and

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<sup>28</sup> John Maynard Keynes (1938), possibly following his father’s interpretation (J. N. Keynes 1890 [1910]), calls these rules “the art of choosing models.” To John Neville Keynes, economics was essentially an art, that is a “system of rules for the attainment of a given end” (35).

transmission before they get established within the scientific community, and before they provide legitimacy and justification to macroeconomists' findings and arguments. But, to be accepted by the community as "scientific" and as providing rigorous results, these practices had to be conceived first, then constructed, and used; afterwards, they had to be further developed through trial-and-error procedures, and through longstanding discussions and negotiations among practitioners. And thus, in the same way as "the meaning [...] of theories cannot be settled by general principles, but must be worked out by a narrow group of specialists" (Porter 1995, 219), so does the nature and meaning of scientific standards and evaluation of models necessarily originate from a collective activity.<sup>29</sup>

To be considered scientific, these practices must meet certain criteria that might vary not only among disciplines, but that will necessarily vary as well with time and space within any specific discipline. A clear case for these changing criteria between disciplines can be made through a quick comparison of the natural and the social sciences.

Natural scientists (in general) adopt laboratory practices that allow them to isolate, control, and reproduce phenomena through experimentation.<sup>30</sup> Since these practices can be invariably performed at (almost) any location and time, at least in principle, they can be understood as being "universal." On the contrary, social scientists (in general), and especially field scientists and macroeconomists, do not have the possibility of conducting this kind of experiments.<sup>31</sup>

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<sup>29</sup> The criteria that must be met for an observation or an explanation to be considered as "scientific" will not be the same in every scientific discipline, or at different points in time, of course. A very helpful way to understand what determines to be scientific or not is Leo Corry's (1989; 2004) distinction between "image and body of knowledge." The construction and establishment of these criteria make essential part of my dissertation. See Porter (1995, chapter 9).

<sup>30</sup>I take this very optimistic vision of the natural sciences only for the sake of the exposition. Natural scientists are not necessarily able to isolate, control or reproduce phenomena in this idealized way, of course.

<sup>31</sup> One of the exceptions being, of course, experimental economics. Yet, to think that one can conduct experiments in economics in the same vein as in the natural sciences is to ignore the nature of the phenomena studied, as well as the scope of the methods of economics. For a critical assessment of experimentation in economics in this vein see Boumans (2015, chapter 5). For a history of the development of the practice of experimental economics see Svorenčík (2015). For a history of experimentation in behavioral economics see Heukelom (2014, chapter 5). For critical assessments of experimentation in development economics see Favereau (2014).

Their experiments are the “products of *Nature*” (Haavelmo 1944, 50), and their only possible target of observation is the field.<sup>32</sup> They are confined to the problem of “passive observation,” i.e. the “observable results of what individuals, firms, etc., *actually do* in the course of events, not what they *might* do, or what they *think* they would do under other certain specified circumstances” (16).<sup>33</sup> Consequently, the phenomena macroeconomists analyze cannot be fully isolated, controlled or replicated, and so the data produced by these phenomena are never constructed in a “clean” and controlled environment. Thus, macroeconomic analyses must necessarily be the product of locally situated and rigorous reasoning. As Marcel Boumans (2015) puts it, “the standards of the field are much more context dependent and sensitive to local conditions,” and so the scientific standards in the field sciences cannot be “universal” in the same sense of the laboratory sciences (174). On the contrary, the scientific standards of the field sciences must be flexible enough to adapt to the local conditions in which the analyzed social phenomena are embedded. They, too, must be necessarily the product of collective constructions and negotiations among experts.

Yet, the fact of being more flexible, adaptable, local, collectively constructed and negotiated, i.e. the fact of being “less universal” and less rigid does not mean that field sciences like macroeconomics are less rigorous (Boumans 2015, 174). Rigor makes still part of the objectives pursuit by macroeconomists; only, the nature of this kind of rigor is different. Boumans (2015) defines the fulfillment of rigor in field sciences as “clinical judgement,” that is, as “the combination of model-based procedures of attaining precision and calibration combined with a rational consensus of expert judgements” (177). Similarly, “clinical judgement” can be understood in the light of Griffith C. Evans’s image of mathematical rigor as “materialist-reductionist quantification” (Weintraub 2002, 71), i.e. as a kind of rigor with a

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<sup>32</sup> Haavelmo’s problem of “passive observation.” See also Boumans (2015) for a fascinating account on measurement and observation in field sciences.

<sup>33</sup> For a comprehensive discussion of the problem of passive observation see Boumans (2015, chapter 4; 2010).

human face, where the researcher rather than his sophisticated mathematics, plays an important role in understanding the world in which he lives (see chapter 2).

I thus borrow Bouman's notion of "clinical judgment" to refer to this combination of mathematical and model-based procedure, and of a human face or rational and consensual expert judgement, to attain a certain standard of rigor or scientific norm. Yet, I want to add a third element to this notion that is related to the policy sphere. Understood as spatiotemporal entities, scientific practices are defined within institutional frameworks that are clearly identifiable in the case of macroeconomic modeling. This spatiotemporal characteristic provides the institutions and the practices with specific policy objectives that are necessarily introduced within the organizational frame of the practice, becoming (unconsciously) integrated into the modeling activity. The Klein-Goldberger model, for instance, was built with the explicit objective of performing both forecasts and policy simulations for the US economy, and asked questions that were imposed from the economic, political, and social reality that put pressure on the US society, in 1955. For instance, the way in which the discussion of the structure of the models takes place not only in Klein-Goldberger (1955) but also in Klein (1950) and Klein *et al.* (1961), in which the consumption function plays a paramount role, shows not only that this function is central for building the models in technical terms, but also that in the aftermath of World War II and after Keynes (1936), consumption constituted a priority for both stimulating the economy and providing welfare to the society.

Every social science, then, and especially macroeconomics since the 1940s, is necessarily a combination of at least three different elements. First, one that yearns for a kind of "mechanical objectivity," i.e. the kind of objectivity represented in routinized and standardized practices that render scientific results and analyses impersonal, neutral, and value-free, which in the case

of macroeconomics is translated into macroeconometric modeling.<sup>34</sup> The second element is the absolute necessity of experts who can interpret results in a context-dependent framework, and who can reach some kind of consensus of their results through negotiation and discussion. Yet, this consensus is not reached only at the level of the results. It must be reached also at the level of the practices, modeling strategies, and the mathematical, economic, and statistical theory used as well as the criteria adopted to construct and judge suitable methods and tools. In short, consensus must be reached to define in an implicit way what it means to perform good scientific practices. And the third element consists in the institutional framework necessary to carry out a scientific practice, to build scientific tools, and to shelter experts. This institutional framework, again, introduces the policy sphere into the scientific practice.

Experts can be understood both as individuals or institutions (Martini 2014; Boumans 2015), possessing different kinds of knowledge and capable of interpreting the mechanically obtained results through the lens of particular situations. Expert intervention, however, does not happen just in the political sphere or in the very last stage of the modeling process; nor does it happen in a whimsical or disorganized way. Experts are necessary at every stage of model building, as they follow certain practices that make their advices become rational and organized (see Boumans and Martini 2014; Boumans 2015, chapter 6). For instance, they gather together into specialized institutions and teams, they found journals to communicate, they organize seminars and conferences where they meet, discuss and negotiate, and they participate in methodological controversies. Gradually, through their participation in the scientific community and through their affiliation to key institutions, these experts create and define a scientific practice that is not only the product of technical and intellectual work, but also of social negotiations.

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<sup>34</sup> “Mechanical objectivity” is, of course, a very problematic notion that has also been constructed and developed throughout history and science. For a history of this notion see Daston and Galison (2007). See also Porter (1995).

The formative years of macroeconomics, i.e. the decades from the 1930s to the 1950s, provide a fascinating story about how macroeconomic modeling became a scientific practice, and about how macroeconomists adopted these model-based procedures combining them with a particular kind of reasoned consensus, and sheltered under an institutional frame. These formative years also shed light into how these rules and practices were created and transformed, especially through discussion and controversy, but also through the response to a specific political and social context that demanded concrete actions to manage the economy. Emphasizing the role of particular institutional settings, methodological controversies, and figurations of specific personae, I explore how the macroeconomists' community constructed, discussed and negotiated the criteria to judge the performance of their tools and their practices, i.e. how they constructed a specific kind of "clinical judgement." I show that it was through the building of macroeconomic models that macroeconomists not only trained themselves to become experts, but also that it was in the very practice of model-building that they defined and established the adequate criteria, tools, and practices to turn macroeconomics into a scientific discipline with policy objectives.

### 1.5. Scientific tools, practices, and experts

During the period that Joel Isaac (2010) has defined as the "tool age," i.e. during the 1940s and the 1950s, "[US] American science was plunged into the material, instrumental world of military research, and thereby into the plethora of engineering and logistical problems attendant on the conduct of mechanized warfare." This material and instrumental world brought important transformations in the kinds of tools that economists used to understand the economy and to give policy advise, since "[US] American-born and émigré scientists – physicists, most notably, but also mathematicians, chemists, economists, physiologists, and psychologists – were compelled to develop facility with, and to create, new machines, research tools, and engineering solutions" (136). Not only economists, but also scientists in general in the

US, were “immersed in technical, craftlike practices” during the 1940s, which were translated into practices of calculating, building, testing, modeling, and mapping (136). The empirical and instrumentalist principle of “getting the numbers out,” was the common denominator of the scientific sphere at the time.<sup>35</sup>

In macroeconomics, tools became more sophisticated in terms of mathematical and statistical methods but remained very attached to the goal of solving concrete real-world problems, very much in the mold of engineering. These transformations were also accompanied by a reinvention of the scientific practices necessary to master the new tools, and by a redefinition of the role of the economic expert. Therefore, three central themes to my dissertation are the study of (1) scientific tools, (2) scientific practices, and (3) macroeconomic experts.

#### 1.5.1. Scientific tools to understand and solve real-world problems

The development of scientific tools in economics is directly related to the image that twentieth century economists had of their discipline in terms of the practical orientation they thought the discipline should follow. The first decades of the twentieth century brought important perturbations in the social and economic order of the world: two world wars, an economic depression and the consequent efforts to recover from them, as well as the rise of communism and fascism to mention only a few of the most important upheavals. With these events, the role played by economists in this new and convoluted social, political, and economic order changed, and so did the way of doing economics. Embedded within a pluralistic context during the interwar period, economists in the United States did not converge into a particular theoretical agenda, but rather into a specific vision about what it meant to be “scientific” and about the

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<sup>35</sup> “Getting the numbers out” is the way in which physicist Richard P. Feynman described the kind of work he was doing during the 1940s in a 1980 interview with S. S. Schweber (1986, 96). See also Kaiser (2005b, 36), and Isaac (2010, 137).



inadequacy of an unregulated economy (Morgan and Rutherford 1998). As I mentioned before, the difficult social, political, and economic reality of these decades pushed the development of economics into a highly concrete and problem-oriented direction, compelling economists to develop and adopt tools that would serve their purposes, turning economics into a “tooled” discipline (Morgan 2003).<sup>36</sup>

The study of scientific tools in this dissertation is directly related to the very practical sense that early macroeconomists considered macroeconomics should follow. Economists like John M. Keynes, Jan Tinbergen, or Lawrence R. Klein, saw their primary scientific goal as that of achieving real-world outcomes.<sup>37</sup> Their interests were focused on providing practical solutions to real-world problems through the use of rationalized ways of thinking that required the invention and use of tools.<sup>38</sup> Yet, between the 1920s, 1930s, and 1940s, important changes occurred in the way economists thought about how to achieve these very practical real-world outcomes.

In a description that could be considered his self-portrait, Keynes (1924, 173) provides us with an exalted image of his expectation of the “rare combination of gifts” that “the master-economist must possess”:

[the economist] must be mathematician, historian, statesman, philosopher – in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future.

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<sup>36</sup> Morgan (2003, 277) calls it a “tool-based discipline,” when fields like institutional economics, labor economics, industrial organization business cycle analysis, agricultural economics, econometrics or macroeconomics flourished during this period. See for instance Morgan (1990) and Fox (1989).

<sup>37</sup> Note that the problem-oriented nature of the economics profession was not exclusive from the United States. European countries like England and The Netherlands, but also Germany, Norway and France, which experienced as well these political and social upheavals, oriented their sciences, and especially economics, towards the analysis and understanding of concrete real-world problems (see Fourcade 2009).

<sup>38</sup> For a discussion on the shift that macroeconomists experienced at least since the 1980s in the way of understanding the primary objective of macroeconomists see Colander (2009; 2013).

The tools of the 1920s English economists would be rather abstract in their majority, but above all they would embrace a set of both reflective and practical knowledge. In contrast, the 1930s Dutch economists interested in real-world problems would have a more technical and less speculative kind of training. To Tinbergen, for instance, the tools that an economist would need were those of mathematical shaping and statistical verification (Boumans 2005, 44).

Mathematical treatment is a powerful tool; it is, however, only applicable if the number of elements in the system is not too large. Subjects, commodities and markets have, therefore, to be combined in large groups, the whole community has to be schematised to a “model” before anything fruitful can be done. This process of schematisation is [...] more or less arbitrary. It could, of course, be done in a way other than has here been attempted. In a sense this is the “art” of economic research, depending partly on the attitude in which the approach is made (Tinbergen 1937, 8).

Besides embracing the mathematical and statistical tools from the 1930s and combining them, the 1940s US American economists would believe, in addition, that these scientific tools should remove the personal and subjective element provided by the economist.<sup>39</sup> To Klein (1947a, 111), for instance, it was “desirable to provide tools of analysis [...] that are, as much as possible, independent of the personal judgements of a particular investigator. Econometric models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims.”

Note that even if their emphasis on the different tools differs from one macroeconomist to the other, these three early macroeconomists understood their primary task as policy-oriented. Their policy-driven approach, however, cannot be understood as politically or ideologically oriented.<sup>40</sup> Rather, what I call a policy-driven approach is their understanding of economics as a science that deals with real-world and concrete problems, not the development of theories or

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<sup>39</sup> In the United States, however, this idea cannot be restricted to the economists of the 1940s, but can be found at least in the 1920s in the personification of economists like Wesley C. Mitchell. See for instance Mitchell (1925).

<sup>40</sup> This is not to say that these economists did not have their own (sometimes strong) political views and opinions that might have exerted some influence on their own policy recommendations and conclusions. These economists did of course have their own opinions and these might have been decisive in specific events and aspects of their careers.

the utilization of tools to advance ideologically-related arguments. Keynes, Tinbergen, and Klein, would not think about macroeconomics as an abstract way of understanding the world, but rather as a powerful means to provide solutions to concrete problems that suppose a certain degree of priority for each problem in each specific country. To provide solutions to these problems, macroeconomists understood that they needed tools both to think about and intervene the economy.

To think about economics as a practical, real-world-oriented, and as a “tooled” discipline, allows us to use the metaphor of economics as “engineering.” As Mary S. Morgan (2003, 276) puts it, “to understand twentieth-century economics as a science in the mold of engineering is to see that the economics profession came to rely on a certain precision of representation of the economic world, along with techniques of quantitative investigation and exact analysis.” These techniques of quantitative investigation, of precision and exactness, were provided, mostly, by tools, and in the period studied in this dissertation, especially by (large-scale) macroeconomic models. Contrarily to the tools used in engineering or in the natural sciences, however, the tools used by the economists were not physical objects.<sup>41</sup> Yet these tools had a materiality of their own. What I call a scientific tool is a set of objects constructed on a teamwork basis, and made up by words, hypotheses, numbers, parameters, relations, curves, graphs, and figures that form systems, which can be visible in printed form. They are molded through the use of mathematics (see Boumans 2005, chapter 2) and through the necessities imposed by methodological, political and data-related problems. In addition, complete institutions are built around these tools in the context of the rise of “big science,” like the Cowles Commission, the National Bureau of Economic Research, the Wharton School, the Survey Research Center, Brookings Institution, etc., which impose policy and scientific objectives on the tools, according to the way in which they build and use them. And most importantly, a whole new scientific practice is developed

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<sup>41</sup> Mary S. Morgan (2012, chapter 5) provides us with an example of a physical tool used in macroeconomics: The Newlyn-Phillips machine.

around the use of these tools. In this case, the new practice is macroeconometric modeling.

Although constructed for specific purposes and with a history of their own, from the point of view of the contemporary users, tools might get detached from their history and might be considered “autonomous” objects that provide the economist’s analysis with a certain kind of “neutrality” that warrants his “objectivity” (see Porter 1995, particularly part II; Morgan and Morrison 1999; Armatte 2010). The use of tools is important as one among several ways to reach some sort of mechanical objectivity. It is not only that scientific tools provide a “better” way to solve practical problems, allowing the decision makers to see possible results or scenarios with particular figures, instead of using abstract “speculative analysis” (see Porter 1995, 64); it is also that these tools that have their own materiality and autonomy become “talking things” with their own agency and with a life of their own, transforming their utterances in truth claims that are not distorted by the “filter of human interpretation” (Daston 2004, 13).

In short, the operation and use of a scientific tool like macroeconometric models must be inscribed within an institutional framework (a university, a research institute, a government office) that defines the policy purposes aimed by the tool. This institutional framework provides materiality to the tool in the sense that it provides a team of experts, equipment, seminars, and a physical location where the tool is operated. Sometimes, the institution provides a name for the tool as well, contributing to the construction of visibility and legitimacy for the tool. Within the institution, the tool provides an element of mechanical objectivity in the production of macroeconomic knowledge, framing the experts’ reasoning and judgement and the division of labor of the team, and providing a kind of rigor valued in our twentieth and twentieth-first century idea of science to bestow legitimacy to the whole macroeconometric exercise. To be sure, scientific practices originate in the construction of institutions around tools; in turn, it is through the construction of institutions that aim at producing economic knowledge that economics becomes a “tooled” discipline.

### 1.5.2. Macroeconometric modeling as scientific practice

The second theme central to my dissertation is scientific practices “epistemological practices” (Stapleford 2015). Following Stapleford, I define scientific or epistemological practices as “collections of behavior that are teleological, subject to normative evaluation [and that] exhibit regularities across people in a constrained portion of time and space,” contributing to the generation or sustainment of “formal knowledge that makes truth claims” (6).<sup>42</sup> In particular, macroeconomic modeling is a clear example of a collection of different practices imported from a number of kinds of behaviors and activities, constrained within the limits of an institution at a particular time. First, it combines general practices such as writing, formal and informal discussions, presentations of papers and ideas, publishing results, modeling theories, testing hypotheses, estimating parameters, tinkering with the models, adjusting and calibrating numbers, counting equations, or solving mathematical systems of differential equations.

Note, however, that macroeconomic modeling is fundamentally a collective practice. Even though some parts of it are constructed on an individual basis, the practice must be understood in its complexity, and as a teamwork activity. There is always a group of people, a team, that makes the fulfillment of the practice possible. Sometimes, the individualized task might seem so specialized that it is hard to see the collectiveness of the practice by focusing only on one specific task. Yet, every task must be learned, socialized, assessed, discussed, and even negotiated. To be sure, an important component of social integration lays under the notion of

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<sup>42</sup> Other definitions of “scientific practices” influential for this definition are those provided by Joseph Rouse (2002) and by Theodore Schatzki (1996). To Rouse (2002), for instance, “scientific practices are part of how human beings interact with their surroundings and with one another, in ways that can be and are held accountable as correct or incorrect, just or unjust, insightful or not” (184). Schatzki (1996) differentiates between three types of practices: (1) as a “temporally unfolding and spatially dispersed nexus of doings and sayings,” linked through “understanding [...] of what to say and do”; through “explicit rules, principles, precepts, and instructions”; and through “‘teleoaffective’ structures embracing ends, projects, tasks, purposes, beliefs, emotions, and moods.” (2) as “forming causal chains” consisting in the “succeeding action responding to the previous one.” These causal chains connection are understood of being part of practices, only “if they somehow ‘result’ or ‘follow’ from the practice’s organization, that is the understandings, rules, and teleoaffective structure linking the practice’s constituent actions.” And, (3) as “performing an action or carrying out a practice of the second sort. This notion denotes the do-ing, the actual activity or energization, at the heart of action” (89-90).

practice that goes beyond the more mechanical notion of system integration. While social integration is “the coordination of actions through the harmonization of ‘action orientation,’” “system integration is the coordination of actions through the functional intermeshing (*Vernetzung*) of the unintended consequences of actions” (Schatzki 2002, 89-90). This means that practices coordinate “actions by organizing factors that govern participants’ actions, and not by stabilizing the unintentional consequences of these actions” (90).

Second, macroeconomic modeling is also teleological in that it pursues a specific objective that might differ depending on the economists, institutions, and possibly on other factors like the funding involved in the construction of particular macroeconomic models. In the case of Klein, the primary objective of macroeconomic modeling was to provide sound policy evaluation and advice to the policy makers (see chapter 3).

Third, macroeconomic modeling was subject to normative evaluation. Even if this practice would vary according to the institutions, the objectives of the models, and the macroeconomists involved, there were certain standards in terms of practices that had to be met for the produced models to be considered scientific. Given the tools, and especially the econometric tools developed, at the Cowles Commission during the 1940s, large scale macroeconomic models were supposed to make use of these statistical techniques. The “right” way to understand economic variables was in terms of probabilities (Haavelmo 1944), and, in consequence, the right model specification and the right mathematical framework had to be used: a model of simultaneous differential equations. Also, the “right” approach meant to consider the *whole* economic system, and so specific statistical techniques for estimation had to be used accordingly. Ordinary least squares regressions were not adequate methods, at least in principle in principle, not necessarily because they produced biased results, but because estimation from reduced-form equations represented an incomplete methodological approach, acceptable only for certain purposes of prediction (see chapter 7). Instead, maximum likelihood

methods were to be used. But given the nascent character of the practice, the discipline and the community, all these criteria were still to be constructed, discussed and negotiated (see chapters 5 and 6). It is only once these practices and criteria are stabilized that they can show some regularity across people, and that they become constrained in a particular moment and place in history.

Finally, these practices contribute to generating or sustaining formal knowledge that makes truth claims. In a similar way to the functioning of Ian Hacking's (2002) "styles of reasoning," the practices developed within a specific discipline develop their own strategies of self-authentication, through which they attain some stability as well.

### 1.5.3. "Repersonalizing" history: Klein as the expert macroeconometrician

The study of tools and practices in my dissertation is essential because it provides materiality to the story I want to tell. Yet an examination of both scientific tools and epistemological practices deprived from their social and cultural context would yield an abstract and immaterial study of ideas. The people and the institutions that developed and performed these practices become thus both necessary and complementary elements to provide some true materiality to my history. It is, of course, people who provide these scientific objects like practices and tools with a cultural context and with a specific meaning, and who make their existence possible. Yet, the meaning of these objects does not rely on purely subjective interpretative bases. Rather, the very matter that composes these objects (mathematical form) and the way in which this matter has been put together (econometric models), necessarily provides a particular range of possibilities for these people (macroeconomists) to interpret and to provide particular meanings to these objects (see Daston 2004).

Consequently, the 1940s and 1950s community of macroeconomists constitutes the third central matter to my dissertation. In particular, I focus on one representative figure and expert of this community during the second half of the twentieth century: Lawrence R. Klein. In the

same vein of Till D ppe and E. Roy Weintraub (2014) for the cases of Lionel McKenzie, Kenneth Arrow, and G rard Debreu, my intention is not to write a personal biography of Klein, but rather to do a “repersonalization” (xv) of the history of macroeconomic modeling; to understand the development of this practice within not only its historical, political, and social contexts, but also its intellectual one.<sup>43</sup>

Klein is not only a path-breaking and pivotal figure to understand twentieth century history of economics, but he is also a fascinating and important character at any level of analysis. I have already mentioned that the path-breaking character of his work resides ultimately in the development of the new practice of macroeconomic modeling, which in turn had important effects on the production of knowledge in macroeconomics (see chapter 2). Given his pivotal and path-breaking position in twentieth century macroeconomic modeling, Klein plays the main role in my narrative, although he does not stand for anything like a “hero” or a “villain.”<sup>44</sup>

## 1.6. Towards a history of macroeconomic modeling

The 1960s experienced a proliferation of large-scale macroeconomic models around the world. Made up of hundreds of equations, these models analyzed specific sectors of each national economy in great detail, and were used for the formulation and evaluation of public policy, as well as for orienting specific firms in their production and planning decisions. The construction of these models during the 1960s, was possible, among other things, because of the pioneering and path-breaking efforts of Klein who developed not only important

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<sup>43</sup> D ppe and Weintraub (2014) make a “repersonalization” of general equilibrium theory, in which the building of personal credit plays a paramount role. This is not the approach I adopt here, although some elements of scientific credit might appear throughout the dissertation, of course.

<sup>44</sup> Given the importance of Klein’s contributions to economics (see Ball 1981), and his fascinating life (see Klein and Mariano 1987; Bjerkholt 2014a) both a history of econometrics taking Klein as the protagonist and a biography of him are, of course, legitimate subjects for a PhD thesis. My purpose and my methodological approach, however, are somewhat different, and neither would hold if I were to show Klein as a “hero” or “villain.”



pedagogical examples of these models (Klein 1950; 1955), but also the whole scientific practice behind the complex activity of building this kind of models. Yet, historians of economics have neglected the role played by these models so far.

On the one hand, some historians of macroeconomics have focused on the history of theories and ideas, without necessarily paying attention to the way in which these ideas were produced, as if one could abstract from the personae and from the academic, governmental, and private institutions in which they were produced. Following this line of thought, one of the most recent efforts to write a history of macroeconomics that aims at providing a comprehensive narrative of the discipline is De Vroey's (2016) *A History of Macroeconomics: from Keynes to Lucas and beyond*. Focusing on what he considers "the most salient episodes in macroeconomics," De Vroey offers an "internal history [that] leaves aside most of the contextual dimension" (xvi), and emphasizes the logical consistencies and inconsistencies of each theory.

Hoover (1988) offers a detailed account of the emergence of Neoclassical macroeconomics and of Friedman's monetarism. De Vroey and Hoover (2004) focus on the rise, fall and persistence of the IS-LM model for more than seventy years not only as a pedagogical device, but also as a simple and powerful tool to understand the functioning of macroeconomics in governmental institutions. Duarte and Lima (2012) have compiled a collection of essays that study the search for microfoundations in economics, while Backhouse and Boianowsky (2012) have focused on the emergence of the theory of disequilibrium.

On the other hand, historians of econometrics have focused on studying the foundation of econometrics (Morgan 1990; Dupont-Kieffer 2003), the development of the methods of estimation of structural econometrics and their criticisms (Epstein 1987; Qin 1993), and the expansion of some of the new methods of estimation since the 1970s like calibration and simulation (Qin 2013; Hoover 2001). Some of these studies have taken into account the history

of macroeconometric modeling in some way, but none of these treatments have been systematic, and they have relegated this practice to a secondary place in their narratives. The only account that has systematically treated macroeconometric modeling as the central theme of its narrative has been the history edited by Bodkin, Klein, and Marwah (1991) *A History of Macroeconometric Model-Building*. This account, although very informative and fascinating, presents important historiographical flaws characteristic of a narrative written by the protagonists of the events themselves that verges on the limits of Whig history. Some of the most important problems of this account are related to its failure to accept that large-scale macroeconometric models were not able to predict stagflation during the 1970s, and to neglect the importance of Lucas’s critique in 1976.

So far, the history of econometrics and the history of macroeconomics have been regarded as two separate entities. Although this way of looking at the history of these sub-disciplines has helped us understand the internal evolution of the theories and methods, it has also opened new grounds and questions to be explored. In my dissertation, I propose a way of bringing together macroeconomics and econometrics through the study of the history of macroeconometric modeling. This kind of history should provide us with a better and closer understanding of what macroeconomists really did in the mid-twentieth century, of the kind of questions they were trying to answer and of the vision that these macroeconomists had of their political, social and academic world.<sup>45</sup> My dissertation does not represent an exhaustive account of the history of macroeconometric modeling, of course, but it pretends represent some of the first steps in the direction of writing a new history of macroeconomics that focuses on the practice of macroeconometric modeling.

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<sup>45</sup> I must add the caveat that most of the archival material found in Klein’s papers in the Rubenstein Library at Duke University, although very rich and fascinating, date from the late 1960s onwards. Perhaps because the 1940s and 1950s were years of relative instability in his career characterized by numerous moves, Klein did not store his papers in a systematic way. Most of the archival material used in this dissertation for the years 1940s and 1950s stems from other archival sources different from Klein’s. In any case, Klein’s archives indicate a huge potential for the following decades (see chapter 8).

### 1.7. Outline of the argument: economics as a “tooled” discipline

My thesis is that Lawrence R. Klein was the central figure in the development of macroeconomic modeling, a practice that changed the way to produce economic knowledge in the second half of the twentieth century. In fact, when I say that economics became a “tooled” discipline, I refer to the construction of institutions around scientific tools and teams of experts to pursue concrete policy and scientific objectives, and that dominated macroeconomics in the postwar era. To be sure, this new way of producing scientific knowledge consisted in the construction and use of complex tools (macroeconomic models) within specific institutional configurations (econometric laboratories) and for explicit policy and scientific objectives, in which well-defined roles of experts (scientific teams) were embodied within a new scientific practice (macroeconomic modeling).

Taking Klein as a focal point and as a vehicle, I travel across the economics discipline of the 1940s and 1950s, and study the intersection between the history of macroeconomics and the history of econometrics, providing a new understanding of 20th century economics as a “tooled” discipline, in which theory, application, and policy become embedded within a scientific tool: a macroeconomic model. This new understanding presents the history of macroeconomics not as the product of ideological and purely theoretical issues, but rather of divergent epistemological views and modeling strategies that go back to the debates between US-Walrasian and US-Marshallian approaches to empirical macroeconomics.

My dissertation is divided in two parts, each of which is composed by three chapters. In the first part, “Klein’s formative years,” I basically explore Klein’s academic trajectory from 1938 to 1959. In Chapter 2, I explore how Klein constructed his own identity as a macroeconomist, and how he contributed to form this new scientific practice of macroeconomic modeling. Through the study of Klein’s academic life, I revisit the intellectual situation of the economics discipline during the 1940s and 1950s, as well as the relations that Klein established with different institutions and personae, which marked the

formation of his own identity as a macroeconomerician.

In Chapter 3, I consider Klein’s project to “redo” Tinbergen’s macroeconometric work as a well-informed reaction to Keynes’s criticism of Tinbergen (1938), and as one that decisively contributed to the development of macroeconometric modeling. As an expert in Keynesian thought and as a leading figure of Cowles’s macroeconometric program, Klein surmounted the difficult task of reconciling the Tinbergenian world which strove for the implementation of technical and rigorous devices from which to draw inferences with Keynes’s world, which showed a clear aversion to this kind of technicality, although not necessarily to empirical work.

In chapter 4, I provide an account of Klein’s distinctive way of doing econometrics. Focusing on Klein’s time at the Cowles Commission (1944-47), I discuss a series of fundamental publications and events that were decisive in shaping his *image* of econometrics. In particular, I argue that Klein’s adoption of a flexible and practice-oriented methodology, and his endorsement of pluralistic economic theories, resulted from his participation in actual empirical model-building. Furthermore, I show that Klein’s flexible approach contrasts with the prescriptive methodology resulting from the highly abstract and theoretical work led by his colleagues at Cowles. I conclude that Klein’s distinctive *image* of econometrics allowed him not only to enrich the process of model specification, but also to pursue the macroeconometric program beyond the 1940s, and to remain optimistic about what he thought was the political objective of econometrics: economic planning and social reform.

Part II, “The consolidation of macroeconometric modeling,” revisits the longstanding opposition between the econometrics program *à la* Klein, and the statistical economics program *à la* Friedman. Following Porter’s (1994) idea that “the modern history of American economics is fundamentally a history of “rival ideals of quantification,” rather than a history of rival theories or ideas of economic analysis, I study the longstanding opposition between the Cowles’s and the NBER’s approaches to empirical macroeconomics, and in particular between

Klein and Friedman.

In Chapter 5, I consider the Marshall-Walras divide as the point of departure and center of the methodological debate between these two empirical approaches to macroeconomics. I argue that the introduction of econometrics and the transformation of economics into a “tooled” discipline during the 1940s and 1950s changed the relations between economic theory, applied economics and the policy sphere. I insist on the fact that rather than bridging the gap between theory and data, macroeconometrics radically transformed the preeminence of theory over application, data and political issues in economics, and conclude that independently from the economist himself, the macroeconomic practice of the twentieth century (which implies adherence to the econometric tool) does not allow for a dissociation of theory, application and policy, but instead combines and fuses them into a single model system: macroeconomic modeling (whether structural or not). Here, I provide a different understanding of the Walras-Marshall divide which is not related to the opposition between general and partial equilibrium, but which actually refers to two different modeling strategies.

Chapter 6 clarifies these two different modeling strategies through the study of a particular historical event: Friedman’s debate with the Cowles Commission. I argue that the differences between these approaches do not consist only in the use of diverse statistical methods, economic theories or political ideas, but are deeply rooted in methodological principles and modeling strategies that raise questions on both the way macroeconometricians represent and understand the world, and on how they deal with problems of operationality and concrete-problem solving. While the Cowles’s Walrasian approach necessarily considers the economy as a whole, despite the economist’s inability to observe or understand the system in all its complexity, the Bureau’s (and especially Friedman’s) Marshallian approach takes into account this inability and considers that economic models should be perceived as a way to construct systems of thought based on the observation of specific and smaller parts of the economy.

Focusing on the 1957-1958 controversy between Milton Friedman, Gary Becker, and Lawrence R. Klein, I provide an account of the early discussions on how to evaluate macroeconomic models performance in Chapter 7. At this occasion, Friedman and Becker questioned Keynesian macroeconomic models for their inappropriate treatment of the consumption function, and for their inability to yield accurate predictions of income, resulting from the adoption of the “wrong” criterion to judge models performance. While macroeconomicians adopted reduced forms extrapolations to evaluate their models, Friedman and Becker insisted on the necessity of carrying out full model simulations to conduct sound model selection. Independently of Friedman and Becker’s critical tone, I conclude that their argument can be interpreted as a constructive critique and as a precursor of a criterion to evaluate models performance that became common ground around macroeconomicians in the subsequent decades and which came to be known as full- or dynamic model simulations.

In a nutshell, the thesis of my dissertation is that the transformation of postwar macroeconomics consisted on the development and stabilization of new *empirical* scientific practices, and especially of quantitative modeling practices, that constructed, adopted and adapted scientific tools to produce a new kind of routinized, teamwork-based, and rigorous economic knowledge, which did not allow for a clear disassociation of theory, application, and policy matters. Given that there cannot be an explicit and exact theory in macroeconomics because of its character of field science, the postwar introduction of mathematical and statistical methods to macroeconomics did not seek to formulate general laws or to provide the economists with a “crystal ball” to predict future economic events. Rather, the introduction of mathematical and statistical methods is related to the idea of developing a reasoned and routinized way to produce a kind of rigorous knowledge in a context of mechanization and Cold War rationality, which nevertheless, kept a human face, whether in the form of an individual expert or of a team of experts affiliated to a particular institution. I consider large-

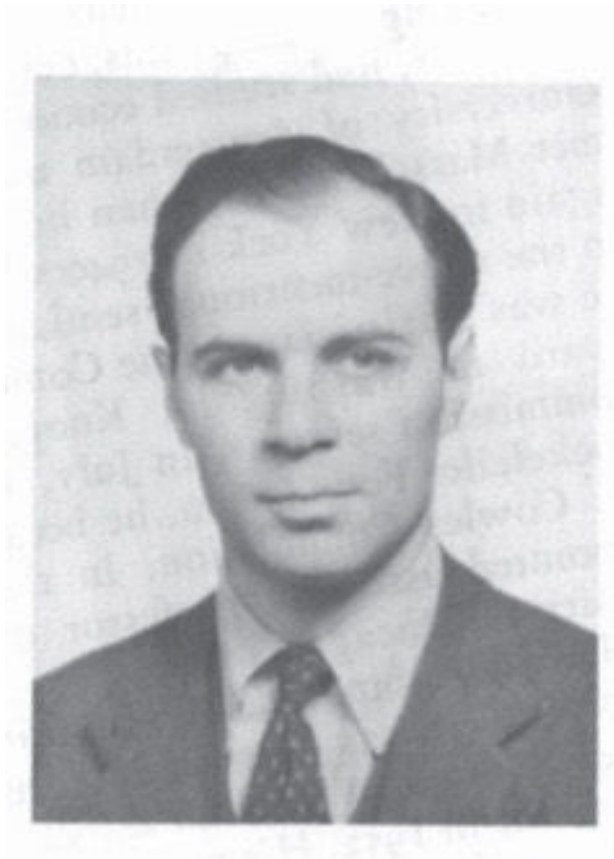
scale macroeconometric modeling *à la* Klein as the most important of these new practices in postwar macroeconomics.







## Part I: Klein's formative years



Lawrence R. Klein at the Cowles Commission, 1944-1947  
Source: Christ (1952, 28)



## Klein's formative years

The Western States are nervous under the beginning change. Need is the stimulus to concept, concept to action. A half-million people moving over the country; a million more restive, ready to move; ten million more feeling the first nervousness. And tractors turning the multiple furrows in the vacant land.

*John Steinbeck, 1939*

What the [Cowles] Commission provided [...] was an abiding interest in Keynesian macroeconomics and, even more importantly, a society of able people with much the same orientation as mine: Hurwicz, Marschak, Koopmans, Rubin, and many others. The staff meeting were models of constructive intellectual violence, a style very much encouraged by Marschak [...] we got reactions from each other very quickly; the atmosphere was highly cooperative.

*Kenneth J. Arrow, 1982*

Young recruits must be *trained* to become working scientists. Part of this training has always involved learning what is to *be* a scientist. What is the proper role or self-image? What are the accepted norms, values, and behaviors?

*David Kaiser, 2005*

Just like any other scientist, economists are not born; they are made. The making of an economist, which depends on the institutional configuration and personal experience that surrounds every economist's education, training, and professional life, determines the way in which economists understand the world and produce knowledge. The first part of my dissertation deals precisely with these matters of education, (formal and informal) training, personal experience, and institutional configuration around the specific case of Klein, and asks the following questions: How was Klein made and formed as an economist? What were the crucial events that marked his early life and career, shaping him into one of the most influential macroeconomists and econometricians in the second half of the twentieth century? How did his personal experience, his personality, and his liberty of interpretation and creativeness affect the development of his own way of doing macroeconomics? What were the institutions and

personae by means of which Klein became the macroeconometrician we knew? And finally, how did Klein himself influence the creation and design of institutions that allowed him to pursue his life-long project of macroeconomic modeling?

In essence, the next three chapters examine how Klein formed his own identity as a macroeconometrician and how he constructed and developed the new scientific practice of macroeconomic modeling, setting the basis for my argument that presents Klein as a path-breaking figure in the history of both economics and macroeconomics. In fact, the development of Klein's identity as a macroeconometrician goes in two directions. First, as a "young prodigy," Klein's image of economics and the world was clearly influenced by specific institutions and personae. Second, once his identity as a macroeconometrician was formed, Klein was able to influence and reshape both the institutions and the people that allowed him to develop and disseminate his new scientific practice. In particular, the development of this new practice took place within a specific institutional configuration, which, in turn, changed the way in which economic experts operated, passing from individual economists sitting in their offices to the construction of complete teams, organized in econometric laboratories that combined a precise division of labor with the use of macroeconomic models to produce knowledge, and express scientific opinions on economic policy in a systematic and organized way.

During the first decade of his career, from 1938 to 1950, Klein not only followed a formal economics study course at various universities, but he also obtained his first jobs as research associate at the Cowles Commission, the NBER, and the Michigan Survey Research Center. During that very first decade, Klein met some of the most influential and celebrated economists and scientists of the time (such as Griffith Evans, Jerzy Neyman, Edwin B. Wilson, Paul A. Samuelson, and Jacob Marschak), and many young economists who would, like him, become very influential (such as Milton Friedman and Trygve Haavelmo).

In 1938, Klein, an 18-year-old Midwestern from Nebraska, moved to California at a time

of economic, social, and political turmoil. Although there is no clear evidence of the reasons why Klein moved to the West Coast, it is hard to picture those days without referring to John Steinbeck's *The Grapes of Wrath*. The generalized difficulties of the times certainly marked Klein's way of thinking and looking at the world. As Klein (2004b, 17) himself remarked, he “entered economics because, as a youth of the depression, [he] wanted intensely to have some understanding of what was going on.” It was, indeed, “psychologically difficult to grow up during the depression. It was easy to become discouraged about economic life and [the aftermath of the Great Depression] did not give one, at eighteen or twenty years, the feeling that there were boundless opportunities just waiting for exploitation.” Like the story of the Joad family, during the 1930s hordes of people from the Midwest, deprived from their lands and work, moved to California looking for new opportunities, trying to survive. Yet, many of these Midwestern families, like the Joads, did not find new opportunities in the Western States, but poverty, violence and death. Although Klein's personal experience in California turned out to be very different compared to the Joads', the social, political, and economic tensions of the epoch did have an important effect on him. In academic and professional terms, however, California, and particularly UC Berkeley, opened a whole new world of opportunities to this Midwestern which he enjoyed and from which he truly benefited.

In Chapter 2, I examine Klein's life-story from 1938 to 1959, tracing back his passage through California, Cambridge, Chicago, Europe, Ann Arbor, Europe again, and finally Pennsylvania. This chapter should be considered an intellectual history, which examines Klein's trajectory throughout these two decades, as well as the encounters with different people and institutions that marked Klein's identity as an economist, and that gave him the tools and knowledge to develop his own program of macroeconomic modeling, and to become a central factor of influence that shaped the configuration and identity of the institutions and people around him.

In Chapter 3, I take a magnifying glass and focus on two important events for Klein's career. First, the Keynes-Tinbergen controversy, and second Klein's recruitment at the Cowles Commission to remake Tinbergen's model of the US economy. In 1944, and only five years after the controversy between Keynes and Tinbergen, Klein found himself in a peculiar position: on the one hand, he was one of the experts on Keynesian economics in the United States. On the other hand, he had been recently recruited to remake Tinbergen's model of the US. I present Klein's macroeconomic approach as a serious attempt to reconcile Tinbergen's work with Keynes's criticism. In Klein's approach, the macroeconomic modeling practice represented both a powerful way to discover things about the economic world (as Tinbergen believed), and a way of taking seriously into account the limitations of the statistical and mathematical methods that Keynes attacked. In the end, rather than the econometric tool alone, it was the whole practice of macroeconomic modeling that allowed the macroeconomist to discover, understand, and make concrete statements about the world. The power of the econometric tool resided, not in the tool itself, but in the combination of a prudential use of mathematics and statistics, and a teamwork-based effort to construct the model, in which discussion, painstaking calculation and analysis, as well as ongoing work played a paramount part.

It was precisely his passage through the Cowles Commission, which impregnated Klein's thinking and practice with this highly cooperative conviction that characterized Klein's life-long scientific program, and so in Chapter 4, I study Klein's early years as a researcher at Cowles, i.e. from 1944 to 1947. Although the history of the Commission is quite well known among historians of economics, the role that Klein played both in the dissemination and further development of econometrics *à la* Cowles has been hardly studied.<sup>46</sup> In addition, the variety in

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<sup>46</sup> For the most important accounts on the history of the Cowles Commission and in particular on the econometric program see Christ (1956; 1994), Hildreth (1985), Klein (1991), Morgan (1990), Epstein (1989), Bjerkholt (2007; 2014a; 2014b; 2015), Boumans (2013b), and Hoover (2014).

the conception of econometrics and economics among the members of Cowles remain unexplored. An example of this unexplored area that I study here is the contrast that existed between the elegant and theoretical mathematical econometrics developed by people like Koopmans, Herman Rubin, or Marschak, and the empirical econometric approach developed by Klein. In this chapter, I argue that Klein’s adoption of a flexible and problem-oriented approach to econometrics, his endorsement of pluralistic theories, and his participation in actual modeling practices were decisive elements in the further development and dissemination of econometrics among the economics community. While Cowles abandoned the econometric program shortly after Klein left the Commission, Klein continued to develop his own approach pursuing his life-long conviction that macroeconomic modeling should provide powerful tools for economic planning and social reform. Despite the differences in Klein’s approach that I underline in this chapter, the Cowles period was fundamental in shaping Klein’s conception of how science should be practiced. A common background remained for all the members of the Commission, consisting on the creation of highly qualified and cooperative teams to build macroeconomic models.

In a nutshell, during these early years, Klein was formed as a mathematical and statistical economist, something that was quite new at the time and which became his very identity as an economist. What is more important, however, is that during these years Klein actually crafted the new practice of macroeconomic modeling, which makes him a unique and path-breaking figure among his contemporaries.





# From the “Dwellers-on-the-bluff” to the Ivy League: the making of a macroeconometrician

The true theorist in economics has to become at the same time a statistician.

*Ragnar Frisch, 1930*

Although I was not aware of it at the time, the experience of growing up during the Great Depression was to have a profound impact on my intellectual and professional career. Collegiate life subsequently gave me a basis for understanding this experience and to develop some analytical skills for dealing with the important economic aspects of this era, as well as the exciting times that were to come – World War II, postwar reconstruction, and expansion.

*Lawrence R. Klein, 1980*

I used to say that after Klein’s PhD, it was diminishing returns all the way [...] Best of all Laurie, all that you accomplished you did your own way. Imagine being at all such places as Evans’s Berkeley, MIT, Chicago’s Cowles, Ann Arbor in the Musgrave, Boulding era, Oxford, Oslo, Rotterdam, and Penn. Time could not stale Cleopatra’s charms or your sagacities.

*Paul A. Samuelson, letter to Lawrence R. Klein, 31 January 2006*

### 2.1. Introduction

During the 1940s and 1950s, Lawrence R. Klein created a new scientific practice that dominated macroeconomics throughout the subsequent decades: macroeconomic modeling. In fact, along the 1960s, 1970s, and 1980s, governmental institutions, university departments, and private organizations, saw an expansion in the construction of large-scale macroeconomic models based on Klein’s early work. At the summit of his career, these achievements won Klein the 1980 Nobel Prize in economics, and confirmed his paramount place in the history of twentieth century economics.

Sixty years before, on September 14, 1920, the Midwestern city of Omaha – which translates “dwellers-on-the-bluff” in Dhegiha (Mathew 1961, 91) – saw the birth of Klein, who was to become one of its more prominent sons. Klein grew up during a very difficult time for the United States. These were the years of the Great Depression, which affected the entire country, hitting particularly strongly the Midwestern states. As documented by John Steinbeck in his novel *The Grapes of Wrath*, hunger, unemployment, displacement, and bankruptcy in the Midwest forced people to move to the West, predominantly to California, in search of better fortune. There is no definite evidence to point at hunger and poverty as the primary reasons behind Klein’s move to Los Angeles in 1938, but it is safe to say that Klein too, must have seen California as a land of opportunity, as it indeed turned out to be for him.

In 1938, Klein moved to California where he entered the Los Angeles City College, “then a junior college in the public two-year system of California” (letter from Klein to E. Roy Weintraub, 11 October 2010, LRKP, box 30). In the fall of 1939, he joined the University of California, Berkeley, through which he forged himself the path to become one of the most prominent economists of the twentieth century. As documented by John K. Galbraith (1997), in the thirties Berkeley, “the great love of his life” (54), was a place where “professors [...] knew their subject and, paradoxically, invited debate on what they knew” and “also had time to talk at length” (46); “the graduate students [...] were uniformly radical and the most distinguished were Communists” (49). The economics department at Berkeley was strong in agricultural economics, but it was also in the process of becoming an important center for the study of mathematical and statistical economics, Klein’s areas of interest. There is no doubt that UC Berkeley was the trampoline that launched Klein’s career, opening him some of the biggest and most important doors of the time. After his period at Berkeley, Klein seemed always to be in the right place, at the right moment, and with the right people. Yet, it was through his dedication, his agreeable way to treat people, his effort, and his intelligence that he made

himself an important place in both each institution he visited, and in the life of each person he encountered.

After completing his BA in economics in 1942 at UC Berkeley, at the time of Jerzy Neyman and Griffith Evans, Klein went to the Massachusetts Institute of Technology (MIT) to work under Paul A. Samuelson’s supervision, and to complete his PhD on *The Keynesian Revolution* (Klein 1944). Afterwards, in 1944, Klein joined the Cowles Commission which was under Jacob Marschak’s directorship, before going to Europe to work with Ragnar Frisch and Trygve Haavelmo, where he also met Richard Stone and Jan Tinbergen (Klein 1980a). In 1948, and after his one-year stay in Norway, England and the Netherlands, Klein accepted an invitation by Arthur Burns and returned to the United States to work at the National Bureau of Economic Research (NBER), precisely at the zenith of the “Measurement without theory” controversy between the Bureau and Cowles. Attracted by the Survey Research Center (SRC), Klein moved in “November or December of ‘49” (Klein 1980b) to the University of Michigan, in Ann Arbor, where George Katona and Richard Musgrave were teaching, and where he built, his famous Klein-Goldberger model (Klein and Goldberger 1955) together with Arthur S. Goldberger. Harassed by the anti-democratic pressures of the post-WWII period, Klein suffered from the effects of McCarthyism, and was forced to leave the country “for the peace and academic freedom of Oxford” University in 1954. After constructing a macroeconomic model of the United Kingdom (Klein et al. 1961), and once he had set his mind to stay in Oxford, Klein eventually returned to the US, accepting his new appointment as professor at the University of Pennsylvania in 1958 where he continued regular teaching activities until his retirement 1991. This early stage of his career, from 1938 to the late-1950s, in which he opened and forged his place as a macroeconometrician among some of the most prominent members of the economics community, culminated with his awarding of the John Bates Clark Medal in 1959, just before he turned 40.

Since the very first stages of his undergraduate studies starting in 1938, Klein felt an “early fascination with higher mathematics” which “blossomed into speculative thinking that could provide a basis for dealing with economic issues” (Klein 1980). First, at Los Angeles City College, and then at Berkeley the “teachings of the mathematics faculty [...] provided [him] with great stimulus.” In addition, “the onset of World War II, with all the associated disturbances leading up to it, made a tremendous impression on [his] thoughts about socio-politico-economic interrelationships” (Klein 1980), marking his later convictions about intervention, economic planning, and social reform.

In addition, Klein begun his studies in economics and mathematics at a time of important changes in both the US-American university landscape, and the economics discipline (Morgan and Rutherford 1998). Although still not dominant at the time (Backhouse 1998), technical teaching that included econometric, statistical and mathematical methods were gradually gaining prominence in undergraduate and graduate economic programs across the country.<sup>47</sup> Yet, this transformation in the education and training of US-economists was neither abrupt nor homogeneous. Quite on the contrary, each academic establishment underwent a particular process of transformation characterized by its own specificities and personae, which provide complex stories at the level of each institution and individual.

This ongoing transformation did not occur only at the educational level, however. What was considered scientific and objective in economics was also changing. The new boundaries expanded towards a more technical or mechanical type of objectivity represented in the construction of routinized and standardized practices that attempted at rendering scientific results and analyses impersonal, neutral, and value-free.<sup>48</sup> Yet, this transformation was not

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<sup>47</sup> For an account of the transformation occurred at the MIT department of economics, for example, see Weintraub (2014a). Emmet (1998) provides an account of the transformation at the University of Chicago, “as part of a transformation that narrowed the boundaries of ‘legitimate’ economics in the attempt to entrench it in its area of scientific competence” (135).

<sup>48</sup> For a history of the notion of “mechanical objectivity” in the natural sciences see Daston and Galison (2007), and Porter (1995).

immediate either, and many of the interwar values of pluralism, of “moral committed to ensure scientific inquiry, and [of] evenhanded objectivity” (Morgan and Rutherford 1998) kept playing a major role in the definition of economists’ ethos.

In a sense, both Klein’s socialization as an economist, and his educational and training trajectory are representative of the kind of scientific and professional education any student in economics of this generation might have received.<sup>49</sup> This particular socialization taught him how economic science was done, which was the role, self-image, values, norms, and behaviors of an economist during the 1940s and 1950s. Yet, Klein’s experience is also the story of a personal academic life, and so it must be understood in its uniqueness too. Both the people he encountered and the institutions he visited are part of Klein’s personal process of becoming an economist, of recognizing himself as such, and of forming (and inventing) his own identity as a rather new type of economist: a macroeconometrician.<sup>50</sup>

By the time Klein embarked on his economics education in 1938, both econometrics and macroeconomics had hardly been established as fully-fledged disciplines.<sup>51</sup> While the Econometric Society had been founded in 1930 and *Econometrica* in 1933, the term “macroeconomics” became of common use only during the 1950s.<sup>52</sup> Compared to

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<sup>49</sup> For an account on the importance of pedagogy in the making of scientists see Kaiser (2005a).

<sup>50</sup> For an account on the formation of an identity as scientist in the lives of Kenneth Arrow, Lionel McKenzie and Gérard Debreu see Döppe and Weintraub (2014). Note, however, that Döppe and Weintraub focus on questions of scientific credit, which I do not take into account in the case of Klein.

<sup>51</sup> For example, Morgan (1990, 259) argues that the beginning of the maturity period in the case of econometrics was marked only by the publication of Haavelmo’s (1944) paper, and that it was only at that moment that economists found themselves with the possibility of looking at econometrics as a complete research program and not as a series of separate works that provided practical solutions and insights in an individual manner. Furthermore, as Weintraub and Döppe (2014b, 477) put it, still in 1949 “half of the members of the Econometric Society and certainly most of the members of the American Economic Association, were in no position to appreciate [the work of mathematical economists.]”

<sup>52</sup> Through a search in 97 JSTOR economics journals between 1938-2006, and relating a term in the macroeconomics family (such as “macroeconomics,” “macroeconomic,” “macro economic,” or “macro economics”) to the total number of articles, Hoover (2012b, 22) shows that the term “macroeconomics” was practically inexistent in the economics literature until the mid 1940s, although, some economists like Ragnar Frisch, Jan Tinbergen, and J. M. Fleming had already use the term in the 1930s. After the mid-1940s, however, the use of the term become more common, stabilizing at around 23 percent in the 1980s. Again, this does not mean that there were no macroeconomic works before that time, but rather

econometricians and macroeconomists, macroeconometricians were even more scarce. Only Jan Tinbergen in the distant and occupied Netherlands, provided a clear example of what it meant to be a macroeconomic modeler. In short, during these early years Klein not only forged his own identity as a macroeconometrician, but also crafted a new scientific practice of macroeconomic modeling. In this chapter I want to understand how specific personae, events and institutions marked Klein's own identity as a macroeconometrician, and how this identity contributed to the construction of the new scientific practice of macroeconomic modeling.

## 2.2. The Neyman-Kuznets-Evans connection at Berkeley: statistical testing, empirical work, and mathematical rigor, 1940-1942

Klein arrived at the University of California, Berkeley, in 1940 after completing two years at Los Angeles City College. Although Berkeley "was not a leading center in mathematical economics" at that moment, it provided "a rather good environment [...] to someone interested in mathematical economics and econometrics" (Klein and Mariano 1987, 410). This good environment to learn mathematical economics was stimulated mainly by the presence of, at least, four important figures that marked Klein's vision on the use of mathematics and statistics at a very early stage: Jerzy Neyman, George M. Kuznets (Simon Kuznets's younger brother), Griffith C. Evans, and Francis W. Dresch.

### 2.2.1. Mathematical statistics at Berkeley

At the time of Klein's arrival in Berkeley, UC Berkeley was not recognized as a strong institution in statistics. Yet, only two years before, in 1938, Berkeley had hired the already internationally renowned statistician Jerzy Neyman.<sup>53</sup> After four years as a reader in statistics

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that the self-identity of "macroeconomists" or "econometricians" was still weak and the community was still at an embryonic stage.

<sup>53</sup> Jerzy Neyman (1894-1981) was born in Bendery, today's Moldavia from a Polish family. He studied mathematics at the University of Kharkov, Ukraine, between 1912-1916. "During the turbulent years that followed [Neyman] was arrested as an enemy alien, and later forcibly sent to Poland in an exchange

at the University College London, Neyman had rejected an offer from Michigan University where statistics was richly developed (Reid 1998, 154), and had chosen to go to Berkeley where he wanted to be in “a position to start a cell of statistical research and teaching from the start, not being hampered by any existing traditions and routines which were established long ago, for no good reason, and [were] still [...] being respected for no good reason either” (Reid 1998, 151).<sup>54</sup> In short, Neyman wanted to build a statistics program “in his own lines” (Kendall *et al.* 1984, 163), which meant to build a program with a strong focus on mathematical statistics.<sup>55</sup> Neyman did not have major problems in building his own program and, in 1945, after only a few years in California, he had founded the Statistical Laboratory, and the Berkeley Symposia, which consolidated Berkeley’s position as a top university for the study of mathematical statistics (Stigler 1999). According to Francis Dresch, it was only with Neyman’s arrival that he and his colleagues started to learn “real statistics together”:

All of us [...] had some kind of smattering [...] of somebody else’s notion of statistics, on a very elementary level, so we all attended Neyman’s lectures. He also established a kind of workshop. It went on the university books as a two-hour lab section, but it really lasted more like four. Essentially it was a kind of bonehead session on miscellaneous bits of mathematics that were required for statistics and that he thought were kind of skimmed by the mathematics department [...] During the time I was giving some of Neyman’s courses, George Dantzig [now a professor at Stanford] was running that so-called lab section. At one point, I remember, Neyman was also conducting a seminar in economics, an evening seminar, which was held at Evans’s

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of prisoners. Thus the random fortunes of war forced [Neyman] to leave the land of his birth and take up residence in the land of his fathers, and these same events also caused him to abandon a career in pure mathematics and turn to statistics” (Kendall *et al.* 1984, 163). In 1921, he found a job at the Institute of Agricultural Research at Bydgoszcz, moving later “to Warsaw as Head of the Biometric Laboratory in the Nencki Institute.” In 1934, he was appointed lecturer and reader in Statistics at University College London. Four years later he left for the University of California, Berkeley. For a biographical account on Neyman see Reid (1998).

<sup>54</sup> The statistics department at Michigan relied on one such tradition, and had a leading position in the United States. It boasted “the most completely equipped statistical laboratories possessed by any university in the world, [...] courses in elementary and advanced mathematical statistics of long standing and wide reputation, [...] an increasing number of graduate students whose primary interest lies in the mathematical theory of statistics, [...] staff members of outstanding competence and experience, [...] a very complete mathematical and statistical library” (Reid 1998, 154).

<sup>55</sup> Economics was not the only science that experienced a strong debate and transformation because of its increasing mathematization. Statistics, too, was itself immersed in this debate. See the discussion between M. G. Kendall and Udny Yule quoted in Kendall (1942).



house. Evans was there of course. Also Larry Klein, who later won a Nobel Prize for economics. One of the students in my course was Julia Bowman [later Robinson, the first woman to be elected president of the American Mathematical Society]. I think our first official student from ‘outside’ was Dantzig. We really had quite a group” (Dresch in Reid 1998, 168).

Besides attending Neyman’s course referred to by Dresch, Klein also “worked with a lot of Neyman’s disciples at that time” (Klein and Mariano 1987, 410), getting acquainted with mathematical statistics and hypotheses testing *à la* Neyman-Pearson from a very early stage in his career. The impression that Neyman must have exerted on students interested in statistics at Berkeley must have been remarkable. After all, Neyman was one of the most important statisticians in the world, especially after his 1933 publication, together with Egon S. Pearson, “On the problem of the most efficient tests of statistical hypotheses,” which had marked a milestone for statistical testing procedures.

One of Neyman’s disciples Klein worked with was George M. Kuznets.<sup>56</sup> In 1941, Kuznets completed his PhD in psychometrics in Berkeley, where he joined the Department of Agricultural Economics, and embarked in empirical research to estimate demand functions for lemons in California. At Berkeley, Klein spent one summer (in 1942) working with the Gianini Foundation as an assistant to Kuznets, who “was a very good statistician, though his degree was in psychology” (Klein and Mariano 1987, 411). This collaboration between Kuznets and Klein ended up in Klein’s first publication: *A statistical analysis of the domestic demand for lemons: 1921-1941*.<sup>57</sup> Despite the differences between the work that Klein performed during that summer, consisting on an applied microeconomic exercise, and his future macroeconomic

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<sup>56</sup> Kuznets was born in Kiev in 1909, and died in 1986. He moved to the United States in the 1920s, earning his BA in psychology in 1933, and his PhD in 1941 from the University of California, Berkeley (American Journal of Agricultural Economics 1982). From 1937 to 1939 he taught psychology and education at Stanford University and in 1941 he joined the department of Agricultural Economics at UC Berkeley, where he became a professor in 1952 and retired in 1977. His main contributions were in the fields of agricultural economics and applied microeconometrics. For a brief account on Kuznets see (American Journal of Agricultural Economics 1982).

<sup>57</sup> This publication constituted a report for the Giannini Foundation of Agricultural Economics, UC Berkeley, by the California Agricultural Experiment Station.

work, this experience of getting into the field to analyze real data marked Klein’s enthusiasm for statistical work in economics, which he never abandoned.<sup>58</sup>

### 2.2.2. Mathematical economics and rigor

The other important personality at Berkeley was Griffith C. Evans.<sup>59</sup> Just as in Neyman’s case, Klein “didn’t study with him,” but he did work “with people that were his students” (Klein and Mariano 1987, 410). With hindsight, “it is not unreasonable [...] to see [...] Klein as linked to [...] Evans” (Weintraub 2002, 71) and to argue that Evans’s image of mathematics passed on to Klein, influencing his practices as a macroeconometrician.<sup>60</sup> In order to understand the kind of image of mathematics that Evans transmitted to Klein it is important to locate his position within the landscape of mathematics.

E. Roy Weintraub (2002, chapter 2) identifies Evans with a tradition in mathematics that presents a close relation with application, going back to the Italian mathematician Vito Volterra and the French polymath Henri Poincaré, among others. For this tradition, “the kinds of values that a mathematician ought to exhibit in his work” were “not just a mathematical sophistication and power of analytical reasoning but a deep and thorough understanding of the

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<sup>58</sup> Zvi Griliches (see Griliches 2000) and Arnold Zellner (see Rossi and Zellner 1989) also remember Kuznets as an important figure in their careers. Yet, besides a few mentions, there is not much information about the influence that Kuznets actually exerted on these econometricians in terms of their econometric practice. There is no account either that relates or compares the quantitative work of the two Kuznets brothers, which seems to be quite different at a first glance. While George was very enthusiastic about the development in econometrics at Cowles, Simon worked at the NBER.

<sup>59</sup> Griffith Conrad Evans was born in Boston on 11 May 1887, and died on 8 December 1973. He studied mathematics at Harvard where he completed his BA (1907), MA (1908), and PhD (1910). After completing his PhD, he spent two years in Rome working with the mathematician Vito Volterra, returning to the United States in 1912 to join the faculty of the Rice Institute (now Rice University) in Houston, Texas, where he stayed until 1934. Since 1930, Evans played an important role in the creation of the Econometric Society. During his last twenty years of his active academic career (1934-1954) he became chair of the Department of Mathematics at the University of California, Berkeley. For a detailed account of Evan’s contributions and life see Weintraub (1998a; 2002, chapter 2), Duarte (2016), and Simon (2008).

<sup>60</sup> Here the notion of “image” refers to Leo Corry’s (1989; 2004) framework of image and body of knowledge as used in Weintraub (2002). The image of knowledge of a discipline is formed by a set of “second-order questions” concerning the methodology, philosophy, history, or sociology of any particular discipline.

scientific basis and connection of those mathematical ideas.” Scientific reasoning, then, should not be based on “the free play of ideas, or axioms, or abstract structures,” but “directly and specifically on the underlying physical reality,” which would be “directly apprehended through experimentation and observation” being “thus interpersonally confirmable” (48).

These visions are definitively consistent with Klein’s own idea of the use of mathematical tools and statistics.<sup>61</sup> While Klein placed more faith on the introduction of a broader type of analysis to improve his econometric results which included analysis of the “data base, economic analysis (both institutional as well as theoretical), political insight, and attention to the steady flow of information” (Klein 1991, 113-114), he sustained that “the adoption of more powerful methods of mathematical statistics [was] no panacea” (Klein 1960, 867). To Klein, “if econometric results are today more useful than in the past, this is only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature.” In addition, Klein expected “marginal improvements of [only] five or ten per cent through the use of more powerful methods of statistical inference” (867).

This vision on the use of mathematics was strongly related with an idea about mathematical rigor, where “the mathematical models are not free but are rather tightly constrained by the natural phenomena themselves” (Weintraub 2002, 70). Both in Evans’s and Klein’s understanding, rigor was not provided by abstract ideas, or axiomatization, but by the constraints imposed by the real phenomena themselves, which molded the mathematical models. Weintraub (2002, 71) refers to this kind of rigor as “materialist-reductionist quantification.”

As noted by Weintraub, “Evans’s views on mathematical modeling are the views of an econometrician or applied economist today,” or those of “one who insists that the assumptions and conclusions of an economic model [...] must be measurable or quantifiable” (70). Trygve

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<sup>61</sup> See chapter 4 for an account on Klein’s image of econometrics.

Haavelmo also praised a kind of rigor consistent with this vision. To Haavelmo, one of the leading figures of the Cowles Commission and a close friend and collaborator of Klein’s, the use of mathematics must be related both to practical application and to observed real-world phenomena, and so “contrary to what many people seem to think, it is in the practical application of theories to facts, in attempts to draw conclusions on the concrete level, that the need for stringent logic and fancy mathematics really shows up” (Haavelmo 1958, 352).

To these mathematical economists, however, the use of mathematics in economics had to be carried out with caution. To be clear, rigor, in Evans’s, Haavelmo’s, and Klein’s sense, must guide the use of mathematics in economics, and so the use of mathematics had to be based on the observed and studied reality. But the researcher had to go beyond the mathematics, for once economic theories are expressed in those terms, they might become rigid structures that do not let any new classes of phenomena enter the minds of the researcher, diminishing his imaginative and creative capacities. Evans (1930, 110) was conscious of this problem, and so he warned economists of the dangers of the use of mathematics to form economic theory:

General principles are apparent in the particular phenomena which we have studied, or at least, there are some general methods, which we can make use of in unifying those separate treatments. Nevertheless we must adopt a cautious attitude towards comprehensive theories. They do of course, in their special applications, suggest the treatment of particular problems, as well as classify them. Yet this comprehensive character, which they may have as sorts of inductive syntheses of previously studied situations, may precisely in that way circumscribe our ideas, and prevent from entering our minds the observation of other classes of phenomena. We may thus consider only one part of our subject, while we are under the impression that our study is general.

Evans was an “end-of-the-nineteenth-century rationalist, a Harvard pragmatist who [believed] in reason with a human face, and man’s capacity to understand the world in which he [lived]” (Weintraub 2002, 53). Apart from the “end-of-the-nineteenth-century” and the “Harvard” bits, Klein was very much like Evans.<sup>62</sup> He considered the highly sophisticated

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<sup>62</sup> Although Klein did not go to Harvard, he maintained a close relation to that institution during his

mathematical and statistical methods that he used throughout his career only as a rational way to understand the world, but never as the ultimate or infallible tool. Despite his sophisticated models and methods, reason had always a human face. Reason was not mechanical, but it was always an expert (whether an individual or an institution) who should direct the construction of the large-scale macroeconomic models based on knowledge that goes beyond mathematics and statistics; and it was also the expert and his team who should discuss and interpret the models' results both to adjust them through reasoned tinkering, and to make context-dependent policy recommendations. This knowledge that Klein and Haavelmo called "a priori knowledge," contained as well a great deal of economic theory, and a broader understanding about the institutional and historical arrangements of the economy. Imagination and creativity also played an important role in this heuristic practice of building macroeconomic models.<sup>63</sup>

The other figure at Berkeley that exerted important influence on Klein, was Francis W. Dresch, who, like Kenneth May, Charles F. Roos, and Ronald Shephard, made part of a group of former Evans's students (Simon 2008, 2).<sup>64</sup> Dresch was a mathematician with a special interest in economics, and econometrics. In 1937, still a Berkeley graduate student under Evans, Dresch went to Cambridge "hoping to study economics with John Maynard Keynes," and ended up playing a paramount role in Neyman's recruitment from University College London to California, acting as the connection between Evans and the Polish world-famous statistician (Reid 1998, 146). Almost "as a kind of bribe," Evans promised Dresch that he would

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period at MIT as I comment later, in particular with important figures such as Edwin B. Wilson and Alvin Hansen.

<sup>63</sup> Chapters 3 and 4 discuss both the limitations of the macroeconomic models under the light of the Keynes-Tinbergen controversy, and Klein's and Haavelmo's use of a priori knowledge to overcome the limitations imposed by the use of mathematical and statistical methods in economics.

<sup>64</sup> Dresch was born in Sharon, PA, in 1913, and died in 2011. He received his BA in 1932, and his MA in 1932 in mathematics from Stanford University. In 1937, he received his PhD from University of California, Berkeley, and spent a year in Europe at Cambridge University and the University of Paris between 1937 and 1938.

name him Neyman’s assistant, if he was able to convince Neyman to come to Berkeley (Dresch in Reid 1998, 154). Dresch not only secured a great job opportunity for himself, but he also helped Berkeley become one of the most important centers in mathematical statistics in the world through Neyman’s recruitment.

In 1938, the same year of Neyman’s arrival, Evans kept his promise and Dresch was appointed instructor in mathematics at UC Berkeley, a position he held until 1941. During these years, Dresch closely followed the development of the Econometric Society, and participated actively at the Research Conferences of the Cowles Commission (Dresch 1939).<sup>65</sup> In addition, Dresch worked on problems of aggregation in economics (Dresch 1937; 1950), which rapidly became central problems to Klein as well. As Robert Solow put it, Dresch “had worked out some nice properties of Divisia indexes in general equilibrium models” (Solow 1983, 7), and Klein (1946c; 1947a; 1947b) referred several times to Dresch’s work, although he found it “unsatisfactory, because the Divisia indexes lack the analogue property that a macrovariable should be a function of ‘corresponding’ microvariables” (Solow 1983, 7).<sup>66</sup> In any case, Dresch was a junior faculty member, Evans’s *protégé*, a key figure in Neyman’s recruitment, and Neyman’s assistant, and so, the only person able to establish a firm link between Klein and the important Berkeley figures in mathematics and statistics.

### 2.3. Becoming technical at MIT: Samuelson, the Statistics Seminar, and Wilson, 1942-1944

After two years at Berkeley, Klein was launched into a career of mathematical and statistical economics. His contact and collaboration with Kuznets, Neyman, Evans and Dresch literarily opened him the doors of MIT, capturing the attention of the young professor Paul A. Samuelson:

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<sup>65</sup> Dresch participated in the Cowles Commission Research Conferences of 1939 and 1940.

<sup>66</sup> In his 1925 book *L’Indice monétaire et la théorie de la monnaie*, François Divisia (1889-1964) developed a continuous-time index able to take into account the changes affecting the structure of the economy. For a detailed account of the Divisia index see Hulten (2008).

In my correspondence with faculty and staff in Cambridge, I found that my work as a research assistant to members of the Berkeley faculty, especially in mathematical statistics and in mathematical economics had been of interest to Paul Samuelson. He knew, by reputation or personal contact, about their interests in and direct contribution to new trends in economics, taking the subject into more intensive use of mathematics and statistics. He was interested in the work of Berkeley professors Francis Dresch (mathematical economics and statistics) and Jerzy Neyman (mathematical statistics) (Klein 2011, 502-503).

Yet, neither Neyman, nor Evans or Dresch, who were professors in the departments of statistics and economics, provided letters of recommendation for Klein. It was Norman S. Buchanan, R. Aaron Gordon and William J. Fellner, from the economics department, who supported Klein's application for MIT providing letters of recommendation.<sup>67</sup> According to Samuelson, they all agreed that Klein "was one of the best undergraduates they had ever had." (Samuelson, letter to Alice Bourneuf, Board of Governors of the Federal Reserve System on September 12 1944, PASP box 45). Klein's passing through MIT was paramount in his sharpening of mathematical and statistical techniques, but also in his consolidation as one of the rising economists of the early 1940s.

### 2.3.1. Draft status 4-F: Klein's scholarship at the new MIT program

In the late Spring of 1942, while "straightening a desk [...] in the main room of the economics facilities" at Berkeley, Klein "noticed a group of new announcements of economics faculty attractions at the Massachusetts Institute of Technology" (Klein 2011, 502), and felt immediately attracted to the new program. At that time, Klein had been attending government recruitment meetings to resolve his military situation, and had been almost resigned about accepting "a secondary job in the U.S. government," which would have probably consisted on a "desk job" at the military. Due to a childhood accident, Klein's military draft status was 4-F, "disabled," and so the new program at MIT opened up a fantastic opportunity for him after

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<sup>67</sup> Klein had assisted Fellner in his "Treatise on Wartime Inflation: present policies and future tendencies in the United States" published by the University of California in 1942.

“the government recruiters kept stressing that [he] could be an expensive disability to the government” (502).<sup>68</sup>

Klein arrived in Cambridge, MA, in September 1942, one year after MIT had started a graduate program that would become one of the top programs in economics by the 1960s (see Weintraub 2014a). In fact, the department of social sciences had been focused on providing teaching for engineers until 1940. Klein made part of the second entering class of the new graduate program of economics at MIT, and became the first economics PhD recipient on October 9, 1944 (Klein 1991b; Duarte 2014).<sup>69</sup> The whole MIT had experienced important changes of restructuration (Backhouse 2014a), passing from an “undergraduate engineering school to [a] full-fledged research university” (75) during the 1930s. The economics department, however, lagged some years behind and only embarked in this transformation in 1940. MIT’s inauguration of a new graduate program in economics (Duarte 2014), its openness to Jews (Weintraub 2014b) at a time when anti-Semitism “was woven into the fabric of academic institutions” (Backhouse 2014, 73) and when Harvard was clearly anti-Semite (Backhouse 2015, 74; Weintraub 2014b), and the development of a more technical way of doing economics (Cherrier 2014), marked the rise of MIT economics.

W. Ruppert McLaurin was instrumental in the transformation of the department and in helping Harold Freeman hire Samuelson (see Backhouse and Maas 2016).<sup>70</sup> Only 25 years old and with a PhD earned at Harvard in the field of “mathematical economics,” Samuelson was a very good fit for MIT’s economics department given that every student there was required to

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<sup>68</sup> Klein’s “right leg was two inches longer than [his] left leg, as a result of an automobile accident” (Klein, letter to E. R. Weintraub, October 11, 2010, LRKP, box 30).

<sup>69</sup> Duarte (2014) provides an account of the early history of the MIT graduate program in economics.

<sup>70</sup> Although Samuelson appears very often as the most visible figure in this story, Backhouse and Maas (2016, 424) have recently argued that the initial driving force “behind the transformation of the department, which grew in size and expanded the range of its activities” was McLaurin. The importance of this finding resides in the fact that MIT economics might have been developed in a less technical and more interdisciplinary way, if McLaurin’s project on innovation and technical change funded by the NBER had not failed.



study mathematics and physics. Samuelson proved his value contributing to the rise of MIT as one of the most important economics departments in the country. Yet in the early 1940s, Samuelson was reluctant to accept MIT's offer because he wanted to stay in the, by the time, stronger department of economics at Harvard (Backhouse 2014; 2015).<sup>71</sup>

This hiring process of promising economists was not consolidated until the end of the 1940s. First, Samuelson's own permanence at MIT was seriously threatened by tempting proposals from other universities at least until 1949. The most important of these proposals was led by Theodore Schultz in Chicago (see Maas 2014a), Samuelson's alma mater. Also, other important and representative figures of MIT's transformation were not hired until the second half of the 1940s, a few years after Klein had left the Institute. These enrolments included Cary Brown's (1947), Robert Bishop's (1949), Charles Kindleberger's (1948), Morris Adelman's (1948), and Robert Solow's (1949) (Cherrier 2014, 20). To be sure, the economics department at MIT that Klein integrated was still in a very embryonic stage. Yet, at a time where mathematical economics was still striving to become dominant and to be considered more than a narrow specialty in economics (Backhouse 1998), MIT seemed to be the perfect place for somebody like Klein to complete his PhD, above all because of Samuelson's presence.

By 1942, even if Samuelson and Freeman were responsible for the branch of mathematical statistics, the economics department was not able to teach advanced topics in mathematical statistics, but offered only general courses in this topic through the mathematics department (Klein 1991b, 320). Together with his MIT classmates, and especially with Joseph Ullman, Klein "felt the need for extra knowledge about mathematical statistics" and decided to organize a series of seminars with external speakers. This is how the Statistics Seminar came into being between 1942 and 1943 (Klein 1991b; Bjerkholt 2013, 768-769).<sup>72</sup> It was at the occasion of this

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<sup>71</sup> For a comprehensive account of Samuelson's move to MIT see (Backhouse 2014).

<sup>72</sup> A complete list of the speakers with abstracts of their presentations is available in Klein (1991b). The speakers included: Kenneth J. Arnold on "Spherical Probability," Albert H. Bowker on "Enumeration of Latin Squares," William Feller on "Stochastic Processes," Harold A. Freeman on "Bayes' Theorem

seminar that Klein met Haavelmo for the first time, in 1943. This was the beginning of a fruitful and friendly relationship, further cultivated between 1946 and 1948, when they overlapped in Chicago as research assistants for the Cowles Commission, and during the year Klein spent in Oslo, Norway, after his period at Cowles.<sup>73</sup>

### 2.3.2. From “Prof. Samuelson” and “assistant Klein,” to “Paul” and “Laurie”

Klein was assigned assistant to Samuelson, presumably because Samuelson, impressed by Klein’s references and work in mathematical and statistical economics, insisted on keeping him close. To Klein (1980), in any case, “working as an assistant for Samuelson was something that is very hard to duplicate anywhere in the world,” because “he generates ideas so fast [...] It was a very exciting time” characterized by “a whole succession of ideas concerning Keynesian macroeconomics and econometrics and the development of mathematical methods in economics [...] and [Klein] felt very fortunate to be in that background” (411).

When Klein arrived at MIT, Samuelson was very busy working on important consultancy projects, including one for the National Resources Planning Board (NRPB) (Maas 2014a, 279-282). According to Samuelson, “to a surprising degree, [Klein had] been able to go ahead on his own steam in these disorganizing war years” (Samuelson in a letter to Marschak, dated October 28, 1944, PASP box 45). Yet, despite Samuelson’s multiple engagements, Klein (2011) remembers that “Samuelson interacted closely with graduate students on a larger and larger scale, playing (poker) card games together and getting some professional papers written” (505). The difference of only five years might have facilitated this friendly relationship between

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and Testing Hypotheses,” Trygve Haavelmo on “Some Problems of Statistical Inference Arising in Econometrics,” Klein himself on “Neyman’s Smooth Test for Goodness of Fit,” Paul A. Samuelson on “Gram-Charlier Series,” Dirk J. Struik on “The Foundations of the Theory of Probabilities,” Richard von Mises (Ludwig’s brother) on “The Probability of Occupancy,” Abraham Wald on “A Problem of Multivariate Analysis,” Norbert Wiener on “Ergodic Theory,” and Edwin B. Wilson on “Contingency Tables.”

<sup>73</sup> Klein joined Cowles on November 1944 and stayed there until September 1947. Haavelmo joined Cowles in early 1946 and returned to Oslo in the Fall of 1947.

Samuelson and Klein, despite Samuelson's Chicagoan methodology (perhaps adopted from Jacob Viner) of "suddenly [directing] attention to a relevant question, asking for more complete information from a single student, who might be well prepared in Samuelson's approach, and who should have been prepared to elaborate on the implied questions in his statement." This approach "made many students uncomfortable and often frightened that they would be singled out as unprepared," driving "students to careful and detailed [and collective] study before class" (504).

Klein might have inherited Samuelson's idea of "becoming a technical expert" in which the model, and not the economist himself, would provide the impression of being the one who "speaks" and makes recommendations.<sup>74</sup> In his *Economic Fluctuations in the United States economy, 1921-1941*, for instance, Klein (1950) showed that "it [was] possible to develop [the same macroeconomic model] from the un-Marxian principles of utility and profit maximization, but [...] also [...] from purely Marxian principles" (see chapter 4). According to Klein, then, "the same model can be consistent with a multiplicity of hypotheses" (63-64). As noted by Maas (2014a), to Samuelson, "technicality implied impartiality and detachment" (273). Furthermore, "emphasizing the operational significance of economic theory" provided "another way to defend [...] ideological neutrality" (276). For instance, while "Samuelson presented his Keynesian message not as a policy creed but as a technical assessment" (286), Klein's advocacy of Keynesian theory and policy during the 1940s can also be seen as a result of his technical work rather than of his political agenda. At the time, indeed, Klein had Marxist political inclinations as well as a quite critical position about the Keynesian approach. Econometrics, however, provided "tools of analysis suited for economic policy that are, as much as possible, independent of the personal judgements of a particular investigator." In fact, "econometric

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<sup>74</sup> In particular, Klein (1947a; 1950) are examples of this, as I explain in chapter 4.

models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims” (Klein 1947a, 111).

Apart from their affinity on the subject of doing “technical economics,” time and again, Samuelson repeated that Klein had been one of his best (if not the best) students, and that he had set the bar too high for the future generations of MIT students in economics. “Often in public lectures I’ve [Samuelson] had to say that, if MIT pursued the maximand *average* quality of our PhD graduates, we’d need to have stopped with Lawrence Klein our first graduate!” (Samuelson, letter to LRK, May 22 1985. PASP, box 45). Samuelson also insisted on how “we at MIT have always appreciated the key role you [Laurie] played in getting our graduate program off the ground. You were not only a first PhD, but also a first Nobel” (Samuelson, letter to LRK 21 June, 2005. PASP, box 45).

During his two years at MIT, Klein also published his first important papers. The first in *Econometrica*, Klein (1943), and the second in *The Quarterly Journal of Economics*, Klein (1944b).<sup>75</sup> These papers reflected not only the technicality of Klein’s economics already at the time, but also the sophistication and sagacity of his analysis of broader questions of economic policy and political reform. In the first paper, “Pitfalls in the Statistical Determination of the Investment Schedule” Klein (1943) engaged in a controversy with Mordecai Ezekiel on the estimation of future investment.<sup>76</sup> He argued that, in general, future investment was estimated “by means of a regression equation relating investment to income, a trend variable, and [...] a variable which

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<sup>75</sup> From that point onwards, and until the end of the period studied in this dissertation, 1959, Klein published an impressive amount of more than twenty papers in top journals like *Econometrica*, *The Journal of Political Economy*, *The American Economic Review*, *The Review of Economics and Statistics*, and some twenty more in other journals.

<sup>76</sup> Ezekiel (1899-1974), was a well-established agricultural economist, who had occupied important governmental positions during the 1930s at the Federal Farm Board, the Secretary of Agriculture, and the Bureau of Agriculture Economics, to mention only a few. He was born in Richmond, Virginia, and had earned his BA in Agriculture from the Maryland Agricultural College in 1912, his MA from the University of Minnesota in 1923, and his PhD from the Robert Brookings Graduate School of Economics and Government in 1926.

introduces a lagged income effect” (246).<sup>77</sup> According to Klein, however, this type of estimation could entail a serious problem of identification, since the “observed data on savings, investment, and income are [...] the co-ordinates of the intersection of [the statistical savings schedule and the statistical investment schedule]” (246). Instead of estimating these curves using “classical regression methods” like the method of least squares, Klein proposed a “much more elegant approach” (251) following Haavelmo’s (1941; 1943) probability approach to econometrics. The second paper, “The Cost of a ‘Beveridge Plan’ in the United States” (Klein 1944b), provided an important contribution to the postwar question of “how much a full social insurance and assistance program [would] cost” (423) for the United States, showing, as Samuelson put it, that “his feet were on the ground and not in the clouds.”<sup>78</sup> Inspired in the British Beveridge Plan, Klein calculated, indeed, what such a plan would cost if implemented in the US for the years 1945-1965. Even if he recognized that the discussion of other plans including the “Wagner-Murray-Dingell bill and the Security, Work and Relief Policies Report of the National Resources Planning Board may be thought to be more relevant at the moment,” he considered that the Beveridge scheme was “so polished and simple that it can well serve as a model for postwar planning in many countries,” since it is “more comprehensive than any of the American plans [...] more specific than the National Resources Planning Board’s program [and] more finished in structure than the Wagner bill” (423).

The other important work that Klein produced during his stay at MIT was his PhD dissertation (Klein 1944a). According to Samuelson (1995, 3), it was Klein (1944a) who coined the term “Keynesian Revolution,” which was published with the same title three years after its

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<sup>77</sup> In particular, Klein criticized Ezekiel (1942).

<sup>78</sup> Samuelson to Alice Bourneuf, September 12, 1944, PASP, box 45.

completion, in 1947.<sup>79</sup> <sup>80</sup> Although Klein’s dissertation included several mathematical models of Keynes’s different works, the piece could be considered a contribution in the history of economic thought and economic methodology. Contrarily to what one could imagine beforehand, Klein approached Keynes’s theory and methodology from a very critical (Marxian) point of view, stating for instance, that “Keynes did not really understand what he had written, and chose the wrong thing to publicize as his innovation” (Klein 1947b, 83), i.e. that wage rigidities and market imperfections provided the explanation for the existence of unemployment. Instead, Klein thought that Keynes’s innovation was the rejection of the classical theory of interest, and his contribution to the multiplier theory and the theory of the determination of effective demand (86). Another example not only of Klein’s critical tone towards Keynes, but also of his enthusiasm for Marx’s theories, and for social reform and economic planning is to be found in chapter VII “Keynes and Social Reform,” added as the last chapter in the 1947 published version.<sup>81</sup> There, Klein argued that “our program of social reform must continue even after we have solved the problem of unemployment.” Yet, even if “Keynesian economics gives us a set of tools with which to work on the unemployment problem, [...] it does not deal at all with many other important socio-economic questions that also deserve a large share of our attention and study” (186).<sup>82</sup>

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<sup>79</sup> Throughout their lives, both Klein and Samuelson always expressed words of mutual admiration, respect and friendship. Yet, despite a life-lasting friendship maybe also boosted by the small age difference of only five years, their correspondence gives the impression that a certain hierarchy remained, where Samuelson never ceased to be the professor and Klein the student.

<sup>80</sup> To Klein, it was clear that the term “Keynesian Revolution” had been used by “a number of economists” especially in England, but also in Western Europe and in the United States, where “interesting books and articles appeared with [this] title” (Klein 2011, 506).

<sup>81</sup> Note that apart from the PhD version, there were two different editions of Klein’s *Keynesian Revolution*. The first was published in 1947, and the second in 1966. For the second edition, Klein added two additional chapters: “The Keynesian Revolution Revisited,” and “The Econometrics of the *General Theory*.”

<sup>82</sup> Klein’s Marxian inclinations were also source of debate with Samuelson. In a letter dated November 2, 1945 in which Klein talked about his year at Cowles and in particular about his aggregate macroeconomic model of the US economy, Klein asked Samuelson (presumably as a way of teasing him) whether he “would know how to get in touch with Peter Elias [since Klein had] his three volumes of Marx’s *Capital*, which [he had] been reading from time to time” (PASP, box 45). Yet, at the occasion of the publication of the second edition of *The Keynesian Revolution* in 1961, although with a few years of

### 2.3.3. Cambridge connections: the influence of Wilson's views on science and economics

Another important figure for Klein during his years as an MIT student was Edwin B. Wilson.<sup>83</sup> Although professor of mathematics at Harvard, Wilson kept a strong and close relation to economics and to MIT, particularly through Samuelson who “encouraged [his students] to visit Wilson, [and] urged [them] to learn how Wilson's views on economic analysis were related to his own” (Klein 2011, 504-505).<sup>84</sup> Klein, in fact, not only took one of Wilson's courses at Harvard, but made such an impression on Wilson that he suggested Samuelson to support Klein for election at the Harvard Society of Fellows (Wilson, letter to Samuelson, 12 April, 1943, PASP box 77).

Wilson was “not strictly an economist, but an older scholar with wide interests in many subjects in science and in economics as well as in higher education in general, either in the direct pedagogical sense or in terms of academic influence in general” (Klein 2011, 504-505). His ultimate purpose in economics was to see “economic thinking better controlled by analysis of the facts of the economic world, and the facts themselves better collected under control by economic thinking,” promoting a pedagogical approach to explain applied mathematics in economics and, especially, econometrics under the light of “some important, particular, concrete problem” (Wilson 1946, 173). Although sympathetic to the econometric project in

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delay, Klein sent the latest edition to Samuelson whom in a letter dated July 1, 1966 expressed his gratitude “for the new edition. It is a classic, and with two new chapters it is two-sevenths more valuable” (PASP, box 45).

<sup>83</sup> Edwin Bidwell Wilson was born in Hartford, Connecticut on April 25, 1879. He was considered a polymath, for his knowledge in physics, mathematics, statistics and economics. He graduated *summa cum laude* from Harvard with a major in mathematics in 1899, and went to Yale University where he completed his PhD in 1901. After a one-year leave to the *École Normale Supérieure* to study mathematics between 1902-1903, he came back to the United States and to Yale where he became an assistant professor in 1906. In 1907, he left to MIT where he became full professor of mathematics and physics in 1911, and head of the department of physics in 1917. In 1923, Wilson moved to the Harvard School of Public Health and became professor of Vital Statistics, retiring in 1945. See Hunsaker and Mac Lane (1973) for a biographical essay on Wilson. For an account on E. B. Wilson and his relation with Samuelson see Backhouse (2014) and Carvajalino (2017).

<sup>84</sup> For an account of the influence of Wilson on Samuelson's work, especially on his 1947 book *Foundations of Economic Analysis*, see Carvajalino (2017).

general and to Haavelmo’s ultimate objective in particular, Wilson wrote a quite harsh review of Haavelmo (1944), in which he criticized the “extremely abstract and metaphysical” approach of the Norwegian, and his emphasis on “ideal worlds” and “hypothetical or abstract illustrations” in his econometrics. Instead, Wilson preferred that econometricians focus on the application of mathematics and statistics to relevant and concrete problems using simpler methods, which had “satisfied [James C.] Maxwell, [Ludwig] Boltzmann, [Willard] Gibbs, and [James] Jeans as a basis of their work on the theory of gases and statistical mechanics” (173). According to Wilson,

there is a small group of econometricians who are well trained in mathematics and who apparently choose to write for one another rather than for economists (or even econometricians) in general. I believe they have something important to say – important not alone for the further development of a purely mathematical dialectic but for economics in the large. Furthermore, I believe that not much will be accomplished by them in the development of economics until and unless economists in general can understand why their contributions are important (173).<sup>85</sup>

Towards the end of 1946, Wilson also wrote two letters to Milton Friedman after reading his review of Oskar Lange’s (1944) *Price Flexibility and Employment*.<sup>86</sup> Although Wilson agreed with Friedman in his criticism of Lange’s abstract approach, which emphasized on the formal structure of economic theory and was deprived of sound empirical work, Wilson did not think that providing “these expositions of what gets us nowhere” was the best thing to do to improve economics. Rather, he thought that it was important to get “exhibits of what gets us somewhere” (Wilson, letter to Friedman, November 24, 1946, MFP). Therefore, Wilson asked Friedman to provide him with a list of “jobs in economics which [he thought] thoroughly good in the way contrary to the bad way of Lange.” Friedman provided a list of five books that

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<sup>85</sup> As I show in chapter 4, Klein too was critical of the highly abstract and sophisticated methods promoted by his colleagues at the Cowles Commission, and advocated for the application of mathematical and statistical methods to concrete economic problems.

<sup>86</sup> I will come back to Friedman’s (1946) criticism of Lange’s book in the second part of my dissertation (in chapter 5), when I study the longstanding discussion between Friedman and the members of the Cowles Commission.



included:

1. W. C. Mitchell *Business Cycles* (1913) in contrast to the 1927 book which is pretty neutral.
2. A. F. Burns and W. C. Michell, *Measuring Business Cycles*, National Bureau of Economic Research (1946).
3. A. F. Burns, *Production Trends in the United States*, National Bureau of Economic Research.
4. Jacob Viner, *Canada's Balance of International Indebtedness, 1900–1913* (1924).
5. J. M. Clark, *Economics of Overhead Costs*.

Friedman considered these books to be “good work in economics,” primarily because they embodied “the appropriate methodological approach in respect to the combination of empirical and theoretical analysis.”<sup>87</sup> Apart from the important discussion about why Friedman considered these books to be “good work” in economics, the letters expose both Wilson’s image of science and some of his positions concerning what good social science and economics should be. For example, to Wilson “the great difference between the natural & social scientists [...] lies in the fact that the natural scientist suppresses promptly the errors of his fellows, generally before they get into print, whereas the social scientists tolerate the sloppiest sort of stuff.” In addition, while the natural scientist, only interested in getting more knowledge, considers methodology as “a pastime or occupation of age, [...] the social scientist’s interest when young in methodology is an evidence of the continuing pernicious influence of moral philosophy & philosophy and the meager influence of logic and of facts in the field of the social sciences.” Finally, to Wilson “theory is very impermanent; it is just one method of classifying the facts.” Note that this consideration of theory as adaptable and as providing a system of thought to understand facts, goes well in line not only with Friedman’s criticism of Lange, but also with Klein’s way of building macroeconomic models. To Wilson, “there is a great deal of

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<sup>87</sup> I will come back to Friedman’s notion of the appropriate methodological approach in the second part of my dissertation.

pretentiousness about theorists that is lacking in scientists – [Albert] Einstein is a theorist (almost exclusively) whereas [Albert] Michelson was a student of phenomena (almost exclusively). [Léon] Walras was much more the theorist, [Adolphe] Quetelet much more the naturalist, I would not exclude theorists from the group of scientists, [Thomas A.] Edison & Einstein are alike scientific physicists but Edison’s discoveries may well outlast Einstein’s.”

In short, Wilson’s influence on Klein can be understood in two ways. First, in an indirect way that goes through Samuelson, and second, in a direct way going through Wilson himself. In any case, Wilson’s influence on Klein helped bringing a new air to the Cowles Commission, which contrasted with both the abstract approach led by Lange, and the econometric theory developed at Cowles, where statistical and economic theory, and not empirical work, occupied the most privileged place. This “new air” can also be understood as introducing an image of mathematics that was closer to a more pragmatic and applied US American tradition of mathematics.

#### 2.4. Redoing Tinbergen’s macroeconometric model at the Cowles Commission, 1944-1947

After two years in Cambridge, Klein finally handed in his dissertation on October 1944, eager to get a good position on the job market. Then like now, however, going in the job market was a hard experience, especially for mathematical economists and econometricians who still “operated in an academic underground [where] job opportunities were scarce [and] post graduate scholarships were not abundant or generous” (Klein 1991a, 112). Samuelson was concerned with the situation of his first PhD graduate, and made an important effort contacting several people to ask for the possibility of an available opening for this “very promising, able, young economist” (Samuelson to Marschak, October 28, 1944, PASP box 45).

2.4.1. A meager job market for mathematical economists and econometricians

On September 12, 1944, Samuelson sent a letter to Alice Bourneuf of the Board of Governors of the Federal Reserve System, telling her that he had “an excellent person [...] well trained in statistics, but unlike most statisticians [...] also very well trained in economics [...] who might be interested in [taking a job] in international economics.” One week later, Samuelson also reached out to Howard S. Ellis, insisting on his idea of having Klein recruited by the FED.<sup>88</sup> Despite the enthusiastic responses on Klein’s profile, nothing definitive came out of this correspondence in terms of real recruiting options.

Quite confident about the good impression that his former professors had certainly retained from Klein, Samuelson contacted Berkeley professors Norman S. Buchanan, whom, he believed, “knew [Klein] best at California,” and Joe S. Bain, to whom he “heartedly recommended [Klein]” as “really a first-class man.” In his letter to Buchanan, Samuelson explained that Klein’s work “might be considered to be equally in the fields of business cycles, theory, and money and banking,” and that he was “extremely well grounded in mathematical statistics.” Indeed, Samuelson insisted that if “the work of a man like Neyman is not well integrated with that of the Department of Economics, Klein could be useful in this sector,” and that “although he [had] not done much work in the field of mathematical economics, he could certainly build up a fruitful liaison with the work of Evans and his students in the Mathematics Department.” Samuelson emphasized, however, that Klein was “primarily, [...] an economist, and a good one.” There is no evidence of a response of Bain or Buchanan, but Prof. Malcolm M. Davisson, chair of the Department of Economics at Berkeley, contacted Samuelson on October 8.<sup>89</sup> Davisson explained to Samuelson that “while [he was] not at this time in a position

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<sup>88</sup> Howard S. Ellis earned his PhD in Harvard in 1929. As a faculty member at UC Berkeley from 1938 to 1965, Ellis was president of the American Economic Association in 1949. In 1944, however, he was focused on advising the FED, rather than on “academic issues.” Most of Ellis’s work was on monetary and macroeconomic issues. For an account of Ellis’s work during the 1940s see Herren (2001).

<sup>89</sup> Davisson had earned his BA (1928) and first MA (1929) from the University of California, and a second

to make a definite offer, [he was] interested in Klein,” and that he “had for some time been considering the possibility of adding someone to [the] staff who could work on a cooperative basis with Evans and certain other members of the Mathematics Department.” This response, which opened a real possibility for Klein, was, at last, encouraging.

Davisson asked Samuelson for additional information about Klein’s teaching ability, about the possible development of his research interests, and, more specifically, about the possibility of Klein working in the field of insurance. He explained to Samuelson that “Professor [Albert Henry] Mowbray, who for many years [had] carried most of the work in statistics and insurance, [was] within a few years from retirement,” and that he was “interested, therefore, in someone who might either devote a large part of his time to work in the field of statistics [...] or who would be able occasionally [...] to offer undergraduate or graduate work in this field” (Davisson to Samuelson, October 7, 1944, PASP box 45). Samuelson’s response to Davisson’s inquiries was plain and honest. According to him, Klein would “continue to have interests in statistics, mathematical or otherwise, all his life,” but he expressed serious doubts about Klein being “interested in teaching actuarial mathematics and insurance more than very occasionally” (Samuelson to Davisson, October 17, 1944, PASP, box 45).<sup>90</sup> In any case, Samuelson asked Ralph E. Freeman and Douglas W. Brown to send their impressions about Klein’s ability to teach. Even if there were a number of favorable conditions at UC Berkeley for Klein to pursue a career as an econometrician in California (in particular with the presence of people like Evans and Neyman), it was clear that the Economics Department was rather interested in getting somebody with particular abilities to teach, and whose research was centered in a very specific area of actuarial mathematics and insurance theory. Despite

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MA (1930) and his PhD (1931) from Harvard.

<sup>90</sup> Concerning Klein’s ability to teach, Samuelson responded that he had “had no contact with Klein’s undergraduate teaching in economic principles.” He went on to say that he would fail to declare “that [Klein was] the best teacher of his generation” and say instead that “he is as good or better than the average.”

Samuelson's effort, it seemed that neither Klein was the right person for this position, nor was Berkeley (other than as a student) the right place to be for him.

Fortunately for Klein, getting a job did not depend only on his supervisor's direct efforts. Klein submitted his "thesis paper" for presentation at the first postwar meeting of the Econometric Society in Cleveland on September 13-15, which "was accepted mainly because Paul Samuelson was the thesis supervisor" (Klein, letter to E. R. Weintraub, 11 October 2010, LRKP, box 30). There, he presented a paper in a session chaired by Jacob Marschak (Bjerkholt 2014, 769).<sup>91</sup> Marschak had been research director of the Cowles Commission since January 1943, and was assembling a team to embark in a very ambitious research program. It was during that session that Marschak pronounced his famously sentence to Klein that "what this country needs [...] is a new Tinbergen model, a fresher approach to it." The Tinbergen model Marschak referred to was the macroeconomic model that Tinbergen had prepared for his report to the League of Nations in the late 1930s. The "fresher approach to it" was the use of the latest advances in econometric theory not available six years before at the time when Tinbergen published his work, specifically Haavelmo's (1941; 1943; 1944) probability approach to econometrics. In order to raise Klein's enthusiasm about the project even more, Marschak also mentioned his plans to recruit Haavelmo for this project (Bjerkholt 2014, 769). Marschak's proposal "was so fascinating to [Klein] that [he] returned to Cambridge [...] and reported to Paul [Samuelson] about the first meetings, without the overhang of the war with Germany" (Klein, letter to E. R. Weintraub, 11 October 2010, LRKP, box 30).

More than a month after the meeting, Marschak wrote to Samuelson to tell him that he "was favorably impressed by Klein's article in *Econometrica*, and also by the paper he read at the Cleveland meeting" and that there was "a possibility of offering a job [at Cowles] on conditions which may satisfy [Klein]" (Marschak to Samuelson, October 25, 1944, PASP box 45).

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<sup>91</sup> The title of the paper was: "From the Treatise to the General Theory: A Study in Keynesian Economics." For a detailed account of Klein's recruitment at Cowles see Bjerkholt (2014).

Marschak also wanted Samuelson to confirm or contest his impression of Klein “as one of the best men of his age available for econometric work.” Although his letter was considerably shorter than the one he had written before for the Berkeley professors, Samuelson answered almost immediately (on October 28), appraising Klein as “a very promising, able, young economist with an excellent training” and “very well qualified to work on [Marschak’s project], most unusually so for a man of his age.” Marschak offered Klein a three-years position, with funding secured by the Commission for the first two years, and on the condition that Klein would have to apply for an SSRC fellowship for the last year. Klein accepted Marschak’s proposal “without any hesitation,” and “moved to Chicago in order to build an empirical system that could be used for extrapolation into the new peace time world” (Klein, letter to E. R. Weintraub, 11 October 2010, LRKP, box 30). Klein joined the Cowles Commission on November 21, 1944, and begun one of the most influential periods of his academic life.

#### 2.4.2. Klein, the new prodigy at Cowles

When Klein arrived in Chicago in 1944, the Cowles Commission was integrated by Marschack, Leonid Hurwicz, Tjalling C. Koopmans, and George Katona (who stayed at Cowles only until the end of the year), Joel Dean and Theodore O. Yntema (both on leave of absence during that year), Harold T. Davis, Dickson H. Leavens, H. Gregg Lewis, and Jacob L. Mosak. There were also two research assistants, Herman Rubin “a candidate for the SM in mathematics,” and Sami Tekiner, “a graduate student in economics and a Fellow in the Department of Economics.” (Cowles Commission Annual Report for 1944). Although their number is not specified in the Annual Report for 1944, the secretarial assistance and the “human computers” at Cowles were also a fundamental part of the Commission.<sup>92</sup> First, because this staff participated in the calculation of estimates and parameters for the macroeconometric models,

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<sup>92</sup> On the importance of the role played by “human computers” in institutions such as Cowles see Grier (2005).

and in the Cowles's seminars, taking notes of the discussions. Another reason for paying attention to the staff at Cowles is because Sonia Adelson, or Sonia Klein after 1947, was part of the staff.<sup>93</sup> In addition to this already excellent staff, Marschak had still plans to hire brilliant people such as Trygve Haavelmo (who finally arrived in 1946), Abraham Wald, and James Tobin, among others.<sup>94</sup> In any case, and apart from the two assistants Rubin and Tekiner and from the two Sarah Hutchison Fellows Adelson and Schweitzer, Klein, who had recently turned 24, was the youngest member of the Commission, and was hence its prodigy.

The positive impression that he had exerted on Marschak, Hurwicz, and Koopmans at the Econometric Society Meeting in 1944, was soon confirmed by Klein's work and personality. In a confidential statement written by Marschak to support Klein's application for the SSRC fellowship (quoted at length in Bjerkholt 2014a, 771-772), Marschak confirmed that: "my collaborators and myself have found in him a person prepared to understand and appreciate the other point of view; equally agreeable in giving and in taking; and more interested in having the problem solved than in winning the argument or making a career." In addition, Klein

has a good eye for the essential. His goal is a logically consistent explanation of observed facts. He will not try to escape into theoretical perfectionism (which tends to make economics logically complete and beautiful but unverifiable) or into empirical detail (substituting enumeration for explanation). The kind of study proposed by Klein requires a sound instinct for properly assigning each variable to one of the three groups: the 'economic' variables, whose interacting constitute the system in question; the identifiable 'external' variables which strongly affect the economic variables but are not significantly affected by them; and the non-identifiable external variables whose effects are visible only in aggregation, as 'random components' of the system. If this instinct fails, the hypotheses subjected to verification are either so incomplete as to lead to biased conclusions; or so pretentiously complete as to be unverifiable by the facts at our disposal. Klein seems to possess the necessary instincts: it will save him

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<sup>93</sup> In fact, between 1946 and 1947, Adelson was awarded, together with Selma Schweitzer, the Sarah Hutchinson Cowles Fellowship by the University of Chicago upon nomination by Cowles. Both Adelson and Schweitzer, were preparing for the degree of Master of Arts or Doctor in the fields of Social Sciences and statistics, and were working on quantitative economics or mathematical statistics.

<sup>94</sup> For an account of Marschak's recruitment ambitions for the Commission see Bjerkholt (2015). Cowles was never able to recruit Wald. On the contrary, Tobin became research director of the Commission in July 1, 1955, when Cowles moved from Chicago to Yale University.

much disappointment. Klein seems to be the type of man who will work overnight and over the week-end if the problem interests him.

The sketch set up by the staff members during a party in 1946 to represent a “mock trial of Klein on the grave charge of stealing into the Social Science building late at night and finagling with the data for his econometric model” (Christ 1956, 41) is an anecdote that reveals not only the collegiate atmosphere at Cowles, but also how central and new Klein’s work was for the rest of the Commission. As Christ recalls, “there were many witnesses and clever counsel played by various staff members, and it made delightful entertainment.” At the end of the trial, however, “Klein was acquitted of all wrongdoing” (41).

Yet, not all the relations in Chicago were as cordial and convivial, as those experienced within the Commission. Cowles had moved from Colorado Springs to Chicago only in 1939, encountering important resistance from the economics department of the University of Chicago. It is not hard to imagine that both institutions with such differences in their personnel, methodological approaches, theoretical stances, and ideological visions, engaged in a longstanding debate that marked the history of twentieth century economics. Yet, these encounters and the relation between the Cowles and Chicago were not always of conflict or “warfare” as some authors have said (Mirowski and Hands 1998, 268; Mirowski 2002, 245). In fact, these discussions produced decisive methodological debates that marked the practice of macroeconomic modeling. I return to this point in the second part of my dissertation.

#### 2.4.3. The student becomes the master: “No recession for the US postwar economy”?

On September 17, 1944, Samuelson published his *New Republic* column with a bold title: “Unemployment Ahead: The Coming Economic Crisis.” In that article, Samuelson stated that the remarkable governmental expenditures consisting on “pumping [the] millions and billions of dollars into the bloodstream of the American economy,” and comparable only with “building a TVA [Tennessee Valley Authority] every Tuesday,” would not be sustainable after the war



was over.<sup>95</sup> The United States had “reached the present high levels of output and employment only by means of \$100 billion of government expenditures, of which \$50 billion represent deficits.” To him, since “in the usual sense of the word, the present prosperity is ‘artificial,’ although no criticism [was] thereby implied [...] any simple statistical calculation [would] show that the automobile, aircraft, ship-building and electronics industry combined, comprising the fields with rosier postwar prospects, cannot possibly maintain their present level of employment, or one-half, or one-third of it” (Samuelson 1944). Samuelson, like most economists, was pessimistic about the economic situation after the war, foreseeing a new recession for the US economy in the years to come (see chapter 1).

Two months after Samuelson’s hardly surprising article, Klein arrived at Cowles and started working on the construction of his macroeconomic model of the US economy. He constructed his famous Models I, II, and III, which would later form the core of his 1950 book *Economic Fluctuations in the United States*.<sup>96</sup> As noted in chapter 1, Albert G. Hart (staff member at Cowles and research associate at the CED) was “getting indications” that instead of “a major postwar depression with ‘8 to 10 million unemployed’ [...] there might be high activity and inflationary pressure,” and wanted first to “confirm” and then to communicate these preliminary but optimistic results. Since Hart and his colleagues knew about Klein’s pioneering macroeconomic modeling project, “it occurred to [him] that it would be illuminating to see how far this model would confirm the existing professional consensus” on a postwar recession or not. “When [Klein] agreed to [Hart’s] request for a trial run [on an early version of his

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<sup>95</sup> The Tennessee Valley Authority (TVA) is a governmental agency created through the TVA-Act of 1933, by President Roosevelt in the context of the New Deal. The TVA constructed an enormous system of dams in the Tennessee river area to produce electricity, improve navigability, and control flooding. The agency itself, however, became a symbol for a new idea of progress, for the potential use of natural resources in the benefit of humans, as well as for the idea of economic planning (Black 1995).

<sup>96</sup> Model I was a six-equation model with three behavioral equations and three identities. This model “was used to analyse the stability of the American economy” (Bodkin et al. 1991, 43). I present Model I in its original form in chapter 4. Model II, consisted on a reduced form model with a consumption function and two identities. Whereas Model III was a “large structural macroeconomic” model with sixteen equations, twelve behavioral equations, and four identities.

Model III], [Klein] indicated that he tended to agree with the school that saw a great tendency to depression,” warning Hart, “moreover, that his model rested on experience under depressed conditions, and might be seen as biased toward predicting only variants of a depression” (Hart 1976, quoted in Bjerkholt 2014a, 772-773).<sup>97</sup> Klein himself was surprised after the trial run. The results were “bullish” and “unexpected,” and so he “took the attitude that the model was telling [them] something special” (Klein 1991a, 114). Yet, “given the unripe state of his model [Model III] and the fact that the data consisted in good part of shaky preliminary estimates, he evidently felt no urge to push them to publication” (Hart 1976, quoted in Bodkin et al. 1991, 50).

Klein might have given up the publication of his preliminary forecasts, not only because he himself was surprised about the results, but also because he now represented the work of the entire Cowles Commission, and the senior researchers were worried about being left out of the conversation with Washington and with the rest of the community, for presenting unfinished work, product of a very new scientific practice: macroeconometric modeling. One must step back for a second, and really think about how Klein must have felt in this situation. Not only his supervisor, already a world-famous economist, thought that the economy would enter a new recession; the whole economics community, and the governmental agencies in Washington thought likewise; his colleagues at Cowles had been working hard together with Klein to build the models, but they too were quite skeptical about the results, the whole macroeconometric modeling practice was completely new for everybody.

It is only natural that Klein did not “urge to push for publication,” and so, even if Hart baptized this episode as “A lost work of Lawrence Klein” that could but did not put him on top of his supervisor, one should imagine how influential these results might have been to consolidate Klein’s confidence in a scientific practice that he was forging and that became his

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<sup>97</sup> Which is not surprising since Samuelson was part of that “school” of consensus.

lifelong project. In any case, Klein remembered that after 1945 “there was no increase in unemployment,” and that:

During that year, after our preliminary calculations with the first version of the Tinbergen- type model at the Cowles Commission, my position was completely changed to one of no immediate serious recession. In office after office in Washington, economic analysts [...] responded to the calculations from the Cowles Commission, ‘Just wait until mid-1946; there will be 6 million unemployed.’ A better response would have been about 2 million (Klein 2006, 174-175).

## 2.5. Pursuing the large-scale macroeconomic program after Cowles, 1947-1956

Although the period between 1947 and 1956 was one of relative instability, it was also marked by important personal encounters, institutional visits, and upheavals in Klein’s career. After culminating three important years at Cowles, Klein went to Europe where he met with Ragnar Frisch and other important European economists. Together with his wife Sonia, he spent most of the time of his European trip in Oslo, where he closely observed the implementation of economic planning in the context of the postwar reconstruction. Klein also spent some time at Tinbergen’s Central Planning Bureau in the Netherlands, making short trips to Denmark, Sweden, Switzerland, France, and England. In Sweden, he met Herman Wold, Ragnar Bentzel, Erik Lindahl, and Erik Lundberg (Bjerkholt 2014a, 781).<sup>98</sup>

### 2.5.1. Planning issues in postwar Europe

In 1947, Klein obtained an SSRC fellowship to travel to Europe, and visit Ragnar Frisch in

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<sup>98</sup> Herman Wold (1908-1992) was an important Norwegian-Swedish statistician and econometrician, member and president of the Econometric Society. His most influential contribution was the Wold Decomposition Theorem from 1938 used in time series analysis. For an interesting interview see (Hendry et al. 1994). Ragnar Bentzel (1919-2005) was also a Swedish statistician and economist at Uppsala University. When Klein was awarded the Nobel Prize in economics in 1980, both Wold and Bentzel happen to be members of the committee (Bjerkholt 2014a, 781). Also Professor at Uppsala University, Erik Lindahl (1891-1960) was a Swedish economist, president of the International Economic Society (IES), advisor to the Swedish government and central bank. Erik Lundberg (1907-1987), was also a Swedish economist, president of the IES, and considered by William J. Baumol (1990) together with Bertil Ohlin and Gunar Myrdal as member of one of the three “outstanding generations of the ‘Stockholm School,’” contributing “most of all [on] the concept of sequence (process) analysis, in which each development is recognized to evolve from its predecessor in a manner that lends itself to illuminating and systematic analysis” (1).

Oslo. Frisch was delighted with the idea of having Klein as a researcher in his institute (Bjerkholt 2014a, 778). They had met in February of 1947 at the occasion of Frisch’s short visit to the University of Chicago when he gave a talk at the Cowles seminar on “Some basic formulae on demand analysis.”<sup>99</sup> After spending much of the war years in the United States, Haavelmo, Klein’s friend and collaborator, was finally back in Oslo since March 1947. Haavelmo had been promised a position as an economics professor at the University of Oslo, under the condition that he worked during the first year at the Ministry of Finance preparing the National Budget for 1948.<sup>100</sup> Klein was fascinated by the work Haavelmo was doing: “I am just getting oriented in the problems of Norwegian economic planning. The whole thing is interesting and is carried out in a more comprehensive peacetime scale than anything we have ever witnessed. Trygve [Haavelmo] is busy with drawing up the National Budget for 1948. This document covers planning in nearly every phase of economic activity here – manpower, production, consumption, investment, imports, exports, foreign exchange, fiscal policy, prices, rationing, etc.” (Klein, letter to Samuelson, December 4, 1947, PASP box 45). Klein was captivated, and wondered if “the average person realizes how much his life is affected by Trygve’s decisions” (Klein, letter to Marschak, February 14, 1948, quoted in Bjerkholt 2014, 780).

Indeed, Klein showed himself very enthusiastic about the possibility of co-authoring a paper with Haavelmo on welfare economics and the theory of planning (Bjerkholt 2014, 780). In the end, that paper was never written, but Klein produced three lectures on the topic, “Econometric Tools for Planning,” an essay on “The Case for Planning,” and a paper published in 1948 on the “Planned economy in Norway” (Klein 1948b).<sup>101</sup> In this paper, Klein

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<sup>99</sup> Interestingly enough, during his short 1947 visit to the United States, Frisch showed himself more favorable to the type of empirical work done at the NBER, and less enthusiastic about the of the Cowles Commission (Bjerkholt 2014a, 778).

<sup>100</sup> For a comprehensive account of Haavelmo’s return to Norway see Bjerkholt (2015).

<sup>101</sup> The three lectures and the essay are part of the archival material of the Institute of Economics in Oslo, but I have not been able to access them.

(1948b, 811-812) not only studied how economic planning was performed in the Norwegian economy, but he also defended the close relation there should be between planning and econometric methods:

A danger which besets all planned economies may be called the problem of 'the number of degrees of freedom.' There is always the possibility that central planners will try to control too many things at once. Given the technological possibilities of the economy and given the markets that are to be left free, there are only a fixed number of variables at the disposal of the authorities. In the National Budget for 1947, a rather complete national accounting system was utilized to bring about mutual consistency among all the plans, but the definitional relations contained in the national accounting systems are not enough by themselves. In addition, such things as the production functions, consumer demand for unrationed goods, tax laws, the supply of labor, etc., must all be systematically taken care of as side conditions.

Although only one out of three papers was published, planning was such an important topic for Klein that he continued to work on and think about it after his return from Europe. For Klein, an important correspondent to discuss on this topic was Samuelson who "jotted down a number of comments" on Klein's manuscript on "The Case for Planning," apologizing in case he had been "perhaps over critical" (Samuelson, letter to Klein, November 29, 1948, PASP, box 45). Although Samuelson considered that the manuscript was "probably worth publishing," he thought that the central point of the argument was "very obscurely developed" and that it would "not be understood even by specialists." In Samuelson's words "Klein's most important contention [was] that people like Lerner, Lange, et al., [were] wrong in thinking that perfect competition is one way of reaching any defined social optimum." Yet, Samuelson thought that "the analytical nature of [Klein's] attack [was] quite diverse and quite hard to follow," since he introduced "various strands of thought, almost no one of which [was] definitely presented." In particular, Samuelson mentioned Klein's claim that "under planning the 'production constraints' turn out to be more 'efficient' than under certain competition," a point that "was never logically or mathematically demonstrated."

Despite Samuelson’s criticisms, Klein was “still sticking to [his] point [...] that planning is superior to competition because it can effect a modification of the constraints upon the system, this modification not being open to the private enterprise economy.” Here, again, Klein insisted on the relation between econometrics and economic planning, making “the purely formal point that one grand, planned production function gives more degrees of freedom in the maximization process than do the separate production functions.” Klein went on to explain that “what [he gets] by planning (pooling of production constraints), is something that competition cannot be relied on to achieve,” and concluded that he still believed “that there are large gains to be made in a completely planned economy on logical and theoretical grounds” since “planners don’t have to use the same technological constraints that private entrepreneurs use” (Klein, letter to Samuelson, 10 December 1948, PASP, box 45).<sup>102</sup>

Klein’s Norwegian experience was not only about enthusiastic discussions and memorable encounters, however. At the end of his sojourn, Samuelson came to Oslo wanting “to experience more than tourism in Norway; he plainly and openly wanted a significant visit with Professor Frisch, who lived in the outskirts” of the city (Klein 2011, 509). Yet, a “meeting between the two great economists” did not take place. “Professor Frisch, from his suburban home, made it clear that he did not want to meet Professor Samuelson,” sending “messages to the heart of Oslo, [and] insisting that he would not exchange greeting with the world’s rising economics champion from the United States. A leg injury was the stated reason for [Frisch’s] inability to visit [Samuelson, Haavelmo, and Klein]” (510). To Klein, Frisch’s unwillingness “to offer as much as a handshake to Professor Samuelson [...] was hard to watch and hear.” It was “painful to experience the attitude and actions of Ragnar Frisch.” Klein was also sure that

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<sup>102</sup> A copy of Klein’s manuscript remains in the archives of the University of Oslo, but I was not able to get access to it.

“the other *first* Nobel winner in economics [Jan Tinbergen] would have provided true hospitality on such an occasion in his own country” (Klein 2011, 510).<sup>103</sup>

Bjerkholt (2013) has described the episode as a “minor event” (781), and yet this incident might reflect the vestige of an older opposition not between Samuelson and Frisch, but between Samuelson’s supervisor Edwin B. Wilson and Frisch, and between a US American and a European image of mathematical economics. In fact, since the foundation of the Econometric Society, there had been important tensions between US and European economists concerning the kind of mathematics and statistics that economists and econometricians should adopt (Carvajalino forthcoming). Wilson and Frisch were paramount in these discussions, which ended affecting the institutional establishment of the society in Frisch’s direction, at least during the first decades. In fact, whereas Frisch’s interest in including a large list of US-American economists as members of the Econometric Society consisted on creating “a safety valve that could function in the event of national intrigues coming up between Europeans,” to Wilson the “American group” was to play an important role in shaping the nascent econometrics both institutionally and methodologically (Frisch 1932, quoted in Carvajalino forthcoming, 18). As Carvajalino put it, here, again, Wilson “felt that econometricians [put] too much emphasis [on] probability and purely theoretical economics, leaving aside the empirical statistical economics that had been so important in the recent development of [US] American economics” (18-19). This opposition between Wilson and Frisch materialized in August 1933, when Wilson, as a reviewer, rejected one paper that Frisch had submitted for publication in *Econometrica*.<sup>104</sup> Indeed, Wilson considered the paper an example of too much mathematics which “did not read the least little bit like the great papers of Willard Gibbs,” i.e. which was too abstract and

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<sup>103</sup> Sonia Klein, Haavelmo and Klein himself, felt even more deceived by Frisch’s attitude when they learned that he had indeed met with Alva Myrdal (Gunnar Myrdal’s wife) the same day he refused to see Samuelson. Besides, even if they “tried to substitute” they could not do so, and had to see Samuelson feeling let down, and going alone to watch a film in Norwegian dialect (Klein 2011, 511).

<sup>104</sup> Frisch’s paper was entitled “Changing Harmonic Studies from the Point of View of Linear Operators and Erratic Schocks” (Carvajalino forthcoming, 20).

which did not clearly reflect applicability for economists (Wilson, quoted in Carvajalino forthcoming, 20).<sup>105</sup> As Carvajalino has claimed, Wilson thought that US economists should not blindly adopt European mathematical economics, suggesting instead, that “mathematical economics needed to follow a process of Americanization in order to succeed in America” (20-21). This process of Americanization consisted in making mathematics of “practical use” for US economists and economics (Wilson, *ibid.*).

Frisch’s refusal to see Samuelson in Oslo might reflect both the personal skirmishes between Wilson and Frisch since the rejection of the *Econometrica* paper, and the residual of the opposition between at least two different images of mathematics, incarnated in the pragmatic and empirical approach of Wilson, and in the somehow more abstract approach of Frisch. Interestingly enough, however, whereas Frisch could still see Wilson’s influence in Samuelson and hence did not accept a visit from him, the Norwegian Professor was fascinated with Klein, and was also pleased to have him working in Oslo for almost a year. In the end, this episode can be read as supporting the idea that Klein’s work represented a middle ground between abstract and more applied approaches to econometrics.

### 2.5.2. Bridging the gaps? Klein’s fleeting experience at the National Bureau of Economic Research

Whether Klein’s approach could be regarded as a middle ground between abstract and more applied mathematical economics was soon to be examined at the other side of the Atlantic when Klein returned to the United States at the end of 1948 after an invitation by Arthur F. Burns to join the National Bureau of Economic Research.<sup>106</sup> It is worth noting that this must

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<sup>105</sup> J. Willard Gibbs (1839-1903) was a US American mathematician and polymath, and Wilson’s PhD supervisor at Yale University. Gibbs’s idea of “mathematics as a language” was very influential to Wilson and later to Samuelson.

<sup>106</sup> Arthur F. Burns (1904-1987) was a US American economist, closely related to Wesley Mitchell, and the NBER. In fact, Burns took over the National Bureau directorship in 1945 after Mitchell’s retirement. For a detailed account of the role that Burns played in the NBER see (Rutherford 2005); for a history of the NBER see Fabricant (1984).



have been a strange moment for a former Cowles researcher to be at the Bureau, since the “Measurement without theory” controversy was at its zenith (see chapter 4). Yet, if there was somebody that could rebuild bridges of communication between the type of work done at both institutions, this person was Klein. On the one hand, Klein understood the importance of the work of his colleagues at Cowles, and especially of Haavelmo’s work which he described as “the inspiration for the research focus at the Cowles Commission” (Klein 1991a, 113). On the other hand, he had always admired the “painstaking tradition of Simon Kuznets” (115), and the unusual attention that him and his team at the NBER payed to data.<sup>107</sup> In other words, Klein’s empirical work using the very sophisticated econometric theory developed at Cowles during the 1940s, was in fact, a middle way alternative to the approaches confronted in the “Measurement without theory” controversy, between the abstract approach of Cowles, and the NBER’s “empiricist” approach, as Koopmans (1947) put it.

The methodological quarrels of those years, and particularly the Measurement without Theory controversy, casted important concerns about the NBER’s leading place in empirical research, especially at the Rockefeller Foundation, which was the most important funding source for the National Bureau (Rutherford 2005, 121; Mirowski 2002). Aware of these criticisms, the director of the Social Science Division of the Rockefeller Foundation Joseph Willits, asked Burns to “give him his view on econometric models and Keynesian economics.” Burns answered that he had “recently set one investigator to work on econometric models, not because any member of our group has much faith in them but because we wish to check our judgment and give this approach an opportunity to prove its merits” (Burns, quoted in Rutherford 2005, 122). This investigator was, of course, Lawrence R. Klein, according to whom “my econometric interests were tolerated but not enthusiastically monitored” at the Bureau. Furthermore, Klein felt that he “was treated somewhat as a curiosity – an outsider who

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<sup>107</sup> For an account of Kuznets’s work and legacy in the economics discipline see Fogel et al. (2013).

might eventually view the NBER approach in a more favorable light” (Klein to Rutherford, August 27, 2002, in Rutherford 2005, 122). Yet, his experience at the National Bureau left Klein with the impression that there was no “real conflict between the econometric work [he] or others [wanted] to do and the work of the business cycle staff of the Bureau.” In fact, to him, the “National Bureau technique” was largely “non-parametric,” which was a necessary first step to a final parametric study like that undertaken by most econometricians, and that there was “actually an econometric school of thought that [fell] in between the work of the Cowles Commission and the National Bureau” represented in the works of Gerhard Tintner, Richard Stone, and Herman Wold (Klein to Burns, January 23, 1950, in Rutherford 2005, 122).

### 2.5.3. Michigan’s happy marriage: The Survey Research Center and the Research Seminar in Quantitative Economics

Klein remained associated to the Bureau until 1951, but starting in 1949 he received a Carnegie fellowship and became a research associate of the Survey Research Center of the University of Michigan, in Ann Arbor. In 1950, his affiliation to this university expanded to the Economics Department when he was appointed Lecturer. The type of empirical work in which Klein embarked at Michigan at the beginning was quite different from what he had done before. Whereas Klein had been mainly confronted with the treatment of time series to build his macroeconomic models, in Michigan he faced survey data produced by an ambitious project on consumer behavior led by George Katona.<sup>108</sup> The effect that Katona’s work exerted on Klein is explicitly found even in the 1970s and 1980s, when Klein defended the use of survey

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<sup>108</sup> Klein and Katona had met before at the Cowles Commission at the end of 1947. Katona (1901-1981) was a Hungarian psychologist and economist who dedicated his career to combining psychology and economics. As stated by his former colleague at the University of Michigan James N. Morgan (1982), Katona attempted at “convincing psychologists to be interested in common behavior patterns of masses of average people, and [at] convincing economists to be interested in behavior that was not assumed as a mechanical response to stimuli but that resulted from problem solving that used past experience and learning to arrive at new insights” (1140). His research project led to the production of important survey programs and data funded by the US Federal Reserve, among which stands the Survey Research Center in Ann Arbor. For an account of Katona’s attempt to understanding consumer behavior see Edwards (2012).

techniques to study not only consumer's but also investor's expectations, as an alternative to the notion of rational expectations, which was gaining ground in macroeconomics at the time (see for instance Klein and Mariano 1987, 442).

Around 1950, Koopmans and Klein toyed with the idea of reviving the macroeconometric project at Cowles. Klein had in mind “a much more elaborate project, on the empirical side, than the former research and [wanted] to know if the Cowles Commission [had] any interest in such a scheme” (Klein, letter to Koopmans, May 29, 1950 quoted by Bjerkholt 2013, 781). Despite two favorable reviews by Tinbergen and Haavelmo, the project never took off. On the one hand, Klein possibly thought that it was too much a risk to embark in such an ambitious project in an institution, which, like Cowles, was rapidly changing its research interests from econometrics to activity analysis, and preferred to go for the safer option in Michigan “where the attraction was the Survey Research Center” (Klein's interview of January 25 1980, TUMA, box 5).<sup>109</sup> On the other hand, Michigan, too, would promise Klein a better institutional position where he would have a team, even if modest, under his command. In addition, since the mid-1940s, the Economics Department at Michigan had been going through a process of expansion, recruiting important people to reinforce the faculty, which was characterized by “a dual tradition for scholarship and public service” (Brazer, “The Economics Department of the University of Michigan: a centennial retrospective,” TUMA, box 5, 102).<sup>110</sup> In particular, some of the new recruits, like Klein, came first through the SRC, later to be appointed faculty members at the Department. Among the new recruits were Gardner Ackley (1946), Katona (1946), Richard Musgrave (1948), James N. Morgan (1950), Klein (1950), John Lansing (1956), and Eva Mueller (1951).

In Ann Arbor, the University of Michigan offered an important opportunity for Klein,

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<sup>109</sup> For a historical account of the origins of Activity Analysis in the Cowles Commission see D ppe and Weintraub (2014b).

<sup>110</sup> I want to thank Pierrick Dechaux for sending me the archives of the University of Michigan (TUMA).

consisting on the establishment of a “marriage of econometrics with Survey Research – one to give breath and the other depth if a quantitative economics is to emerge adequate to deal with the demands made upon it solving questions of policy” (“A Proposal for *A Research Seminar in Quantitative Economics*,” TUMA, box 5, 1). This marriage was represented by the creation of the Research Seminar in Quantitative Economics (RSQE), which begun functioning on October 1, 1951, supported by a Ford Foundation grant, with the “objectives of training faculty and students in quantitative methods of economic research producing substantive results on important empirical problems” (RSQE 1952, 1). Apart from Klein, during 1951-1955 the RSQE research staff included Howard Raiffa, Morgan, Daniel B. Suits as research and faculty associates, and Arthur Goldberger and Stephan V. Vail, among others, as research assistants (RSQE Annual Reports 1951-52 and 1953-54, TUMA, box 5).

The first important project that the seminar proposed to undertake consisted on studying the “reconciliation of microeconomic and macroeconomic patterns of behavior.” The main objective of the project was to assess whether the two sets of information, micro data obtained from survey methods and macro data obtained from market reports or social accounts, “lead to mutually consistent estimates or behavior patterns and the extent to which one set may reinforce the other” (2). An important characteristic of the seminar was its interdisciplinary nature, which involved “the wholehearted cooperation” between economists, sociologists, psychologists and statisticians.<sup>111</sup>

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<sup>111</sup> Apart from Klein and Katona (who would be in charge of cooperating on the estimation of micro- and macroeconomic patterns of behavior, forecasting economic fluctuations and on the interdisciplinary aspects of psychology and economics), the prospective personnel was composed by Musgrave, McCracken, Suits, Morgan, Stolper, Craig, Dwyer, Festinger, and Boulding. In fact, the postwar was an era in which social scientists and natural scientists worked hand-in-hand in numerous projects (see Erikson 2013; Choen-Cole 2007). Boulding, in particular, was a paramount figure advocating for the integration of the social sciences not only in the sense of interdisciplinary collaboration, but also in the sense of establishing common general principles from various disciplines, such as economics, sociology, political science, and psychology. On Boulding’s project see Fontaine (2010).

In a similar functioning to that of Cowles's, the RSQE held weekly meetings "at which seminar members [discussed] research problems and techniques," complemented with less frequent meetings where external speakers presented papers on relevant subjects for the projects of the seminar. The purpose of the RSQE was to reinforce the econometrics of an economics department that "was relatively thin" at that time, with "no formal course in econometrics," other than Daniel Suits's, who taught a course in statistics. At first, however, the department was not desperately eager to go too generous on the very new field of econometrics. "The chairman [Leo Sharfman] was very cautious about introducing something very new, and he told [Klein] that he had always wanted to get something started in a very modest way," and so Klein would give only one course on econometrics.

Yet, the big push for the department came just a bit later when the "Ford Foundation announced that it was giving a million dollars to each university of about ten universities in order to legitimize itself as an eleemosynary foundation for tax purposes" (Klein, *ibid.*, 2). The money would come in soon, but the Economics Department seemed not to know what to do with it. Whereas the psychologists and sociologists "were more attuned to project work and knew how to spend it," the economists "were having precious few positive suggestions" (2), because they "were all [...] the lone scholar in his study working" (3). Sitting on the side lines of the department as a newcomer, Klein watched "some clear policy on a program for the Economics Department," and said in conversation with Kenneth Boulding: "I know how to spend that money [...] we can design a research group that needs that money to get started." And so, with the concept in mind but "fumbling around" the exact title, Klein and Boulding put forward the idea of the research seminar in quantitative economics: "the idea of a research seminar was [...] Boulding's, [...] quantitative economics was [Klein's]" (2).

Klein, now director of the seminar, gave an econometrics lecture, and was "partly on the budget of [RSQE, which] had it as a central project [since] the very beginning to build a macro

model of the US.” The seminar got “\$20,000, which again looked like a lot of money for a small research operation in the early ‘50’s.”<sup>112</sup> But the problem was that the money came in only in late spring of 1952, when the graduate student awards had already been made. Klein “went to the Rackham School and culled through the rejects” to find “Stephan Vail and Artie [Arthur] Goldberger.”<sup>113</sup> Klein also took some people who were on the campus, but it was “the rejects [who] turned out to be the powerhouse” of the seminar. Officially, the RSQE started functioning on October 1, 1951 (Klein’s interview of January 25, 1980, TUMA, box 5, 2).

#### 2.5.4. Finding stability at the University of Michigan? The RSQE-SRC collaboration, and the birth of the economic outlook conference

With the funding secured for the functioning of the Research Seminar in Quantitative Economics, Klein proposed a way to work that was really something new for the economists, “except for [those] who were associated with the Survey Research Center.” Morgan, John Lansing, Katona, and the professors in sociology and psychology “were attuned to that kind of [project] research. But the people who were the professors [and] assistant professors in the [economics] department were not project research oriented” (3). With both the RSQE and the SRC working together, the economics department wanted to “do something to get more involved in public affairs [and] to get more money into the place for a research program.” Some faculty, including Klein, Suits, Katona, Musgrave, and Gardner Ackley, decided to organize a conference on the economic outlook and “started calling on friends in Detroit, who were economists in industry, in the motor companies, and their supplying companies and

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<sup>112</sup> In fact, the RSQE was granted \$60,000 for three years at \$20,000 each, and so Klein “had a string of research fellows,” using the money as well “for a modest office set-up, for secretarial, and scholarships for students” (*ibid.*).

<sup>113</sup> Goldberger (1930-2009) earned his BA in economics from NYU in 1949, where he was influenced by Solomon Fabricant. In 1951, he preferred to enter the graduate program in Michigan over Johns Hopkins and Stanford, where he started working as research of Klein. Until his arrival at Michigan, Goldberger had never taken a course in econometrics or mathematical statistics. Goldberger completed his PhD in 1958 under Daniel Suits’s supervision, after Klein left Michigan.

others.” They “got them down at Ann Arbor for a meeting on a Saturday afternoon” to organize the successful conference for the spring of 1953 (4):

And we set it up, sketched it out, and carried it out. We send letters all over. And we put our own program with the econometric model being used, with the Survey Research Center Consumer attitudes forecast, and then a lot of forecasts from industry. And we got economists from companies all over the mid-west and, indeed, all over the nation to come. We held the first of the outlook conferences in [...] October of 1953.

The economic outlook conference was the occasion at which the RSQE “unveiled the first use of the model” they had been building, which forecasted a small recession for 1953-1954 after the end of the Korean War. This work, an early version of the soon-to-be-famous Klein-Goldberger model, was the basis for writing a press article that gave Klein and his team “enormous publicity,” because the publication took some international height. In fact, on November 16-17, 1953, Colin Clark (1953a; 1953b) published a two-parts-article in the *Manchester Guardian* with a very appealing title: “Danger signs of slump.” In that article, Clark claimed that the United States was heading to a new recession like that of 1929. This recession, Clark argued, had been expected by the end of World War II but it had been successfully delayed by the new Korean war, and now that this war was over the recession was ahead the US economy. Yet, Klein and Goldberger’s projections prepared for the economic outlook conference yielded different results. A recession was indeed ahead the United States, but it would be small and modest, they argued. With no connections at the *Manchester Guardian*, whatsoever, Klein and Goldberger (1954) mailed out their response to Clark’s article, “‘A mild down-turn’: the American trade recession,” which was published on January 4, 1954, on “a full page spread.” This publication gave the Michigan econometricians (and particularly Klein) a tremendous boost and world recognition, above all because Clark’s results were not based on “dull financial journalism” as he himself put it (Clark 1953a, 4), but on a system of mathematical equations he had put together and published in *Econometrica* in 1949: “A system

of equations explaining the United States trade cycle, 1921 to 1941.” Clark’s (1949) system, which had proved capable of mimicking the observed US trade cycle, including the dramatic slump of 1929, had now been improved with the inclusion of an equation dealing “with the effect of high construction costs upon the demand for houses and other buildings,” to better suit the postwar era. The well-established Oxford Professor was optimistic about his system of seven equations and considered that it sufficed “to predict – and therefore [...] to control – the movement of the business cycle” (1953a, 4). “With the greatest respect for the distinguished work of Mr Clark,” the young Lecturer Klein and his student Goldberger “felt obliged to lay some of the forecasts for 1954 from their mathematical model,” which recognized that there would be a minor decline in the US economy, but not at the rate of Clark’s pessimistic projections (see figure 1):

Figure 1:  
FORECAST FOR 1954  
Compared with corresponding observed data in 1952  
and 1953  
(Dollar amounts in billions, persons in millions, indexes  
on a 1939 basis)

	1952	1953	1954
		(estimate)	(forecast)
		from first 9 months)	
Gross national product in 1939 prices .....	172.0	180.1	174.8
Net national income (deflated) .....	137.9	144.1	140.8
Private employee compensation (deflated) .....	78.7	84.7	82.3
Non-wage, non-farm income (deflated) .....	35.2	36.2	36.1
Farm income, excluding Government payments (deflated) .....	7.3	6.0	6.6
Consumption in 1939 prices .....	111.4	116.1	117.3
Gross private capital formation in 1939 prices .....	24.3	26.0	22.7
Private imports in 1939 prices .....	5.5	6.2	6.0
Index of wage rates (1939 value 122.1 instead of 100) .....	326.2	349.7	367.8
Index of prices .....	202.4	204.4	220.5
Number of employees .....	56.0	56.8	56.5
Corporate savings (deflated) .....	1.9	0.9	1.7
Corporate profits before taxes (deflated) .....	16.5	17.5	16.9
Capital consumption in 1939 prices .....	19.4	21.7	19.1

Source: Klein and Goldberger 1954, 4.

Although the Clark episode certainly improved the legitimacy of the RSQE, the SRC, and the Michigan Economics Department, a macroeconomic model could not live only from one-single-time accurateness and world recognition. Given the growing activity and importance of the modeling project, it was also necessary to build up the calculation equipment at the university. For that purpose, the millionaire Ford grant “included an equipment budget that was administered by the chairman of the Psychology Department” (Brazer, TUMA, box



5, 115). Klein insisted that acquiring an electronic desk calculator for the RSQE would facilitate their job, and so an expenditure was approved for up to \$750 to buy the machine. Although the price of the Monroe electronic calculator was lower (\$637.50), Sharfman wanted to be sure that this expenditure be approved, since he thought Klein was asking for “a lot of money.” Indeed, \$640 represented a significant amount of money in the early 1950s, but compared to the Ford Foundation \$1 Million grant, the reticence to buy the calculator out of that money suggests only how new the whole econometric modeling exercise appeared to Klein’s colleagues, and how many doubts and skepticism it raised. In the end, the request was passed on under the condition that the RSQE shared the “new machine with the statistics classes, to which it would be carried for demonstration, and that it would also be available on request to other faculty members in the department” (115).

It was also in Michigan that Klein really started to play around with the possibility of connecting econometrics and computers for the first time. Apart from the electronic desk calculator, and the IBM (electromechanical) tabulator in the basement of the Rackham School, a nascent computation center was available there as well: The Willow Run Research Center in Michigan, which counted with the Michigan Digital Automatic Computer (MIDAC). After the enthusiastic results of the economic outlook conference, Klein and his team tried to make the most out of this center, and “started fiddling around with computers.” They “spent a lot of time working with that group [...] on how to automate econometric models and use them in the computer mode”:

We did a lot of talking, a lot of thinking on that subject, [but] we never did get a successful implementation [...] And we started doing some things by hand, some things by computer. We mixed the process, but we began to get oriented in the computerization of econometric models at that time (Klein’s interview of January 25, 1980, TUMA, box 5, 4).

### 2.5.5. The “rejects” as powerhouse for the RSQE: Building the Klein-Goldberger macroeconometric model

Within this environment of favorable forecasts, growing computation facilities, funding reassurance for his institutional setting, a fully-fledged working team, and a brilliant assistant rescued from the “rejects” of the Economics Department, an enthusiastic Klein continued his project of building a new macroeconometric model of the US economy. Although at the time it was “strange” to “get your feet wet” with econometric work, with hindsight Klein recognized the pioneering character of the Michigan project in terms of both forecasting and teaching:

The public policy process could not operate as it does today without a very important model input [...] Every respectable university throughout the world has classes in econometrics, workshops in econometrics, projects in econometrics, all the things we were doing then in terms of teaching, team research, project research, use of models with private sector and public policy, all those things [...] blossomed in the ‘60’s and ‘70’s [but] we were doing them in the early ‘50’s. And [...] I would say the Michigan group was laying the ground work (Klein’s interview of January 25, 1980, TUMA, box 5, 5).

Apart from the creation of both a team around the RSQE and the economic outlook conference, this pioneering work of macroeconometric model construction took symbolic significance after the 1955 publication of the famous Klein-Goldberger model: *An Econometric Model of the United States, 1929-1952*. This model became later a landmark for large-scale macroeconometric modeling around the world, and set the bases for both the teaching of macroeconometric modeling, and the “growing econometric forecasting industry.” Yet, during the 1950s, the model was not used as a teaching device. Instead, it was considered a cutting-edge research object in constant evolution, and, above all, a forecasting tool. The research seminar was “really based around this project team research effort,” and was understood as a continuous program in that “new data, reformulations, and extrapolations [were] constantly being studied” (Klein and Goldberger 1955, 1). In short, the distinctive feature of the project was “that the task [was] not [...] seen as a once-and-for-all job” (1). The purpose of the weekly RSQE was to build up the model, and, for that purpose, Klein had not only “his own people,”

but he had external visitors like Clark, Jan Tinbergen, Oskar Morgenstern, Wassily Leontief, Gerhard Tintner, Robert Wolfson, Koopmans, Marschak, James Tobin, Robert Solow, Franco Modigliani, Gerard Debreu, among others, to help in the construction of the model as well, giving important insight and discussing relevant aspects of the models during their visits.

With 15 structural equations, five identities, and five tax-transfer auxiliary relationships, the resulting model was a “medium-size” model “truly intended (at the time) to be an up-to-date working model, applicable to economic problems like those encountered in the business cycle forecasting” (Bodkin et al. 1991, 57). As Goldberger put in 1952, “the model is being constructed as a working, aggregative system with an eye towards its use in the future as a trunk on which to graft disaggregated sector equations, special industry models, and up-to-date observations” (Goldberger, RSQE TUMA, box 5, 11). Goldberger remembered that Klein’s “work at Michigan was a continuation of the work he [had] done at Cowles” and that “what [is] called the Klein-Goldberger model is really Klein Model IV” (Goldberger et al. 1989, 135). In fact, “part of [Goldberger’s] job was [to go] to the national income accounts, [pull] out the data, and [construct] variables for the macro model [for which] the computation, of course, was a major consideration” for which he played a paramount role (136). With hindsight, Goldberger saw himself as “a clerk on the model,” and considered that Klein had been “generous enough to put his name on it” (135). This characterization of Klein as a generous person might well fit his personality. Yet, without denying this trait, I think that Klein’s generosity in this case also reflects his own image of macroeconomic modeling as a teamwork endeavor that must be carried out with a clear division of labor and under a specific institutional configuration.

After four years of hard teamwork effort, revisions, and data updates, Klein and Goldberger published their model with a five-page long caveat as a preface, where they explained that the whole model was now outdated “by the basic revisions of the national

income accounts made by the US Department of Commerce in mid-summer 1954”; this, however, did not invalidate the whole approach, but “simply made the problem of parameter re-estimation more urgent” (Klein and Goldberger 1955, vii). Yet, the authors, and their whole team, felt confident about the fact that their model “[gave] empirical insight into the structure of the United States economy” (vii).

2.5.6. “Leaving for the academic freedom of Oxford”:  
McCarthyism and dark times for US democracy and  
freedom of thought

MR. TAVENNER: What is your name, please, sir?

MR. KLEIN: Lawrence R. Klein.

MR. TAVENNER: It is the practice of the committee to advise every witness that they have the right to consult counsel during the course of testimony if they so desire. I note that you do not have counsel with you, so I assume you are willing to proceed without counsel.

MR. KLEIN: Yes.<sup>114</sup>

On April 30, 1954, during one of the periods of most severe political and academic freedom repression in the history of the United States, Klein was called in to Detroit to testify before the Committee on Un-American Activities about his actions “as a Communist in Chicago during 1946 and 47” (Klein’s interview of January 25, 1980, TUMA, box 5, 7). These were the years of the McCarthy Era, during which the lives of thousands of individuals, as well as the institutional configuration of US academia were heavily affected (Schrecker 1986).<sup>115</sup> Steered by the East Lansing representative Kit Clardy, the investigations on Un-American activities had been underway since about 1952 at the University of Michigan with investigators coming on the campus and interviewing people (Klein’s interview of January 25 1980, TUMA, box 5, 7). At that time, Klein, who was “up for promotion, [...] had a long talk with Sharfman about

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<sup>114</sup> Eighty-Third Congress, second session (1954) “Hearings before the Committee on Un-American Activities,” Washington D.C.: United States Printing Office.

<sup>115</sup> For different accounts on the McCarthy Era related to economics, see Schrecker (1986), Solberg and Tomilson (1997), Rancan (2008), Weintraub (2016).

the fact that [he] was one of the people [the investigators] were calling up for questioning.” Sharfman said to Klein: “you are a lecturer now [...] I’m not going to appoint you an associate professor or anything [...] I’m just going to lead frog the whole thing [and in] one year [...] we will promote you from your status as Lecturer and Director of the Research Seminar into a regular professorship” (7).

But one year past, the investigations continued, the tensions increased, and Klein was not promoted. Although people on campus did not know whom the investigators were seeing, the University administration did know this. To Klein, “the one disturbing aspect was that all the investigators worked hand in hand with the University administration on this [and] also the State Police got in on the act too, and they came around and investigated people” (7-8). The accusations went on, as did the investigations, and the hearing in Detroit, in which Klein not only mentioned names, but also “confessed” that he had been a member of the Communist Party, although just for a short period of time and for reasons that were not really of ideological adherence:

MR. TAVENNER: Did you become a member of the Communist Party during the period of your teaching at [...] the Abraham Lincoln School [in Chicago]?

MR. KLEIN: Yes.

MR. TAVENNER: Will you tell the committee, please, the circumstances under which that occurred?

MR. KLEIN: Well, I am not sure whether the direct line is through the Abraham Lincoln School or through the Jewish People’s Fraternal Order, of which I was a member, but they both occurred at the same time, and one night after a meeting, either I was the chairman or the speaker, I don’t know which, we were having coffee afterwards. I was approached and asked if I would give a course in the neighborhood near where I lived in Chicago on Marxist economics [...] I was working on this general problem of, first, adult education, that you could teach fairly complicated ideas to adults who weren’t regular students, who didn’t have a lot of formal education, and [second] I was working on the problem of the relationship between Marxist economics and [...] Keynesian economics, and I was interested in this challenge because I developed what I thought were some new ideas on this relationship, and I said I would try to teach this course in Marxist economics. Then I was told [...] sometime later, well, no, all the people coming to this class are Communists, members of the Communist Party, and

it wouldn't be right for someone who is not a member of the Communist Party to teach Marxist economics to them. I didn't comment, and then shortly after a bid came that if I would join the Communist Party, I would teach this course, and then it would be all right. I did [join], [...] I was a member [of the Party].

“The upshot was that, as Sharfman said, they couldn't recommend [Klein] for the promotion,” and so, Klein although not forced to leave Michigan, preferred to look for other options, and decided to go to Oxford. The department, however, remained optimistic about the possibility of appointing Klein full professorship with tenure at  $\frac{3}{4}$  time in the department, and  $\frac{1}{4}$  in the SRC for the 1955-56 academic year.

In fact, two meetings were organized to discuss whether Klein could be offered tenure. On November 19, 1954, a vote was made among the faculty, resulting on a 15 to 1 tally of the votes, and three absences (Brazer, TUMA, box 5, 132-133). The challenging vote was by William A. Paton, whom Klein later remembered as “a reactionary person [with] violent dislike for anything liberal” (Klein's interview of January 25 1980, TUMA, box 5, 12).<sup>116</sup> In fact, Paton wrote various letters to the Dean explaining his dissenting vote in “strident tones” through which he did not hesitate to point out Klein's personal idiosyncrasies, and also questioning whether there was a real necessity to hire another professor in econometrics (Brazer, TUMA, box 5, 133).<sup>117</sup> In concrete terms, Paton judged “that Mr. Klein is aggressively Socialist in his entire outlook” with “very little economics in it,” arguing again that the Department had “no need for a thoroughgoing Socialist professor at this juncture” (Letter from Paton to Dean Odegaard, February 3, 1955, quoted in Brazer, TUMA, box 5, 134).<sup>118</sup>

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<sup>116</sup> Paton (1889-1991) earned both his BA (1915), and PhD (1917) from the University of Michigan. He was appointed Edwin Francis Gay University Professor of Accounting in 1947.

<sup>117</sup> Klein also recalled that “when [...] Milton Friedman came to give some lectures [to Michigan] the only person he wanted to see and talk too was William Paton. I don't know if that reveals anything or not, though I was very friendly with Milton over the years before that” (Klein's interview of January 25, 1980, TUMA, box 5, 11).

<sup>118</sup> In his delirious nonsense, Paton also denounced that Klein's “conversion [was] only skin deep, because he currently states that he espouses the Norwegian brand of socialism, regarded by most economists [...] as the most extreme case of statism in Europe [...] outside of Russia and the satellites (Paton, quoted in Brazer, TUMA, box 5, 133).

After all this stale situation, and after multiple comings and goings between Klein, other faculty members, and University administrators, on June 24, 1955, Ackley enthusiastically contacted Klein again to let him know that his appointment was official, and that he was offered full professorship, but without tenure.<sup>119</sup> Paton's defamatory campaign had resulted in the decision of the Board of Regents not to grant tenure to Klein. Although disappointed for the tenure question, Klein accepted the offer expressing "his deepest gratitude for the effort made by Ackley and the other department members on his behalf," and informed that he would come back for the spring semester of 1956. Yet Paton persevered in his internal effort to block Klein's appointment. In late October 1955, Ackley received a surprising letter from Klein:

A few weeks ago [...] I received an anonymous letter outlining Paton's recent moves against my appointment. I am not certain whether this tip was sent as a warning by a friend of what to expect on my return or as a threat by a foe against my returning at all. Should I take seriously Paton's activities? He now approaches the Regents and administration directly to let my appointment expire at the end of the current year and makes the veiled threat of carrying the case to the public if he gets no satisfaction from University authorities. I had planned to come back full of enthusiasm [...] but I don't look forward to the bother of carrying on this fight once again.<sup>120</sup>

Marvin L. Niehuss, Vice President and Dean of faculties, sent a letter to Klein in November saying that he could assure Klein that the University of Michigan would renew his appointment as Lecturer, but that he could not give any further guarantees about the actions of the Board of Regents after Paton's moves. Niehuss ended his letter saying that "it is my personal hope that you will decide to return to Michigan and rely upon time and your own accomplishments

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<sup>119</sup> One of the absentees, Robert Ford, explained his doubts on Klein's appointment in similar ways: "How much time is required for a person to become de-communized, I do not know [...] The story of Dr. Klein's activities [...] is a sordid one involving clandestine meetings, contacts with persons who were known only by their first names [...] I have serious misgivings as to the objectivity of one who has been a party to such proceedings [...] Appointment as a full professor [...] is very rapid progress that is hardly [...] warranted under the circumstances" (Brazer, TUMA, box 5, 133).

<sup>120</sup> Klein remembered later that "Paton... somebody wrote me an anonymous letter, sent it to England, telling [him] not to come and the dire things that would happen if I did [...] It was a very nasty letter. During the presidential campaign of '76 I got a lot of, you know, hate mail, spite mail, and this was in the same kind of character" (Klein's interview of January 25, 1980, TUMA, box 5, 12).

to overcome the problems that now exist.”<sup>121</sup> The letter was, at the very least, ambiguous and disappointing.

On November 18, Ackley wrote again to Klein, reassuring him that “none of us in the Department had any inkling that the question of your status had been reopened,” and reiterated the support of the majority of the Department in his appointment.<sup>122</sup> Clearly disheartened for all the effort and enthusiasm he had invested in his macroeconomic project in Michigan, and tired of the whole situation, Klein replied to Ackley on December 9, 1955:<sup>123</sup>

Your letter is very kind and certainly makes me hesitate, yet I can’t banish the feeling [...] that there is a serious deficiency of academic freedom in the summation made by Niehuss. It isn’t the risk of uncertainty [...] that bothers me so much [...] and it isn’t any fear of Paton’s actions. It is simply a feeling that it is wrong for the Regents to pay heed to Paton and also to weaken what was already a less than satisfying offer. I don’t put much value in tenure as such; I simply don’t like the reasons for which it is withheld [...] I find it hard to make this decision, to give up all that I worked so hard on at Michigan, and especially to refuse acceptance of the very best fruits of the magnificent efforts you and other dear friends made on my behalf.<sup>124</sup>

Despite the despicable treatment he received from Paton, despite his tenure denial, and despite the hostile situation he had to undergo in his own country, losing the fruits of at least four years of effort, Klein remembered his days at Michigan as “very enjoyable” and important for his career, showing, once again, his generosity and optimism:

I liked the department, I liked the university, I liked the community, and I did a lot of good research in those years. It was a very formative period in terms of my professional career. And I look back on it very fondly. I often go back and I always enjoy being there [...] I don’t feel any sense of rancor or anger or anything. I just have sort of good feelings, and it’s all so long ago on this issue. And the other things we talked about, the Survey Research Center and [the] Research Seminar in Quantitative Economics I look upon [...] with great pride and fondness and those are pleasant memories (Klein’s interview, TMUA, box 5, 12).

While Paton was still fighting his own crusade, and the University of Michigan was not

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<sup>121</sup> Letter from Niehuss to Klein, November 14, 1955. Copy in Klein department file, quoted in Brazer, TUMA, box 5, 138.

<sup>122</sup> Letter from Ackley to Klein, November 21, 1955 in Klein department file, *ibid.*

<sup>123</sup> Letter from Lawrence Klein to Gardner Ackley, December 9, 1955 in Klein department file, *ibid.*

<sup>124</sup> Letter from Lawrence Klein to Gardner Ackley, December 9, 1955 in Klein department file, *ibid.*



able to offer Klein a tenured position, Oxford University offered Klein a “very good appointment” as Reader. Klein spent four years in Oxford at the Institute of Statistics from 1954 to 1958. These were very productive years as well, which gave Klein the opportunity to embark not only in the construction of another macroeconomic model, this time of the UK economy (Klein 1961), but also to complete some methodological work. When Klein went to England in 1954, he remembers, “econometrics hardly existed at Oxford” (Klein and Mariano 1987, 422), but, of course, important people were either working on similar problems, or they were at least sympathetic to the econometric project in other parts of the country. To mention only three of them, Richard Stone was working with his team in Cambridge on “theoretical and applied problems in econometrics.” A. W. Phillips, was at the London School of Economics (LSE), and was very active publishing papers that became central to macroeconomics (Phillips 1954; 1957; 1958).<sup>125</sup> And there was also Colin Clark who was the director of the Institute for Agricultural Economics in Oxford.

Klein recalls that he was “essentially given green light” to do what he thought necessary to promote econometrics. Especially appealing to him was that the Oxford savings survey, although very sparse and not as good as that in the US, was “patterned very much after the Michigan surveys.” This was, of course, good news for the macroeconomic model construction. In addition, Klein “had a lot of hand computer support, and [...] could use whatever hardware was on the premises, but it was very primitive and kept in conditions that were not ideal for work” (Klein and Mariano 1987, 422). Despite the equipment and funding possibilities, despite the double freedom Klein counted on, first of in terms of liberty of thought, then liberty to embark in his new modeling project, his arrival in Oxford Klein was not an easy task. Klein had to start from scratch, building a whole new team again, constructing and preparing data, and getting both the research assistants and seminars at the service of the

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<sup>125</sup> For a detailed biographical account on Phillips see Bollard (2016). For a history of the Phillips Curve see Forder (2014).

model. In the end, maybe because of these difficulties, and because Klein had set a task that was “far too ambitious for the available data,” the model that resulted from this experience was, in Jim Ball’s words, very poor on the whole (Ball 1981, 83).

## 2.6. Joining the Ivy League: Pennsylvania University and the John Bates Clark Medal, 1958-1959

Although very productive and significant, the 1950s was a complicated decade for Klein, because of all the upheavals in his academic life. This decade was characterized, on the one hand, by great periods of enthusiasm, nascent projects, and the realization that he was after something new and important; but, on the other hand, this was also a decade of important difficulties, frustrations, and disappointments. The years 1958 and 1959, however, represent the beginning of a new and more stable stage in Klein’s academic life. 1958, marked the opening of another important door through his appointment in an Ivy League University, allowing him not only to come back to his country, but also to find a stable place where he could finally develop his life-long macroeconomic modeling project in his way. In 1959, both the importance of his work and his stature as an economist were recognized through his awarding of the most prestigious prize for economists under age 40. At the end of the 1950s, then, Klein was in his way to consolidating the new scientific practice he had forged through the overcoming of important obstacles and resistance. He was no longer seen as the young prodigy of the 1940s with curious affiliations to left-wing organizations. Instead, he had become an industrious, original, generous, pluralistic scholar, and founder of a new way of doing and understanding macroeconomics. Klein, of course, was backed up by institutional, academic, business, and public relations, and by a growing demand for his product. The potentialities of the macroeconomic tools were now realized by many, and people in academia, the private sector, and the public sphere were also embarking in ambitious macroeconomic modeling projects. Soon, macroeconomic modeling would be adopted not only in several institutions

across the United States, or England; other countries like Japan, China, Israel, Mexico, France, etc., would adopt this new scientific practice as well, based on the pioneering effort of Klein. That expansion would come later, in the 1960s.

### 2.6.1. Klein's definitive return to the US: turning down Oxford for Penn

In the summer of 1957, Klein came back to the United States for some months as a research consultant to the now "Cowles Foundation" at Yale. During that summer, he also received two faculty offers in the United States. One of the offers came from his *alma mater*, the University of California at Berkeley, and the other from the University of Pennsylvania. The Economics Department at Penn was not only expanding, but people who had done important work in "more traditional economics of the pre-war" were also retiring (Klein and Mariano 1987, 428). In addition, the faculty was excellent with people like Irwin Kravis, "a true loyal Pennsylvanian" and responsible for enhancing and broadening Penn's Economics Department (Klein 1993, 175), Irwin Friend, Richard Easterlin, and Sydney Weintraub. Klein "was very impressed with the University of Pennsylvania's offer in terms of the academic freedom aspect," which he saw as a "very tough and fair stand" given the stale political atmosphere that was still reigning the US (Klein's interview of January 25, 1980, TUMA, box 5, 8). The offer was very appealing, and so Klein decided to go back one year to Oxford, and start his professorship the next year at the Wharton School in Pennsylvania.

Klein recalls that Kravis, as Associate Dean, did a great job in liberalizing the curriculum of the Wharton School, which had an unusual BA program of four years, while most business schools focused on MBA programs. "Kravis made the Wharton curriculum of the 1960s so liberal that as much as 80 – 85 percent of an undergraduate's studies were indistinguishable from those of many BA students in social sciences in the College at Pennsylvania" (Klein 1993, 176). To Klein, "the situation was an open ticket to develop econometrics," and to build new

large-scale macroeconometric models; to that end, he got a grant from the Rockefeller Foundation, which “looked like a big grant then, but [which] was rather small for [1980s] standards.” In any case, Klein was eager to “return to American modeling, to take up a lot of the issues that had come up at various times.” Klein, for instance, wanted to use sample-survey data for indicators of both consumer attitudes or investment intentions, something that he had already been working on back in Michigan. After his experience in Oxford with the model of the UK economy, Klein also insisted on his idea that the models should be quarterly to improve forecasting results. The use of quarterly data, of course, entailed new and fascinating challenges for the econometric modelers, with involved problems of seasonal adjustment or the specification of lag structures within the model. But quarterly data were becoming available in the United States and this was an opportunity not to be wasted. In addition, the use of data of a higher frequency by macroeconometric model builders, meant going a step further in the direction of the NBER’s painstaking approach. To Klein (1964), the NBER had “often made the point that annual data are inadequate in business cycle analysis,” and so using quarterly data in the new models was “to recognize that we ought to try to do better. Without going to the extreme that the NBER reaches in doing most of its analysis with monthly data, we in econometric model-building research ought to go at least as far as the construction of quarterly systems” (11). That was precisely the objective of the Postwar Quarterly Model. When Klein “got to Pennsylvania [he] started teaching statistics and econometrics and [...] went right to work on the problem of recreating an American model” (Klein and Mariano 1987, 428).

Computing facilities had by now gained more importance in sciences. The University of Pennsylvania was a leading computational center, equipped with the first large-scale computer of the 1940s: The Electronic Numerical Integrator and Computer (ENIAC). By the time Klein arrived in Pennsylvania, the University had “later-generation equipment from UNIVAC [Universal Automatic Computer],” but the problem was that there was not much software for

econometrics. And so, like in Michigan, much of the work had to be done still as a combination of hand- and computer work. Together with Gerry Adams, Klein established the Economics Research Unit (ERU), and the Econometric Forecasting Unit (EFU). Those units were funded with money from the Ford Foundation, and the National Science Foundation (NSF), which, combined with the Rockefeller grant, soon allowed Klein to put together a new team at the Wharton School. One day, Klein had a “telephone call from a friend [he] had known in Canada when [he] was there in the summer of 1947 to build a Canadian model. [His friend, probably T. M. Brown,] said that he had an unusual student who was just a super computer expert on econometric techniques” (429). The “super computer expert” was Morris Norman, who became one of the new recruits, and a paramount person in the development of computation in econometrics. The other recruits were Ross Preston, George Schink, Michael Hartley, Tom Cooley, Chris Higgins, and Paul Taubman.

### 2.6.2. Consolidating large-scale macroeconomic modeling at the Wharton School

With a new team set up, a welcoming and dynamic Economics Department, and with the funding secured through the Rockefeller grant, Klein embarked, once again, in an ambitious macroeconomic modeling project to be carried out successfully throughout his whole academic life. To Bodkin et al. (1991, 88), from that point onwards, macroeconomic model-building experienced “a quantum leap,” with numerous models emerging in the United States. At Penn, the Postwar Quarterly Model, a direct descendant of the Klein-Goldberger model, was born. The model consisted of 29 behavioral equations, and eight identities, which made it considerably larger than its predecessor, and, of course, less aggregative.<sup>126</sup>

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<sup>126</sup> Compared to its immediate descendants such as the Brookings Model, which was made up of about 400 equations, the Quarterly Model represents, with hindsight, a medium-size model. But in the late 1950s, and early 1960s, this model was definitely seen as a large-scale macroeconomic model.

At first, the forecasts produced by the Quarterly Model were “sent around to friends and people” in the Kennedy administration. Soon, however, a whole industry of macroeconomic modeling started to gravitate around Klein’s figure. In this new stage, macroeconomic modeling would be directed not only by research institutions and universities, but also by private companies. Klein picks up the story:

One day some economists visited from General Electric and said they had an idea [...] about data banks and about model building with the data from the banks. Their idea was for us to provide their corporation with results, and I thought that the proposition was interesting. Another day, an economist, a former student, from Standard Oil of New Jersey asked if I could help them build a model for forecasting in their economic research department. Still another day, an IBM economist came with the same request [...] I started thinking about this. Many large American corporations were individually starting their own projects. Wouldn’t it be more sensible if we were to form a small consortium and pool resources to do everything here? I had lunch in New York with economists from [various companies]. We decided it looked like a good idea. I came back [to Penn] and talked to Willis Winn, the Dean [who] liked the idea and said he would back it up [...] The original five were IBM, Deere, Bethlehem Steel, Standard Oil, and GE [...] We put models together and discussed tables and results. We sat in a small Wharton seminar room and looked at the results four times a year. That was the start. Then, by word of mouth, people heard about us from other companies and asked if they could be included. The project started to grow on its own momentum, and we had probably close to a dozen participants in a short time (Klein and Mariano 1987, 429-430).

This alliance between Klein and the private sector, established the bases for the formal creation, in 1969, of a corporation to manage the model and the forecasts: The Wharton Econometric Forecasting Associates (WEFA), which Klein formed together with F. Gerard Adams and Michael Evans. This association between Klein and the private sector was important at least for three reasons. First, as Klein (1987) recalls, the Wharton School had an increasing interest in finding new financial resources, since the grants provided by the Rockefeller to Klein, and those by the Ford Foundation and the NSF given to Penn, had been conceived as “startup money,” and were likely to disappear soon. Klein perceived this money mainly to support graduate students. “There was a *quid pro quo* arrangement with the industrial and the business sectors. We collected money from them, and we supported about fifteen students in the

pipeline” (429). Second, improving his contact with real actors in the economy could open the door for Klein’s macroeconomic modeling to become influential in the decisional sphere. Yet, this influence was not to be exerted by Klein as an individual, since he was never interested in figuring as the savant counselor of kings.<sup>127</sup> Instead, the kind of influence he wanted to establish was one in which a whole team of modelers-experts, organized under an institutional roof, with a specific division of labor, and supported by a robust econometric tool, could lay their scientific results, including forecasts, so that others (government officials, entrepreneurs, academics) could use them.

The third important aspect about Klein’s association with the private sector lies in his gaining of trust as an economist in the public sphere. Indeed, it must have been hard for someone like Klein to find his place as an intellectual in a narrow-minded and profoundly conservative postwar US-American society, capable of producing phenomena such as McCarthyism. Both in symbolical and practical terms, this alliance was beneficial for Klein and for the companies. Yet, the alliance truly took off, when Klein met with his friend Leonard S. Silk who was the economics editor of *Business Week*.<sup>128</sup> At that occasion, Silk warned Klein that he could not provide information confidentially in the university, “because everything has to be in the open, everything has to be made available to the public at large,” and proposed him to make this information available to *Business Week* and to publish an article out of it (Klein and Mariano 1987, 430). Once Klein’s forecasts were “written up in *Business Week*, people came from all over, [and the] project grew [...] so fast that we couldn’t manage it between classes,” and WEFA became necessary. Klein, as well, became an important figure for the private companies.

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<sup>127</sup> Apart from providing advisor in Jimmy Carter’s presidential campaign, Klein stayed out of the public administration, and never accepted any permanent position in the government.

<sup>128</sup> Silk (1918-1995) was a US American economist and journalist, who played a paramount role in establishing the idea of “economics as news.” According to Silk (1992), “as an economic journalist, [he had] sought [...] to demonstrate that behind the tone, economics has nonstupid and nonobvious things to say.” For a fascinating account of Silk’s influence see Mata (2011).

2.6.3. Lawrence R. Klein, Clark Medalist 1959

The first time that Klein was considered for the John Bates Clark Medal (JBCM), he was only 31 years old. This was in 1951, only four years after his mentor Paul Samuelson was awarded the first JBCM. That year, Klein was the youngest economist among ten considered for nomination by the Committee on Honors and Awards of the American Economic Association (AEA). The complete list consisted of: Moses Abramowitz (born in 1912); Joe S. Bain (born in 1912); Abram Bergson (born in 1914); John T. Dunlop (born in 1914); Milton Friedman (born in 1912); Lloyd A. Metzler (born in 1913); Jacob L. Mosak (born in 1913); and Melvin W. Reder (born in 1919). That year the award was for Milton Friedman.

For the next award, in 1953, the nomination was somehow flawed since the very beginning. In March, Norman S. Buchanan, former professor of Klein at UC Berkeley (see section 2.3 above) and chairman of the Special Committee on the JBCM, was “puzzled” because he had no “prior knowledge that he was a member of the Committee on nominations (letter from Buchanan to Bell, from March 16, 1953, AEAP, Box 40, folder Committee Honors and Awards). In fact, on April 5, Buchanan sent a letter to Secretary Bell that shows that Buchanan had taken up the nomination issue too late, and that his list of nominees might fall short. In that letter, Buchanan asked what it meant for the Medal to be an “American economist,” and whether somebody like Don Patinkin, for instance, who had studied in the US (and was born in Chicago), but was now Professor in Israel would count as “American” or not. Buchanan also raised a question related to the age of the awardee. It was clear that the medalist had to be under 40, but when should he be under 40; was it at the moment of the nomination, or when the medal was actually awarded?<sup>129</sup> Secretary Bell, who could not give a definitive response and asked President Hoover and former chairman Sharfman for confirmation, believed that foreigners and “even Canadians” were excluded for consideration, and that

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<sup>129</sup> Letter from Buchanan to Bell, from March 5, 1953, AEAP, Box 40, folder Committee Honors and Awards.



candidates who were 40 at the moment of the award had also been ruled out in the past. Both Hoover and Sharfman (now Klein's colleague in Michigan) confirmed Bell's answer, but did not refer to the particular case of Patinkin, who in the end was not nominated, although he was born in 1922 (Letter from Sharfman to Buchanan, April 18, 1953, AEAP, Box 40, folder Committee Honors and Awards).

Apart from Buchanan, the Special Committee on the JBCM that voted for the medalist was composed by 12 additional members: Hoover, Burns, P. T. Ellsworth, Condliffe, Hart, Boulding, L. Reynolds, Gerhard Colm, David Wright, Frank Knight, and Williams. Compared to the 12 young economists proposed for the 1951 award, the list for 1953 with Bergson, Domar, and Klein appeared somehow short. On May 12, Chairman Buchanan informed Secretary Bell that the Special Committee had completed its work and wished to report the results of the deliberations and balloting: "Bergson has received the highest number of votes, and Domar is second, and Klein is third" (Buchanan to Bell, letter from May 12, 1953, AEAP, Box 40, folder Committee Honors and Awards). According to these results, the John Bates Clark Medal should have gone to Bergson, but that year no award was conferred. In fact, 3 out of 12 Committee members (Ellsworth, Knight, and Wright) had abstained from voting (see figure 1):

Figure 1: Ballot results, Special Committee on the Clark Medal Award

	Bergson	Domar	Klein
✓ HOOVER	1	2	3
BURNS	3	1	2
ELLSWORTH			
CONDLIFFE	1	2	3
HART	1	3	2
BOULDING	1	2	3
REYNOLDS	1	2	3
COLM	2	1	3
✓ WRIGHT			
KNIGHT			
✓ WILLIAMS	1	2	3

Source: AEAP, box 40, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

The general feeling among the Committee members was that the list was short, and that the nominees did not fulfill the standards. In a letter to Bell dated June 11, Wright said that he could not vote for any of the three nominees, that Knight had influenced his decision to abstain, and that there should be no award unless other names were presented.<sup>130</sup> Ellsworth too, could not make up his mind because he felt that none of the candidates was “extremely outstanding,” and so he “had omitted to send [his] vote,” but was willing to listen to arguments in the December meetings.<sup>131</sup> Another member of the Committee conveyed that he felt “very uneasy about [his] ratings of these 3 men, only one of whom [he knew] personally, and most of whose work [hid] outside [his] field of special competence.”<sup>132</sup> Hoover, too, thought that the whole JBC Medal issue should be held in abeyance until the meeting of the Executive Committee in Washington, and that he personally favored not making the award that year.<sup>133</sup> Although the Clark Medal was not awarded in 1953, Klein was still very young (33), and other chances awaited for him to be awarded medalist. Even more so, since at the time, Klein was launched in his ambitious project of building a macroeconometric model of the United States in Michigan, and had continued a very productive period of publications (see section 2.5).

The nomination process for the following years went more smoothly awarding James Tobin and Kenneth J. Arrow in 1955 and 1957. For the 1959 award, with Clair Wilcox as Chairman of the Special Committee, Burns as President, and George J. Stigler as Vice President of the Executive Committee, 12 economists were considered through a system of votes that gave “5 points for the first choice, 3 for second, and 1 for third.” The results were: Lawrence R. Klein (23pts.), William J. Baumol (16 pts.), Robert M. Solow. (7pts.), Carl Kaysen

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<sup>130</sup> Letter from Wright to Bell, June 11, 1953, AEAP, Box 40, folder Committee Honors and Awards.

<sup>131</sup> Letter from Ellsworth to Bell, July 28, 1953, AEAP, Box 40, folder Committee Honors and Awards.

<sup>132</sup> Letter to Bell, June 16, 1953, AEAP, Box 40, folder Committee Honors and Awards. I was not able to identify the writer of this letter who just signed it as “Eve.” Yet, Given the list of members of the Committee, and the first names that I could identify, this letter must have been sent either by Condlife or Williams.

<sup>133</sup> Letter from Hoover to Bell, July 20, 1953, AEAP, Box 40, folder Committee Honors and Awards.

(5 pts.), Thomas C. Schelling (2pts), Arnold C. Harberger (1pt.), and John S. Chipman, Carl F. Christ, Otto Eckstein, Hervey M. Leibenstein, John R. Meyer, and Albert Hesse, zero points.<sup>134</sup>

Just one year before he turned 40, and after 8 years of nominations, Klein was finally awarded the 1959 John Bates Clark Medal.

In this way, Klein closed an important but difficult first period of his career, and was, after much struggling, finally established at the top of the profession and ready to continue to work, teach, and counsel. This time, however, enthusiasm, would not be his only companion; maturity, respect, and admiration now also made part of his professional life in any scenario. Paul A. Samuelson's words congratulating both Klein "and the American Economic Association on their awarding [Klein] the Clark medal" must have been one of the satisfying and gratifying moments. After all, as Samuelson himself explained, he had a fourfold interest in the matter:

First, I am one of your proud teachers. Second, as a member of the Honors Committee, it is my duty to see that the Association gets the best man. Third, as a former Clark medalist, it is to my selfish interest that it not be down graded. Finally, and most important, as your friend I am glad to see you get this richly merited recognition (Samuelson, letter to Klein, April 13, 1959, PASP, box 45).

## 2.7. Concluding Remarks

There are two ways of reading the early years of Klein's fascinating academic life from the perspective of the history of twentieth century economics. First, at the individual level, Klein's life reads as an episode colored by countless encounters and experiences that molded his identity as an economist and as a macroeconometrician. Second, at the level of the economics discipline, Klein's academic life reads as a focal point, representative of a multiplicity of other life-stories embedded within the same historical, social, political, and disciplinary context of

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<sup>134</sup> Letter from chairman Clair Wilcox to President Burns, March 17, 1959, AEAP, Box 47, folder Committee Honors and Awards.

postwar transformations. Both readings are important for the type of history I am trying to construct in this dissertation.

On the one hand, the first reading allows us to “repersonalize” the history of macroeconometric modeling, and to recognize this practice not only as a way to understand the world, but also as an “existential project” (Düppe and Weintraub 2014, xv) in which the macroeconometrician seeks for self-identification within a particular community, while forging, at the same time, a new kind of identity marked by the individual events of his life.<sup>135</sup> These events, in turn, affect scientific practices and reshape the economics discipline. On the other hand, taking Klein as a focal and representative point in the history of economics, provides us with a well-informed perspective to understand the complex dynamics and transformations that were happening within the discipline.

In particular, Klein’s economics career contrasts with the narrative that presents the US American post World War II period as one of “neoclassical” uniformity and domination. In fact, Klein’s way of doing economics shows that a great amount of pluralism remained, inherited from the eclectic and pluralistic interwar period. It also shows that despite its relative fragile position, mathematical economics and econometrics rapidly gained in importance, extending their networks from Massachusetts to California, and from Michigan to Illinois and Pennsylvania, supported by a weakened but still symbolic European network principally stemming from Norway, the Netherlands, France, and England. In addition, Klein’s life helps us to understand the institutional arrangements that were at stake both to build economic laboratories in the era of “big science,” and to develop new graduate and undergraduate programs in economics to teach particular econometric and mathematical methods first in specialized institutions, then in most of the institutions across the US.

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<sup>135</sup> Given the importance of Klein’s contributions to economics and his fascinating life, however, a biography of him, in the traditional sense, would be a perfectly legitimate subject to complete a Ph.D. thesis or a book.

Klein “happened to be fortunate to have studied at Berkeley and at MIT, where the subject [of econometrics] was first appearing” (Klein 1991a, 112), and to encounter people like Kuznets, Samuelson, or Wilson. Rather than “fortune,” however, Klein’s path reveals both the formation of important institutional conditions that allowed him to conduct the kind of econometric work that inspired and defined him, and his ability to do both integrate networks and communities and gain scientific recognition. It is not just that Klein “seemed to be” in the right place, at the right moment, and with the right people throughout his academic life, but that he quickly learned to identify which were the right places, moments, and people to be with, not in an opportunistic way, but because of his conviction and power to demonstrate that he was after something important. In addition, Klein’s brilliancy and geniality (Mariano 2008), provided him with the individual attitudes necessary to grant him access to the most selective community of economists.

In a nutshell, his collaboration with mathematically oriented economists and statisticians in Berkeley was enough to get him the necessary credentials to be noted by Samuelson. It was not so much the fact of being the first PhD graduate of MIT which marked Klein’s academic career, but rather the fact of being *Samuelson’s first PhD* student and friend, which truly opened a number of important possibilities for “Laurie.” Later, his work at the Cowles Commission and his collaboration with people like Marschak, Haavelmo, and Koopmans overshadowed the “disappointing” results of his first macroeconomic model. In addition, Klein’s relation with the European econometricians, symbols of the transformation of economics, as well as his award of the John Bates Clark Medal, were certainly central to consolidate his position in the economics community, which was confirmed later with the dissemination of macroeconomic modeling, and with his Nobel Prize in 1980. The period 1940s-1950s was not one of great achievements and success alone. This was also a period of important drawbacks, disappointments, and difficulties. Getting a job after completing his PhD was not particularly

easy, and neither were the harsh criticisms directed by Friedman since the late 1940s (see Part II). Klein’s attempt to get tenured at the University of Michigan was also unsuccessful, even if the reasons for this failure are to be found in a very dark and anti-democratic period in the history of the United States, marked by McCarthyism. Klein’s early academic years tell us how he became to be a macroeconometrician due to both the possibility conditions of educational, disciplinary, and institutional transformation, as well as to his own personal search for a new scientific identity.



# Reconciling Keynes and Tinbergen?

I hope that I have not done injustice to a brave pioneer effort. The labour it involved must have been enormous. The book is full of intelligence, ingenuity and candour; and I leave it with sentiments of respect for the author. But it has been a nightmare to live with, and I fancy that other readers will find the same. I have a feeling that Prof. Tinbergen may agree with much of my comment, but that his reaction will be to engage another ten computers and drown his sorrows in arithmetic. It is a strange reflection that this book looks likely, as far as 1939 is concerned, to be the principal activity and *raison d'être* of the League of Nations.

*John M. Keynes, 1939*

I want to apologise for not having been clear enough in some of my arguments [...] As to the real controversies – apart from a number of misunderstandings of Mr. Keynes's on mathematical questions – I must admit that in my view the method under discussion promises – and actually yields – much more than Mr. Keynes thinks. Since the proof of the pudding is in the eating, I hope Mr. Keynes and other critics will give more attention to the economic premises, and especially that competing 'explanations' of actual series representing some economic phenomena will be given, in order that the 'public' may choose!

*Jan Tinbergen, 1939*

### 3.1 Introduction

The Controversy between John M. Keynes and Jan Tinbergen on the role of econometric method to test economic theory has remained an event of paramount importance to understand not only the historical evolution of econometrics, but also some fundamental methodological discussions in economics.<sup>136</sup> Even after Keynes's (1939) explicit recognition of the arduous and pioneering nature of Tinbergen's work, *Statistical Testing of Business-Cycle Theories*, a general impression persisted among economists and historians of economics, suggesting that his critique was not only ruthlessly destructive but also uninformed (Morgan,

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<sup>136</sup> For other accounts of this controversy see for instance Patinkin (1976), Stone (1978), Hendry (1980), Zellner (1982), Pesaran and Smith (1985a), Carabelli (1988), Morgan (1990), Keuzenkamp (1995), Garrone and Marchionatti (2004), and Maas (2014b).



1990). And so, the relevance of Keynes's claims is attenuated and his critique is sometimes dismissed as one in which "Dear Old Maynard," overwhelmed by the mathematical and statistical sophistication of the new approaches, would just be grumping about the methods of the younger economists.<sup>137</sup>

Like Pesaran and Smith (1985), I do not "intend to dissect this debate for its lessons" in the spirit of "military historians re-fighting Waterloo," where "[despite] the merits of his campaign, Napoléon loses every time the battle is refought, [as] does Keynes" (Pesaran and Smith 1985, 134); my aim is rather to take a fresher look at the controversy and to assess whether the econometricians' reaction after the critique was to "engage another ten computers and drown [their] sorrows in arithmetic," as Keynes put it, or whether econometricians took Keynes's methodological critique seriously into account, and thought about alternative ways to deal with the problems that were raised.

One of the most memorable alternatives thought of by the econometricians was the program of structural econometrics developed at the Cowles Commission.<sup>138</sup> In the 1940s, Jacob Marschak assembled a team to remake Tinbergen's model (Klein 1991; Bjerkholt 2014a), providing a constructive reaction to Keynes's critique, which did certainly not consist on the econometricians drowning their sorrows in arithmetic calculations. The project was one of high ambitions, not merely because of the sophistication of the mathematical and statistical rigor, but also because of the great care attributed to the role that economic theory should play in the econometric exercise.<sup>139</sup> For this purpose, Marschak recruited, in 1944, the

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<sup>137</sup> There are other accounts that take Keynes's criticism very seriously into account. See for instance David Hendry (1980) and Don Patinkin (1976).

<sup>138</sup> Commentators hold divided opinions about the structural econometric program at Cowles. On the one hand, even if they remain critical, historians of econometrics tend to appraise the Cowles program in a rather positive way, see for instance Epstein (1987), Morgan (1990), Qin (1993) and Nell and Errouaki (2013). On the other hand, econometricians or "practitioners," tend to be more critical appraising the program in a rather negative way. See for example Liu (1960), Hendry (1980), Sims (1980), or Leamer (1983; 2010).

<sup>139</sup> On this, see for instance Haavelmo's (1944), Koopmans's (1947) review of Burns and Mitchell's (1946) book, which provoked the *Measurement without Theory Controversy* at the end of the 1940s.

young economist Lawrence R. Klein who had just completed his PhD thesis on *The Keynesian Revolution* (1947).<sup>140</sup> Klein, therefore, found himself in a peculiar position only five years after the *Keynes versus Tinbergen Controversy* (1939-1940) had taken place: on the one hand, Klein was one of the experts on Keynes's thought (at least in the United States), and on the other hand, he was explicitly recruited to improve Tinbergen's work.

Klein's position in 1944 is fascinating, since he finds himself between two different visions of empirical economics: on the one hand, there is Tinbergen's vision, which strives for the implementation of technical and rigorous devices from which to draw inferences. On the other hand, there is Keynes's vision in which a priori theoretical claims based on the observation of the world are the only possible way for discovering the actual functioning of the economy.<sup>141</sup> Klein tries to make the most of both visions, and so, he seems to be the right person to look at in order to assess the kind of reaction that econometricians provided to Keynes's critique. Klein's book *Economic Fluctuations in the United States* (1950, 1) begins with the clear objective of reconciling Keynes and Tinbergen.<sup>142</sup>

Tinbergen did a great service to the study of economics when he prepared his volumes on the statistical testing and measurement of business-cycle theories. This book [(Klein 1950)] is written in the spirit of Tinbergen's investigations and is intended as an improvement and extension of his results. As a consequence of the extensive theoretical discussions since 1936, when Keynes published his *General Theory*, it has become possible to formulate more sharply the structure of the economic system and thereby to gain added simplicity and accuracy not available to Tinbergen.

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<sup>140</sup> 1947 is the year of publication of Klein's first book *The Keynesian Revolution*, based on his PhD thesis. Klein completed his thesis in 1944 under Paul A. Samuelson's supervision. As a matter of fact, Klein was not only the first PhD student of Samuelson's, but he was also the first person ever to complete a PhD in economics at the, by the 1940s, nascent Department of Economics of the Massachusetts Institute of Technology (MIT). For a detailed history of MIT's Economics Department see Weintraub (2014) and for an account of its PhD program see Duarte (2014). For a more detailed account about the relation between Klein and Samuelson, see Klein (1987) and Bjerkholt (2014a).

<sup>141</sup> These characterizations of Tinbergen's and Keynes's visions are put in simplistic terms for the sake of the argument at this stage of the chapter. Yet, these visions will be characterized in a more detailed way through the rest of the chapter and will make more justice to the authors. To be fair, for instance, Tinbergen conferred an important place to a priori hypotheses to build his models as well.

<sup>142</sup> Klein (1950) resulted from his work at Cowles between 1944 and 1947. Yet, it was published only in 1950.

The purpose of this chapter is to ask whether Klein managed to reconcile two antagonistic approaches to the role that econometrics should play in testing economic theories and in finding out about the functioning of the economy. I argue that despite the fundamental differences between Keynes and Tinbergen, Klein's practice of macroeconomic modeling provided a way for reconciliation in, at least, five aspects. (1) Klein's *methodological structuralism* (Nell and Errouaki 2013) allowed him to undertake a rigorous thinking in theoretical terms to build up his models, as suggested by Keynes. (2) In the same line, Klein's methodology allowed (and asked from) the econometrician to include a greater amount of a priori knowledge and information into the models. (3) Klein clearly stated that the validity of his models was limited in time and space, and that models should be permanently revised, rethought, and re-estimated. Tinbergen would admit the same reasoning for the improvement of his models (Morgan and Magnus 1987), while Keynes would consider his own theories in a similar way, as temporal in time and space (Moggridge 1992).<sup>143</sup> (4) The political objective retained by all three authors is also comparable and compatible. Keynes, Tinbergen and Klein, envisaged models as a sound tool for providing economic (and planning) advisory. Last but not least, (5) Klein thought of the econometric exercise not as a "once-and-for-all-job" (Klein and Goldberger 1955), but as a painstaking activity that should be continued and remade every time new information became available.

Reconciliation was not reached for all the problems raised by Keynes, however. There was, in particular, one problem that was just irreconcilable. Klein could not do much about Keynes's fundamental claim on the impossibility of using econometrics for testing economic theories. Klein, like Tinbergen, went beyond to accept not only that econometrics constituted a legitimate way for testing economic theories, but also that it could provide sound criteria for

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<sup>143</sup> Keynes's reasoning and theorizing was of an evolutionary nature: his views about the world were not static and these changed depending on the particular circumstances (Meltzer 1988). Commentators on Keynes, like Meltzer (1988) or Moggridge (1992), argue that the Philosopher G. E. Moore was very influential for Keynes to adopt this posture.

choosing the best available theory or *theories*.<sup>144</sup> According to Klein (1950, 1) – and yet at the antipodes of Keynes – the accuracy in forecasting and in evaluating economic policy would be the criterion that econometrics would provide in order to choose the best economic theory available:<sup>145</sup>

We want to do more than is suggested by the title of Tinbergen’s work, [...] i.e., more than the mere testing of business-cycle theories. We want also to discover the best possible theory or theories which explain the fluctuations that we observe. If we know the quantitative characteristics of the economic system, we shall be able to forecast with a specified level of probability the course of certain economic magnitudes such as employment, output, or income; and we shall also be able to forecast with a specified level of probability the effect upon the system.

According to Klein, one of the most important contributions of Keynes’s *General Theory* was that it had provided the econometricians with the possibility of formulating the structure of the economic system in a sharper way. For econometrics this meant that the specification of the model would gain both in terms of simplicity and of accuracy. In this sense, Keynes’s *General Theory* was important for econometricians like Tinbergen and Klein at least for two reasons: (1) it not only provided a system of thought susceptible of being translated into a coherent and complete econometric model, which constituted Tinbergen’s first test of an economic theory (Morgan 1990), but (2) it also provided the economic theoretical foundations legitimizing state intervention.<sup>146</sup> “After Keynes, political intervention in a market economy was no longer taboo; counter-cyclical budgetary policy became at the very least a meaningful idea, unemployment a failure of the market” (Maas 2014b). Keynes’s work, as well as the

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<sup>144</sup> Klein would defend a pluralistic approach in his way of building up models (Pinzón-Fuchs 2014). His idea was that there was no such a thing as “one best economic theory” but rather that there were many plausible theories that provided useful information for specifying the econometric models.

<sup>145</sup> It is worth noting that Klein remained very optimistic throughout his whole life about the forecasting and policy evaluation accuracy of large-macroeconometric models (see Klein *et al.* 1987; and Klein 1991; 2013). This was also the case, even during the 1970s when large-scale macroeconomic modeling suffered a harsh attack, notably by Robert Lucas (1976). On Klein’s reaction to Lucas’s Critique see Goutsmedt *et al.* (2015).

<sup>146</sup> There were, of course, other contributions by various economists that were completely independent of Keynes’s work, which contributed to the gaining of legitimacy of state intervention. Also some historical events like WWI or the New Deal were of paramount importance in this matter. On this see for example Morgan and Rutherford (1998).

work of various New Deal economists (see chapter 1), had paved the way for econometricians like Klein to undertake one of their most cherished objectives: economic planning, which was a life-long objective not only for Tinbergen (Tinbergen 1981), but also for Klein (1947a; 1991).

Both the Keynes *versus* Tinbergen controversy and its aftermath have to be situated within a particular moment in history: after the Great Depression and the New Deal in the United States, and at the outbreak of World War II. These situations changed the idea that economists, politicians (and even the general public) had about the role that the state should fulfill and also about the part that the economists themselves should play in this new conception of the state (Morgan and Rutherford 1998).<sup>147</sup>

This transformation in the political and economic programs also changed the way science, and in particular economics, was understood. In the United States, this particular context provoked “a cohered [vision of the economists] not about a tight theoretical agenda but around a particular view of science and a conviction of the inadequacy of the unregulated market” (2), which led to important changes in the kind of tools judged necessary for “managing the economy” (Desrosières 2003). The controversy is then situated at a turning point in history, where the vision of economics as a science was changing, driven not only by the advancement of theoretical and technical contributions – after “The Years of High Theory” (see Shackle 1967) or the rise of econometrics, for instance – but also by political, historical and social factors, in which several economists wished to bring economics closer to the natural sciences in order to gain legitimacy.

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<sup>147</sup> Desrosières (2003) proposed to study these new roles of the state by means of the framework of *political constellations*, which was further developed by Armatte (2010) through the concept of *régimes politiques*. In his 2003 paper, Desrosières defines “five typical historical configurations”: (1) Direct Intervention, (2) Classical Liberalism, (3) the Welfare State, (4) Keynesianism and (5) Neoliberalism. It is of course worth noting, as Desrosières does, that “the five configurations are not meant to describe successive stages in a historical progression, nor are they historically or logically exclusive. In concrete historical situations, they are often mixed together. They [are] idealized in this way only to provide a grid on which to arrange the history of the statistical [and I would add economic] tools employed [in] each [*régime*]” (Desrosières 2003, 554).

This turn in economics had consequences for the practices of economists. Theoreticians like Keynes, who were “involved in an inductive quest in which [they] constructed a coherent plot based on a multiplicity of diverse sources,” had to give way to economists like Tinbergen for whom “induction was identified with the application of statistical techniques so as to extract the maximum information out of a given data set” (Maas 2014b). A “generational change” (Backhouse 1998) was also taking place in economics during the 1930s and 1940s, at least in the United States. Klein, one of the prodigies at Cowles (Bjerkholt 2014a) armed with the new “rigorous and formal” practices and tools of this new generation, became an influential agent in this generational change.<sup>148</sup>

Klein, however, cannot be merely seen as a young economist who was just thinking about doing sophisticated mathematics and statistics instead of doing economics. Quite the contrary, Klein remained very skeptical about the role that these sophisticated methods could play in the improvement of econometric results (Klein 1960; 1991), frequently asserting that “[the] adoption of more powerful methods of mathematical statistics is no panacea” (Klein 1960, 867). Klein’s way of doing econometrics, then, was far from fulfilling Keynes’s fear of the econometricians finding refuge in mathematics and statistics. Again, the general image that econometricians held about Keynes’s critique is that it was unfair with Tinbergen and that Keynes did not understand what econometrics was all about.<sup>149</sup> Klein’s way of doing econometrics shows, however, not only that there is plenty of place for revising arguments and procedures, and hence for reconciliation, but also that this reconciliation might enrich

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<sup>148</sup> Klein was only 24 years old when he was recruited at the Cowles Commission with the task of “redoing Tinbergen” (Bjerkholt 2014a).

<sup>149</sup> Keynes’s “misunderstanding” of econometrics would not be necessarily surprising, since econometrics had not reached a “stable and clear” meaning by that time. This lack of understanding did not have to do with economists lacking the capacities or training to understanding what econometrics was about. Even if Keynes’s mathematics might have been “rusty” by the 1930s, like Stone (1978) puts it, nobody should doubt of Keynes’s capacity of understanding highly complex problems, if mathematical, statistical, or of whatever nature. Other mathematicians as renowned as Edwin B. Wilson, who probably understood what econometrics was all about, also expressed his unease with econometricians’ way of presenting their contributions. See Wilson (1946, 173) discussed in chapter 2.

the way of doing econometrics and of understanding its limited scope.

### 3.2. Keynes on probability and on the impossibility of drawing inferences in economics

In September 1930, the Assembly of the League of Nations “decided that an attempt should be made to co-ordinate the analytical work then being done on the problem of the recurrence of periods of economic depression” (Loveday in Haberler 1937, v).<sup>150</sup> After at least five years of hard work Gottfried Haberler and the League still recognized that “there [were] many points at which no definite solution can be proposed” (vi), and yet, their main objective remained to provide a general synthesis of the theories explaining the business cycle.

This synthesis, however, is more than a simple patching together of the theorems of others: it is an attempt to create a living and coherent, if incomplete, theory on the basis of the knowledge at present available (Loveday in Haberler 1937, vii).

The first stage of this effort resulted in Haberler’s study *Prosperity and Depression*, published in 1937, which consisted on the examination of the existing theories of the business cycle.<sup>151</sup> After Haberler’s work, a second stage in the League’s enterprise was still to be undertaken, where the “intention [was] to confront the theories with the ascertainable facts [...] with a view at once to testing the accuracy of the explanations or partial explanations of the cycle now current and to furnishing the basis of fact necessary for the further development of theory where theory is weak, views are discordant or doubts exist” (Loveday in Haberler 1937, vii).

Jan Tinbergen, as Keynes (1940) himself recognized, seemed to be one of the most “gifted and delightful” persons of the time to undertake the task commissioned by the League

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<sup>150</sup> Alexander Loveday (1888-1962) was a British economist working at the League of Nations. He had joined the Secretariat in 1919, and then led the studies on the business cycle undertaken by the League as Director of the Financial Section and Economic Intelligence Service of the League of Nations in 1931, and later as Director of the Economic, Financial and Transit Department in 1939 (see Clavin 2003).

<sup>151</sup> As noted by Morgan (1990, 108), Haberler’s book was largely circulated in a draft version before publication, and that is why there might be some confusion about the date of Tinbergen’s commissioning in 1936, and what would seem a posterior publication of Haberler’s book in 1937.

of Nations.<sup>152</sup> Tinbergen was the most suitable economist for this task since he was already immersed in the development of a macroeconometric model for the Dutch economy (Tinbergen 1937), which had yielded quite promising results (Morgan 1990). His econometric approach, even if not very popular at the time, seemed to be exactly what Loveday and the League of Nations were looking for:

The establishment of a system of equations compels us to state clear-cut hypotheses about every sphere of economic life and, in addition, to test them statistically. Once stated, the system enables us to distinguish sharply between all kinds of different variation problems. And it yields clear-cut conclusions. Differences of opinion can, in principle, be localised, i.e. the elementary equation in which the difference occurs can be found. Deviations between theory and reality can be measured and a number of their consequences estimated. Finally, the results of our calculations show, apart from many well-known facts, that, as regards the types of movement that are conceivable, there exist a number of neglected problems and of unexpected possibilities (Tinbergen 1937, quoted by Morgan 1990, 108).

Tinbergen’s work offered everything the League of Nations was looking for: a method to test economic theories; a method to localize differences in opinion between researchers and, maybe, a solid basis to resolve these differences. His method also appeared to provide a tool for measuring economic variables and phenomena, as well as for discovering new relations. In 1936, the League of Nations commissioned Tinbergen to undertake the statistical tests of the business cycle theories compiled in Haberler’s work (Morgan 1990, 108).<sup>153</sup>

It is in this context that the controversy between Keynes and Tinbergen took shape. Tinbergen’s work at the League of Nations was circulated before publication, provoking “interesting and long-lasting discussion on the role of econometrics in theory testing” (Morgan 1990, 121). The controversy was triggered in 1939 after Keynes’s review “Professor Tinbergen’s Method.” Note, however, that Keynes’s review was on Tinbergen’s *Volume I*, i.e.

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<sup>152</sup> “I [Keynes] felt once more as I had felt before, that there is no-one more gifted or delightful or for whose work one could be more anxious to give every possible scope and opportunity [than for Tinbergen’s]” (Keynes 1945, quoted by Stone 1978, 125).

<sup>153</sup> It took Tinbergen two years before he published the two volumes of his study, *Statistical Testing of Business-Cycle Theories*. *Volume I* dealt with an explanation of the econometric method to be used, and with three concrete examples of its application. *Volume II* basically presented the huge macroeconometric model of the United States.



*A Method and its Application to Investment Activity* which presented the principles of multiple-correlation analysis, and not specifically to the macroeconometric model of the US presented in *Volume II*.

As I mentioned before, some accounts of the controversy have treated Keynes as someone who disliked econometrics and who did not understand very much what econometrics was about.<sup>154</sup> This version of the story, however, discourages the attention of readers and lessens the importance and accurateness of Keynes's critical claims. Some authors like Lawson (1985a), Pesaran and Smith (1985a), Carabelli (1988) and more recently Keuzenkamp (1995; 2000), have done an effort to take Keynes's claims seriously into account, even if, I believe, some of his claims might have changed, had he reviewed Tinbergen's *Volume II*. The present section presents Keynes's conception of probabilities, statistical inference and science, which is of paramount importance to understanding the pertinence of his criticisms on econometrics.

### 3.2.1. Understanding Keynes's critique of econometrics through his Treatise on Probability

As stated by Lawson (1985a) and Carabelli (1988), if one is willing to understand the most fundamental arguments of Keynes's critique that go beyond the technical issues, one has to go back to the position Keynes held on science, probability, and inference, in his work and especially in his 1921 book *A Treatise on Probability*. That is the aim of this section.

“Keynes's theory of probability represents a major development within the logical

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<sup>154</sup> Stone (1978) who became quite close to Keynes during the years of WWII and remained so until his death in 1946, tells a story of Keynes's view of econometrics, where Maynard appears less stringent towards econometrics, and where he would have even tried to “cook” his own “econometric pudding” in Cambridge during the War. According to Stone, Keynes had been very much involved in the *Cambridge Research Scheme of the National Institute of Economic and Social Research* when his harsh criticism on Tinbergen's work appeared (Stone 1978). Other Cambridge economists involved in this project apart from J. M. Keynes as chairman, were Richard Kahn, Piero Sraffa, David Champernowne, Joan Robinson, Austin Robinson (secretary) and Michal Kalecki as statistician (*ibid.*)

tradition of probability” (Lawson 1985a, 117).<sup>155</sup> One of the main differences between the frequentist approach to probability, that Lawson (1985a) considers to be “the more common notion held within scientific tradition,” and the logical approach to probability held by Keynes, is that the frequentist view would consider probability as a “property of the actual physical world, whilst in the logical account [probability] is a property of the way people think about the world” (117). This, however, does not mean that to Keynes probability is subjective, because it is not the case that people “are logically free to set their own values for degrees of belief on the basis of human caprice.” In Keynes’s view, probability “is concerned with the degree of belief it is *rational* to hold in certain conditions” (117) and so, probability remains objective. Keynes’s focus thus is not on the empirical but on the rational content of probability. Rationality in this case, as Carabelli (1988) puts it, has to be considered “as practical and contingent, utterly separated from truth and relative to actual limited cognitive conditions” (234). And so, probability must be necessarily “grounded on ordinary practice” and it has “to be approached by the tools of ordinary language and everyday qualitative and analogical reasoning, rather than by formal and artificial language and by purely quantitative, mathematical tools” (234).

Keynes’s account of probability “is concerned not so much with truth as with what is rational to believe given the evidence” (Lawson 1985a, 118). “In this account all empirical arguments require an initial probability which is usually acquired by analogy” (124), i.e., to the likeness of the objects being compared. The initial probability, however, “may be raised towards certainty by methods of pure induction,” which depend upon the instances and observations made (124). Besides analogy and induction, Keynes adds a third way of getting to this prior probability when “things are self-evident”: introspection.

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<sup>155</sup> Other authors that belong to the logical tradition of probability would be Rudolf Carnap and Harold Jeffreys. An important difference between Keynes’s account and those of Carnap’s and Jeffreys’s, however, is that in the case of Keynes’s account, for most situations, it is impossible to assign numerical values to probability relations, while, for the other two authors, this is possible.

According to Keynes (1921, 276), then, “before the method of pure induction can be usefully employed to support a substantial argument” an *a priori probability* must always be found. “The objective of Keynes’s inductive method [...] is to develop generalisations and to analyse ways of increasing their probability both by limiting their universality and by examining the greatest possible diversity of instances” (Lawson 1985a, 117).

Keynes’s account of probability implies, however, the existence of some restrictive conditions for inference to be possible. Inference would be restricted to an assumption “concerning the character of material laws which, if true, would support the use of inductive methods” (Lawson 1985a, 154). Keynes refers to this assumption as that of *atomic uniformity* and explains that:

[The] system of the material universe must consist [...] of bodies [...] such that each of them exercises its own separate, independent, and invariable effect [If this were not the case] predictions would be impossible and the inductive method useless (Keynes CW VIII, 277).

For most cases, these conditions are impossible to be met in the context of economics, since “economics is essentially a moral science and not a natural science, [for] it employs introspection and judgments of value” (Keynes CW XIV, 297, quoted in Lawson 1985a, 130).

This means that, for Keynes, “economics too was considered [...] as a ‘logic, a way of thinking.’” The purpose of economics “was seen as that of studying the economic agents’ motives for acting in conditions of uncertainty within the framework of cognitive hypotheses rather than empirical causes” (Carabelli 1988, 235). These hypotheses “were linked to convention and practice,” and “were similar both to those of everyday experience and to artistic fictions [in which] one can find analysis and intuition” (235). To Keynes, therefore, economics was concerned with more than the description of facts, and, following his father’s classification, economics “not only mixed descriptions and norms, but was essentially operative; it was indeed merely an ‘art’ [J. N. Keynes 1890, 35]), that is a ‘system of rules for

the attainment of a given end” (35). The hypotheses of atomic uniformity could not be met in economics, since “unlike the typical natural science, the material to which [economics] is applied is, in too many respects, not homogenous through time” (Keynes CW XIV, 296).

### 3.2.2. The method of multiple correlation and its inapplicability to non-homogeneous economic phenomena

Following Keynes, then, homogeneity of phenomena was of paramount importance for inference to make sense. And so, this argument was decisive in his critique of econometrics given that economic phenomena were non-homogenous by nature. In relation to Tinbergen’s work, Keynes referred to this matter in a letter to R. Tyler of the League of Nations. To Keynes, “the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non-homogenous through time,” i.e. a “central question of methodology,” had to be scrutinized first.

If we were dealing with the action of numerically measurable, independent forces, acting with fluctuating relative strength on material constant and homogenous through time, we might be able to use the method of multiple correlation with some confidence [...] In fact we know that [...] these conditions [are] far from being satisfied by the economic material under investigation [...] The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, [...] there is no reason at all why they should not be different every year (Keynes CW XIV, 285).

According to Keynes, inference in economics would not make sense at any rate, given the lack of homogeneity of economic phenomena through time. Keynes’s plea against econometrics would heavily draw on his different way of characterizing economic phenomena compared to that of other economists, and, especially, compared to that of econometricians such as Tinbergen.

According to Keynes no inductive claim could be conducted from Tinbergen’s exercise, “not only [because of] the lack of uniformity in the factors of which no specific account is taken [but] also in the case of those which are included in the scheme” (Keynes 1939, 567).

The important point here is that economic variables (omitted or not) should maintain a certain amount of uniformity through time if the econometrician wanted to draw some inferences. Yet, the econometrician faced two fundamental problems here. On the one hand, again, inferential drawing would be impossible in economics, because of the nature of economic phenomena. The variables actually included in the econometric model might not fulfill the condition of uniformity in time, producing consequently a change in the relation with the other variables impossible to take into account by the econometrician. On the other hand, the researcher relying only on the “passive observation” (Haavelmo 1944, 16) of data would not always be able to include all the “potentially important” variables in his model, since there might be some variables which, even if relevant, do not vary during a particular period of time, making it impossible for the passive observer of data to consider them as explanatory.<sup>156</sup>

In the particular case of business cycles, Koopmans (1941) was much more optimistic than Keynes about the possibility of doing inferential drawings, given the nature of the phenomenon in question. Since the time span in any study of the business cycle would be short enough, a certain amount of homogeneity could be conserved that would allow for careful inferential drawings.

Business-cycle analysis differs from the analysis of economic equilibria in that it is likely to draw a greater benefit from statistical induction. The fact that it deals with short-run movements increases the possibilities of extracting from statistical observations information regarding the relations underlying those movements (Koopmans 1941, 158).

### 3.2.3. Converging visions on testing, prediction, and policy simulation

Since the 1950s, at least, one way of testing whether a model is a “good” model has been the assessment of its predictive accuracy. In the 1930s, however, prediction was far from being considered in this way. Neither Tinbergen or Keynes thought of prediction as an ultimate

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<sup>156</sup> See Boumans (2010; 2015, chapter 4).

criterion to evaluate model performance. Tinbergen, on the one hand, developed a complex “three-stage procedure” to evaluate his models (Morgan 1990, section 4.3). For Keynes, on the other hand, “there is nothing which [...] suggests that a model that predicts the evidence should receive greater support than a model that is constructed after the evidence is obtained [since] the relation between hypothesis and evidence is purely logical [and so] the issue of which appeared first, the hypothesis or the evidence, [would be] unimportant” (Lawson 1985a, 122).

Another way – Keynes’s way – of testing whether a model would be a “good” model was by implementing a special sort of policy simulation. This subsection discusses the econometricians’ and Keynes’s visions about what would account for a sound test of economic theory, i.e. whether it should be its predictive accuracy or the putting into effect of an economic policy based upon the model the economists is willing to test.

Hendry’s (1980) *three golden rules of econometrics*, “test, test, and test,” seems to echo four decades later Tinbergen’s optimistic beliefs in econometrics.<sup>157</sup> Prediction and “predictive accuracy,” in Hendry’s account, would provide a sort of severe test of econometric models to be accepted, if only provisionally, since a prediction would be presented in the form of a *bold* claim that is likely to be refuted (Lawson 1985a). While a model that would “merely accommodate” or fit the data, “is often considered to be *ad hoc*” (120), a model that yields a prediction is actually doing a defined, sometimes courageous, and above all, *bold* claim that can potentially be refuted, and hence, a claim that would constitute a good candidate for being subjected to a test. Both Tinbergen and Klein’s views of prediction as a means of providing severe tests might be considered as forerunners of Hendry’s, and so, this point constitutes one of the aspects where Klein would have a hard time reconciling Keynes and

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<sup>157</sup> Hendry (1980) suggested that “[the] three golden rules of econometrics are test, test and test; that all three rules are broken regularly in empirical applications is fortunately easily remedied. Rigorously tested models, which adequately described the available data, encompassed previous findings and were derived from well based theories would greatly enhance any claim to be scientific” (Hendry 1980, 403).

Tinbergen.

Haavelmo (1943) for instance, defended the idea of testing economic theory through econometrics, but was critical about Tinbergen's way of presenting his tests. Haavelmo would insist on the importance of "probability theory as the essential missing component which would make the [econometric] approach fruitful" (Morgan 1990, 128).

When we speak of testing theories against actual observations we evidently think of only those theories that, perhaps through a long chain of logical operations and additional hypotheses lead to *a priori statements about facts*. A test, then, means simply to take the data about which the *a priori* statement is made, and to see whether the statement is true or false.

Suppose the statement turns out to be true. What can we then say about the theory itself? We can say that the facts observed do not give any reason for rejecting the theory. But we might reject the theory on the basis of other facts or on other grounds. Suppose, on the other hand that the statement turns out to be wrong. What we can say then about the theory depends on the characteristics of the theory and the type of a *a priori* statement it makes about the facts [...]

If the statement deduced is assumed to be one of necessity, we would reject the theory when the facts contradict the statement. But if the statement is only supposed to be true 'almost always', the possibility of maintaining the theory would still be there [...] If the statement is verified, should we accept the theory as true? Not necessarily, because the same statement might usually be deduced from many different constructions. What we can say is that an eventual rejection of the theory would require further tests against additional facts, or the testing of a different statement deduced from the same theory. Each test that is a success for a theory increases our confidence in that theory for further use (Haavelmo 1943, 14-15).

Later, in 1944, Haavelmo added that:

What we want are theories that, without involving us in direct logical contradictions state that the observations will *as a rule* cluster in a limited subset of all conceivable observations, while it is still consistent with the theory that observation falls outside this subset 'now and then' (Haavelmo 1944, 1-2, emphasis in the original).

For Klein, accuracy of prediction constitutes one important criterion in order to decide whether a model was adequate or not (Klein 1987). For example, Klein's defense of large-scale macroeconomic modeling after Lucas's critique (Lucas 1976) during the 1970s and 1980s, was based, among other things, on his claim that macroeconomic models should

not be abandoned because they had made proof of accurate results in forecasting. Indeed, in a paper written in 1985, Klein (1985) provided a direct response to Lucas’s criticism, fiercely defending macroeconometric models. The first point of his defense consisted in asserting that these models had not failed to anticipate the inflationary surge of the US economy in the 1970s. Even if Klein recognizes that there was a period of ‘significant underestimation’ of the inflation rate by these models – in particular for eight quarterly forecasts between late 1973 and 1975 – he showed that, in the long-term forecasts, the error of the Wharton model was consistently less important when compared to other models.<sup>158</sup>

For Keynes (CW VIII), however, the “peculiar virtue of prediction [...] is altogether imaginary.” While “the question as to whether a particular hypothesis happens to be propounded before or after their examination is quite irrelevant.” On the contrary, it is “the number of instances examined and the analogy between them [that] are the essential points” (337) to assesses whether a hypothesis is plausible or not.

Accuracy in forecasting, then, does not constitute a testing criterion for Keynes. In fact, for Keynes the way to test a hypothesis is to put into effect an economic policy based upon it (Lawson 1985a, 123), which is quite similar to testing a model on behalf of its capacity to provide a sort of accurate policy simulation.

Klein’s (1947a) own observations about the way Keynes arrived at the provision of his theories show that policy matters and policy objectives were his main drivers behind the development of his theories. Only after having targeted a particular policy problem through economic intuition and a priori knowledge, economic theory would appear. A sound economic theory should not necessarily lay behind economic policy recommendations. It would be rather economic policy recommendations that should be sound, and it would be through their soundness that these policies would end up producing coherent economic

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<sup>158</sup> The comparison of the forecast capacity of the Wharton and the FED models is undertaken mainly by using McNees’s (1979; 1981) tabulations published in the *New England Economic Review*, who, according to Klein, would be “an objective referee” (Klein and Young, 1980).



theories. The test of an economic theory would begin and end with the necessity and the effects of a sound economic policy. As sound economic policies would constantly change with the normal flow of societal evolution, so would economic theories also endlessly adapt and change.

Economists can sometimes go very far in the advocacy of proper, sound policy measures on an inadequate formal theory. That such things are possible proves only that practical economics is simply common sense, while theoretical economics is ‘common sense made difficult.’ Keynes had a good idea as to what the troubles were in the economic system in the early years after the 1929 crash – in fact, even before the crash – and he supported policy similar to that built up around the *General Theory*, but he was not able to formalize his arguments into a satisfactory theoretical mold [...] *It was not his theory which led him to practical policies, but practical policies devised to cure honest-to-goodness economic ills which finally led him to his theory* (Klein 1947a, 31, my emphasis).

In a nutshell, to Keynes (1921) neither probability, statistical inference, or induction were intended to provide a description or an explanation of the world. They belonged to the branch of speculative knowledge and so they served as guides of life, but they were not subjected to empirical falsification (Carabelli 1988). Nor were scientific laws, which could not be considered as empirical laws. In a similar way, Keynes denied the predictive character of theories. Theories were tied up with action, and so they “had only a projective character, that is, [they] needed hypotheses to operate” (234).

### 3.3. The Controversy between Keynes and Tinbergen

This section deals with some particular aspects of the controversy between Keynes and Tinbergen. First, I will present five technical questions that were raised by Keynes in his critique of Tinbergen. I will state Tinbergen’s and Keynes’s positions on specific matters, to pass then, to the discussion of the conceptions of model that Keynes, Tinbergen and Klein held. Finally, I will treat Keynes’s skeptical attitude in the matter of replication in econometrics.

### 3.4.1. Five technical questions raised by Keynes

Keynes’s 1939 review actually contains six major points questioning Tinbergen’s work. The “first five questions concern the specification of econometric equations, [the] sixth, which [Keynes] believed to be in a ‘different department of the argument’ (Keynes 1939, 566), concerns the inductive and predictive value [of the] estimates” (Pesaran and Smith 1985, 138). I have given a more detailed discussion of the inductive and predictive aspects of the discussion in the first section of the chapter, precisely because of its relevance. In this section I will focus on the first five questions raised by Keynes, as well as in the answers provided by Tinbergen. When pertinent, I will integrate some contributions to the controversy by other authors like Frisch (1938), Friedman (1940), Marschak and Lange (1940), and Haavelmo (1941), criticizing or defending Tinbergen.

(1) States in modern terms, the first question raised by Keynes was about the problem of “omitted variables” in the equations. The econometric method, according to Keynes, was not, in itself, a tool for discovering the variables that were missing in the model. Econometrics would be constrained by the theory economists provided, and then, even if the variables proposed by the theoretician were *veræ causæ* explaining the phenomenon, this would not mean that econometrics could provide any help in completing the theory. The completion of the economic theory would only be possible at the level of the construction of hypotheses, which had to do more with intuition rather than with analytical reason, since it was driven by the necessity of producing a clear action under particular circumstances (Carabelli 1988).

Keynes, specifically, asked whether:

[He] was right in thinking that the method of multiple correlation analysis essentially depends on the economists having furnished, not merely a list of the significant causes, which is correct so far as it goes, but a *complete* list? For example suppose three factors are taken into account, it is not enough that these should be in fact *veræ causæ*; there must be no other significant factor [causing the phenomenon]. If there is a further factor, not taken into account of, then the method is not able to discover the relative quantitative importance of the first three. If so, this means that the

method is one neither of discovery nor of criticism (Keynes 1939, 560, emphasis in the original).

The direct implication of this claim is that econometrics should be regarded only as a tool of measurement (Boumans 2010). It would not be possible, then, to conceive econometrics as a tool for testing theories or for discovering new things about the world that would not be already contained in the theory.<sup>159</sup>

For Tinbergen, as for Keynes, it was clear that it was the economist who had to play the “heuristic role” in economics and not the econometrician. It was the economist’s responsibility to hand over the economic theories containing the *complete list* of explanatory variables or, in Keynes’s words, “factors, or *veræ causæ*,” since statistical theory was, on the one hand, restricted to economic theory, and on the other, statistical theory was not able to prove economic theory to be correct.

The part which the statistician can play in this process of analysis must not be misunderstood. The theories which he submits to examination are handed over to him by the economist, and with the economist the responsibility of them must remain; for no statistical test can prove a theory to be correct (Tinbergen 1939, 12).<sup>160</sup>

Yet, statistical theory could “prove [...] theory to be incorrect, or at least incomplete, by showing that it does not cover a particular set of facts” (12), and hence, “statistical theory” or econometrics, would contribute in recognizing that not all the relevant variables explaining the phenomenon had been included in the model. That is, in plain English, that the model

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<sup>159</sup> In fact, this idea about econometrics as a tool for measurement goes quite well in line with the traditional history accounting for the econometric program developed at the Cowles Commission during the 1940s; see for instance, Morgan (1990) and Qin (1993).

<sup>160</sup> Koopmans (1941) expressed the actual situation of the division of labor between the economist and the statistician in a more refined way, making the demarcation between the “mathematical statistician” and the “economist” less dramatic than it appeared in Tinbergen’s account. For Koopmans the economist should not only provide good theoretical knowledge, but he should also be well informed about statistical results obtained for other countries and for other periods of time. “While [the mathematical statistician] applies the type of reasoning and the procedures elaborated in statistical theory, the [economist] is not supposed to be of the too academic type versed only in abstract deduction from the ‘economic motive’. He is considered to have in addition an intimate knowledge of economic life and of the results of statistical investigations relating to similar countries and periods” (Koopmans 1941, 166).

has problems of omitted variables. Yet, Tinbergen recognized that even if a theory would fit the data, this mere fact would not warrant that it would be the “true” theory:

But even if the theory appears to be in accordance with the facts, it is still possible that there is another theory, also in accordance with the facts, which is the ‘true’ one, as may be shown by new facts or further theoretical investigations. Thus the sense in which the statistician can provide ‘verification’ of a theory is a limited one (Tinbergen 1939, 12).

It is worth noting that for Tinbergen there was a ‘true’ theory underlying the phenomena that ought to be discovered, and that there were two ways of discovering it: (1) by the advent of new facts, and (2) by the advancement of further theoretical investigations.

Tinbergen’s practice was not as schizophrenic as his declaration would suggest. His own way of conducting his research and of building his models led him to be both a good statistician capable of rigorously analyzing the data, as well as a well-informed economist who would hand over theories grounded not only on a priori thinking but also on empirical evidence. “Tinbergen’s approach to economics has always been a practical one [...] [enabling] him to make important contributions to conceptual and theoretical issues [...] always in the context of a relevant economic problem (Morgan and Magnus 1987, 117).<sup>161</sup>

(2) The second point questioned by Keynes was Tinbergen’s assumption that all the variables he included in the model were measurable, and that he could find a way of “supplementing” the model with information about the unmeasurable variables:

The enquiry is, by nature, restricted to the examination of measurable phenomena. Non-measurable phenomena may, of course, at times exercise an important influence on the course of events; and the results of the present analysis must be supplemented by such information about the extent of that influence as can be obtained from other sources (Tinbergen 1939, 11).

Keynes was very skeptical about Tinbergen’s way of providing information on these non-measurable factors and claimed that Tinbergen would “withdraw from the operation of the

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<sup>161</sup> I will come back to this point in the last section of the chapter, where I will show that the political objective of economic planning, by means of economics, would have contributed in reconciling the way the three authors thought of economics.

method all those economic problems where political, social and psychological factors, including such things as government policy, the progress of invention and the state of expectation, may be significant” (Keynes 1939, 384).

(3) The third question Keynes asked had to do with the independence of the economic “factors” or variables. This point has already been evoked in the first section of the chapter, and I will only mention it briefly. According to Keynes, this point was important “[for], if we are using factors which are not wholly independent, we lay ourselves open to the extraordinarily difficult and deceptive complications of ‘spurious’ correlation” (Keynes 1939, 561).<sup>162</sup>

(4) The fourth point raised by Keynes was about the functional form of the model, i.e. about the specification problem, and in particular about the assumption of linearity, which might rule out cyclical factors. Tinbergen had announced that “curvilinear relations [were] considered in [his] studies only in so far as strong evidence exists” (Tinbergen 1939, 11) that the relations between variables present other forms different than linear. In the end, however, Tinbergen treated all the relations between variables as if they were linear relations, since for him linear relations were, in general, good enough approximations of curvilinear relations.

Marschak and Lange (1940) recognized how careful the econometrician should be in “choosing the type of regression function to be fitted” due to “the hidden economic implications of the linearity of Tinbergen’s equations” (Marschak and Lange 1940, quoted in Hendry and Morgan 1995, 396). Yet they defended Tinbergen in this respect and claimed that:

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<sup>162</sup> “This expression [‘spurious correlation’] refers to the danger of drawing hasty conclusions about cause and effect from observed connections between two or more economic variables” (Haavelmo 1989 [1997], 13). Spurious correlations bring up problems of simultaneity and multicollinearity, and it might also appear in a form difficult to be discovered: “If we have what we think is a good and reasonably well-founded theory of some interrelation within a group of economic variables, and the observed facts do not seem to contradict such a theory, we may still be misled, because the same apparent interrelation may often be produced by many different models of economic structures” (13).

Professor Tinbergen’s data [...] do seem to warrant the use of linear functions as a first approximation. Keeping in mind that the linear relationship is a first approximation of any analytic function and taking into account that because of errors of sampling the significant part of a regression curve is restricted to a small section in the neighborhood of the centre of gravity in the scattered diagram, the use of linear functions as approximations appears quite justified (Marschak and Lange 1940 [1995], 396).

(5) The fifth problem brought up by Keynes consisted on the problem of dynamic specification, time-lags and trends:

To the best of my understanding, Prof. Tinbergen is not presented with his time-lags, as he is with his qualitative analysis, by his economists friends, but invents them for himself. This he seems to do by some sort of trial-and-error-method. That is to say, he fidgets about until he finds a time-lag which does not fit in too badly with the theory he is testing and with the general presuppositions of his method. No example is given of the process of determining time-lags which appear, when they come, to be ready-made [...] The introduction of a trend factor is even more tricky and even less discussed. This reference is not obtained by reference to secular changes in the scale of the economy as a whole, but is strictly related to the factors under discussion [...] This seems rather arbitrary! (Keynes 1939, 565).

The way Tinbergen treated time trends “is particularly unsatisfactory and is based on the implicit assumption that all economic variables are subject to a nine-year cycle” and that is why he “bases his regressions on deviations from nine-year moving average trends” (Pesaran and Smith 1985a, 140).

### 3.4.2. Tinbergen’s position

Tinbergen did not naively think that econometrics would just provide a good way of verifying economic theories. With hindsight, he put the matter in the following terms:

I [Tinbergen] think that all of us would agree that you do not prove anything by very favourable values of  $R^2$  and of the t-values. You only say that you give some sort of green light to the man who has formulated the variables used in the regression, and so the proof can only, if there is such a thing, be given by economic reasoning [...] It cannot be done by statistical thinking and I think we would all agree that this is so. *But it does constitute progress if you can say certain things are not correct* (Tinbergen 1987, 128, my emphasis).

Keynes, and any other economist, contemporary or modern, did (and would) certainly

agree with Tinbergen's claim. Yet, Keynes would disagree with Tinbergen in his idea that "a statistical test could prove theory to be *incorrect*, 'by showing that it does not cover a particular set of facts'" (Carabelli 1988, 192, italics in the original).

As Morgan (1990) puts it, "Tinbergen defended the wider inductive claims that [...] econometrics was concerned both with discovery and criticism [and he] believed [...] that if the theory was not confirmed by the results, then the inference was that the theory was wrong or insufficient" (Morgan 1990, 125). In this view, Tinbergen defended the idea that "statistical work might result in the introduction of new variables, or forms of variables, not previously considered as important by theory" (*ibid.*), precisely what Keynes did not think econometrics was capable of doing (see the first section).<sup>163</sup>

And so, for Tinbergen, "[theory] might also be criticized by statistical evidence, if [...] little or no influence was found in the regression relationship for a variable considered important in the theory" (Morgan 1990, 125). Klein (1950) seems to hold a rather close position to Tinbergen's in this respect:

Of what use is the statistical treatment of this simple model [Klein's (1950) famous Model I]? It is agreed that this model is very simplified, very aggregative, but, at the same time, it is more than a mere demonstration of various statistical methodologies. The calculations made for this model serve as a test of certain economic hypotheses. If the data were to refute this model, we should have grounds for questioning the validity of the Marxian theory of effective demand. Since the data do not refute the model, we cannot conclude that this theory stands as proved, but we can have more faith in it, or in any other theory that would produce this model, than would be the case if we made no tests at all (Klein 1950, 64).

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<sup>163</sup> Klein, for instance, provides a good example of the way econometrics could help in taking into account new variables that theory had hitherto not considered important. Klein provides this example in his response to the Lucas's Critique, where he explains that until the 1970s "[many] people failed to realize how important energy or oil, in particular, was for the economy because it represented only a tiny share of total GNP" (Klein 1985, 290). This neglecting of the energy (and food) sector constituted the source of the underestimation of the inflation rate in the forecasts of the Wharton model. Once the macroeconomic models introduced the energy and the agricultural sectors, by the mid-1970s, econometricians "were able to overcome a lack of information from [these] area[s] of economic activity" (Klein 1985, 292). This new available information allowed econometricians to build "an amplified model that was able to handle the inflation problem more realistically by mid-1975, when inflation was still strong", which yielded a moderate forecast error (*ibid.*). For a more detailed account on Klein's (and other econometricians) reaction to the Lucas Critique see Goutsmedt *et al.* (2015).

The latter quotation is quite meaningful about Klein’s vision of the role of econometrics in testing economic theories. At a first superficial glance, Klein’s position would seem to be quite close to Tinbergen’s. Data cannot prove theory to be right, but it can prove it to be wrong, and hence Klein might use this criterion to discard theories. This would be the idea behind Klein’s quotation if taken out of context. If, however, one pays closer attention not only to the terms employed by Klein, but also to the passage of the book where this quotation appears, Klein’s position (as well as Tinbergen’s) might appear more sophisticated and slightly deeper. Klein is talking here about the possibility (and even desirability) of multiple hypotheses. He is asserting that if the data confirms the model, any theory that had produced this model is susceptible of being right. This shows Klein’s (and Tinbergen’s) willingness for the promotion of pluralism.

In the same way, we can test the hypothesis that investment depends upon profits as opposed to other theories of the investment schedule. We shall find that there are several theories of the investment schedule that are not refuted by the data, and this is information worth having. Other cases will be demonstrated, however, in which certain popular hypotheses can be rejected in the sense that they are not consistent with the data (Klein 1950, 64).

### 3.4.3. Three Conceptions of Models

There are many important aspects of Keynes’s understanding of models. The first has to do with the fact that the theories or the hypotheses could be judged to be correct or incorrect if the economist explicitly accepts the conditions that a method like econometric modeling would impose on them. In Carabelli’s words, “only those theories which were homogenous in character with the technique applied could be shown to be incorrect” (Carabelli 1988, 295). “At best only those theories can be shown to be incorrect, which in the view of the economist who advances them, accept as applicable the various conditions of the method proposed by Tinbergen” (Keynes CW XIV, 307).

This argument would go well in line with Haavelmo’s (1944) idea of formulating



economic theories in a statistical way, in order to apply probability theory to test the economic hypotheses, as if they were statistical hypotheses. In this sense, the economist would be accepting that theories and techniques would be homogenous, and so the latter could judge the former to be correct or incorrect.

If we [the economists] want to apply statistical inference to testing the hypotheses of economic theory, it *implies* such a formulation of economic theories that they represent *statistical* hypotheses, i.e. statements – perhaps very broad ones – regarding certain probability distributions. The belief that we can make use of statistical inference without this link can only be based upon lack of precision in formulating the problems (Haavelmo 1944, iv, emphasis in the original).

Another important aspect of the way Keynes would understand models has to do with the usefulness of an economic model as an instrument of thought. A model would not be a representation of reality, but rather a way to inquire and to act on that reality. Furthermore, according to Keynes, it would be dangerous to think that an instrument like an econometric model might be turned into something rigid and general, since, in this case, the usefulness of the model as an instrument would be lost.

In [the] natural sciences the object of experiment is to fill in the actual values of the various quantities and factors in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. Tinbergen endavours to work out the variable quantities in a particular case, or perhaps in the average of several particular cases, and he then suggests that the quantitative formula so obtained has general validity. Yet, in fact, by filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it (Keynes CW XIV, 300).<sup>164</sup>

In order to understand Keynes's fear about the model as an instrument losing its usefulness, it might be helpful to recall Keynes's view on the way people acquire knowledge under uncertain conditions by leaning upon the already institutionalized practices: "[...]

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<sup>164</sup> For Keynes, "*Progress* in economics consists almost entirely in a progressive improvement in the choice of models [for which] one does *not* fit in real values for the variable functions...For as soon as this is done, the model loses its generality and its value as a mode of thought" (Keynes CW XIV, 296, quoted in Moggridge 1993, 623).

lacking information about future outcomes of all current choices, people ‘get by,’ by making use of their significant knowledge of existing practices and their general situation; they make use of knowledge obtained through their practical involvement in the society in which they live. (Indeed by so acting on the basis of such knowledge people help to constitute those practices.) This is an integral part of the [...] account of behaviour under uncertainty” (Lawson 1985b, 924).

When applied to the scientific world, Keynes’s conception of the way humans get access to knowledge through the institutionalized practices, helps understanding Keynes’s fears about the inattentive use of models might lead to put some important limits to economics. “Similarly ‘scientific’ knowledge is determined by particular people, whose ways of acting and thinking are dependent upon the society in which they live, and who thus obtain knowledge through participating in that society. Social theory itself then is dependent upon social practice” (Lawson 1985b, 924). Given the way economists like Tinbergen were using the econometric models as ‘representations the world’ and not as mere tools of investigation, Keynes saw a danger in that this new practice would limit the way economists would gain knowledge about the functioning of economic phenomena.

“The pointing out of these limitations can be interpreted as an application to econometrics of *A Treatise of Probability*’s discussion of the interplay between the specific characteristics of the theoretical tool and those of the material under investigation [...] In *A Treatise of Probability*, as regards mathematical probability and mathematical statistics, Keynes stressed the limitative conditions of their application to the logical danger which could be caused by blind application of them” (Carabelli 1988, 295).<sup>165</sup>

This blind application of instruments goes well in line with Ian Hacking’s (1992; 2002)

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<sup>165</sup> This position could also be understood by the criticism that Rutledge Vining (1949) made of the methodology of the Cowles Commission in his response to T. C. Koopmans’s attack of the NBER methods of investigation, claiming that this methodology would only put a *straight jacket* to economic theory.

argument about the way scientists might use statistical methods in an unreasoned way, as a *style of scientific reasoning*, once these practices have been stabilized in a scientific milieu. As Hacking states, practitioners can sometimes, adopt a style without really understanding the fundamental ideas behind its methods.

This is at its most obvious in ‘cookbooks’ for statistical reasoning prepared for this or that branch of science, psychology, cladistics taxonomy, high energy physics, and so forth. With no understanding of principles, and perhaps using only a mindless statistical package for the computer, an investigator is able to use statistics without understanding its language in any meaningful way whatsoever (Hacking 2002, 184).

Economics, even if not explicitly mentioned by Hacking, does not escape this limitation either. Keynes, “in his discussion with Tinbergen, [...] stressed the partiality of Tinbergen’s theoretical findings, on the basis that the analytical tool adopted automatically reflected its characteristics upon the material examined” (Carabelli 1988, 295-6). This was an argument that had been already anticipated by Alfred Marshall:

He [the mathematician] takes no technical responsibility for the material, and is often unaware how inadequate the material is to bear the strains of his powerful machinery (Marshall 1890 [1895], 644).

Koopmans, some years afterwards, would put the matter in the following way:

In principle, tools have a servant’s status [...] If we look with a historian’s interest at the development of a science, however, we find that tools also have a life of their own [...] Our servants may thus become our guides, for better or worse, depending on the accident of case (Koopmans 1957, 170).

The tool of statistical inference becomes available as the result of a self-imposed limitation of the universe of discourse [...] It should be kept in mind that the sharpness and power of these remarkable tools of inductive reasoning are brought by willingness to adopt a specification of the universe in a form suitable for mathematical analysis (Koopmans 1957, 197-198).

Keynes (1936) clearly stated how the framework he had provided to understand the economy should be understood, and criticized the unreasoned use of mathematics for its lack in precision. His argument is worth quoting at length:

The object of our analysis is, not to provide a machine, or method of blind

manipulation, which will furnish an infallible answer, but to provide ourselves with an organised and orderly method of thinking out particular problems; and, after we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking. Any other way of applying our formal principles of thought (without which, however, we shall be lost in the woods) will lead us into error. It is a great fault of symbolic pseudo-mathematical methods of formalising a system of economic analysis [...] that they expressly assume strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed; whereas, in ordinary discourse, where we are not blindly manipulating but know all the time what we are doing and what the words mean, we can keep ‘at the back of our heads’ the necessary reserves and qualifications and the adjustments which we shall have to make later on, in a way in which we cannot keep complicated partial differentials ‘at the back’ of several pages of algebra which assume that they all vanish. Too large a proportion of recent ‘mathematical’ economics are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols (Keynes 1936 [1964], 297-298).

For Tinbergen, models would help in the search for practical solutions, caring not too much about realism, since for him “a model is but a stylized version of the economic system” (Morgan 1990, 103). Tinbergen saw the economy as a system of causal relations, which could be expressed in a model. He explained that:

We may start from the proposition that every change in economic life has a number of proximate causes. These proximate causes themselves have their own proximate causes which in turn are indirect ‘deeper’ causes with respect to the first mentioned change, and so on. Thus a network of causal relationships can be laid out connecting up all the successive changes occurring in an economic community. Apart from causal relationships there will also exist relationships of definition [...] And, finally, there will be technical or institutional connections. All these relationships together form a system of equations [...] each of [which] can be looked upon as a determining equation for one of the elements, explaining what factors influence that element and how large is the effect of a given change in each factor (Tinbergen 1937, 8, quoted in Morgan 1990, 103).

He thought that mathematics was a powerful tool, but only applicable if the model was simple enough. Yet, simplicity had a cost in terms of losing realism, and this enhanced a degree of arbitrariness (which Keynes would criticize and which Tinbergen would have no problem in recognizing). This part, Tinbergen would also call “the ‘art’ of economic

research”:

I [Tinbergen] must stress the necessity for simplification. Mathematical treatment is a powerful tool; it is, however, only applicable if the number of elements [if] the system is not too large [...] the whole community has to be schematised to a ‘model’ before anything fruitful can be done. This process of schematisation is, of course, more or less arbitrary. It could, of course, be done in a way other than has here been attempted. In a sense this is the ‘art’ of economic research (Tinbergen 1937, 8, quoted in Morgan 1990, 103).

Tinbergen built his models by means of “an iterative process involving both hypotheses and statistical estimation” (Morgan 1990, 103):

The description of the simplified model [...] commences with an enumeration of the variables introduced. The equations assumed to exist between these variables will be considered in the second place. This order does not correspond exactly to the procedure followed in the construction of the model. One cannot know *a priori* what variables are necessary for and what can be neglected in the explanation of the central phenomena that are under consideration. It is only during the actual work, and especially after the statistical verification of the hypotheses, that this can be discovered. As a matter of fact, the two stages mentioned are really taken up at the same time; it is for the sake of clearness that the exposition is given in two stages. A glance at the ‘kitchen’ will nevertheless be occasionally made in order to avoid an impression of magic (Tinbergen 1937, 8, quoted in Morgan 1990, 103).

Finally, Tinbergen was less optimistic than Frisch about the importance that econometrics should acquire in the economic discipline. While Frisch had declared in the first issue of *Econometrica* that econometrics would take over economics and be the forward way, Tinbergen was more prudent and say that Frisch “[...] goes a bit too far. There are also some purely qualitative elements and by definition these cannot be a subject for econometrics” (Tinbergen 1987, 136).

Klein’s conception of models was more ambitious than Tinbergen’s regarding realism. Even if it was clear for Klein that a model could not fully represent the real relations between the variables explaining the phenomena, he was optimistic about the fact that with painstaking work, with a lot of updating, correcting, estimating, re-estimating, thinking and rethinking of the model, one would little by little converge to a model which would resemble

reality more and more. Klein very rapidly understood that in order to be able to account for reality in a model, the statistical methods should remain quite flexible and should not act as “straight jackets” constraining the economic reasoning behind the modeling activity.

It is important to grasp the simultaneity of the macroeconomy but not necessarily to tie statistical estimation methods exclusively to this property. It is more important to be able to update, correct or revise estimates on the basis of a steady flow of important new information, and very flexible methods of estimation are needed for this purpose. The highly flexible methods can be more powerful in simple form than the more complicated procedures [like the maximum likelihood methods]. In particular, for an economy where detailed information is important, it is preferable to aim for large systems [...] and to handle them by relatively flexible, simple statistical methods instead of paying enormous attention to complicated estimation procedures for smaller manageable systems (Klein 1991, 115-116).

Keynes, as stated in his letter to Harrod, would have welcomed this kind of empirical approach, even if he would not make the work by himself:

Do not be reluctant to soil your hands [...] I think it is most important. The specialist in the manufacture of models will not be successful unless he is constantly correcting his judgment by intimate and messy acquaintance with the facts to which his models has to be applied (Keynes 1938, Letter to Harrod, 16 July 1938, 299).

For the purpose of gaining in reality resemblance, Klein made a lot of emphasis in the institutional side of the economy, as well as on the refinement in the collection of economic data:

The building of institutional reality into a priori formulations of economic reality has and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference (Klein 1960, 867).

In a way, the three authors agree on this conception of models as instruments the construction and use of which reveal the underlying mechanisms of the economy. Keynes, of course, is more careful and less optimistic in his opinion of the usefulness that models might provide if one sticks to them, as I have showed. Yet, one could conclude that Morgan and Morrison’s (1999) plea about the fact that models have to be built and used for the researchers to learn from them, goes in the same direction as that of Keynes, Tinbergen and

Klein:

We do not learn much from looking at a model – we learn from building the model and manipulating it. Just as one needs to use or observe the use of a hammer in order to really understand its function; similarly, models have to be used before they will give up their secrets. In this sense, they have the quality of a technology – the power of the model only becomes apparent in the context of its use (Morgan and Morrison 1999, 12).

#### 3.4.4. Econometrics and replication: the story of the seventy translators of the Septuagint

Replication of econometric results is important as a way of increasing the degree of confidence in these results. And, of course, “if very similar estimates are obtained with different sets of data, different *ceteris paribus* assumptions, or different stochastic specifications, then the confidence with which [these estimates] are used will be increased” (Pesaran and Smith 1985a, 139).

Klein was of the opinion that econometric modeling would provide the economists with a tool by means of which they could arrive at the same conclusions, disregarding their particular theoretical or personal whims. This means that econometrics would be a tool capable of replicating results out of the same data set. For him, of course, this was a “desirable” situation, which would “put forward the scientific spirit” of econometrics at the service of economic policy:

It is desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgments of a particular investigator. Econometric models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims (Klein 1947c, 111).

Keynes, however, doubted that econometricians would do any good in this respect, making fun of their capacity of replicating each other’s results. If it were the case that econometricians would come up with the same results, it could only be the outcome of some kind of miracle:

It will be remembered that the seventy translators of the Septuagint were shut up in separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy multiple correlators were shut up with the same statistical material? And anyhow, I suppose, if each had a different economist perched on his *a priori* that would make a difference to the outcome (Keynes 1940, 155-6).<sup>166</sup>

Keynes’s argument goes beyond the econometricians’ inability of replication and is actually about the impossibility of the econometrician to be impartial.<sup>167</sup> Nothing prevents the econometrician from manipulating the results, or the data:

With a free hand to choose coefficients and time lags, one can, with enough industry, always cook a formula to fit moderately well a limited range of past facts. But what does this prove? Are not further and different tests required before it is properly available for inductive argument? (Letter to Tyler, 23 August 1938, CW XIV, 287).

Koopmans (1941), in his defense of Tinbergen, also recognized the possibility that econometricians manipulated the data:

Actually, the econometrician not infrequently finds himself ‘playing with the data’, trying out the consequences of some alternative assumptions (Koopmans 1941, 164).

#### 3.4. What about Reconciliation?

Hitherto, the chapter has dealt with the differences between Keynes and Tinbergen. This last section of the chapter studies to what extent Klein’s macroeconomic practice provided a way for reconciling these authors. But why would it be important for Tinbergen and Keynes to reach any kind of reconciliation? Reconciliation in these terms does not mean that, in the end, the most fundamental problems would be solved. As I have already stated, there were some problems where reaching agreement was not possible. Reconciliation in the sense of this chapter means finding a third way, alternative to that of Keynes or to that of Tinbergen, which would make the most of both worlds, always remaining critical of them. Such a way of

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<sup>166</sup> In the same vein of Keynes’s argument, Pesaran and Smith (1985a) argue that “[the] experience of the past 40 years suggest that few econometric results are robust, and that replication is achieved only when the various econometricians are looking for the same result.” And they are not the only ones arriving at these conclusions. Leamer (1983; 2010) and Magnus and Morgan (1997) would maintain a similar view on that topic.

<sup>167</sup> David Teira (2013) makes the case of impartiality in the case of British clinical trials.



reconciliation allows for a better understanding of the limits of both econometrics and economic theory. One of the limits would be to understand that not everything in econometrics can be approached from a “classical” view of probability, but that there are phenomena that, following Keynes, should be approached from the side of uncertainty. There are many things in the economy that are simply unpredictable.

Klein’s way of reconciling Keynes and Tinbergen might have never been explicitly stated, but it is reflected in his way of building his macroeconomic models. Klein would adhere to Keynes in his way of understanding the economic world as very complex, “approach[ing] the explanation of economic events in terms of a social world made up of institutions, roles, responsibilities, powers and so on [and] consider[ing] the socio-economic system to be made up of ‘structured objects’ whose powers exist independently of our knowledge or perception” (Nell and Errouaki 2013). Klein would, on the other hand, adhere to Tinbergen in his way of specifying his models and of testing them, confronting the hypotheses with data, and retaining those, which seemed to fit the observations. In a word, combining the theoretical with the applied work in order to come to sound conclusions about economic hypotheses that would allow him to build up his econometric models.

I will argue that there were at least five points that made it possible for Klein to make Keynes and Tinbergen come closer. (1) The first point has to do with Klein’s methodological structuralism. Klein thought of the economy as a system made up by structural relations, which would determine the way economic variables move. This way of looking at the world “forced” Klein to think first in terms of economic theory before observing “the world.” However, the way Klein would observe the world would not be just “theory-laden,” but this theory-ladenness would be of a systematic and rational nature. (2) The second important point, very much related with the first one, has to do with the introduction of a priori information and knowledge into the model. (3) The third aspect has to do with Klein’s

restrictive way of looking at the validity of an econometric model in terms of time and space. (4) The fourth element has to do with the political objectives that the three authors followed: Economic Planning. Last but not least, (5) Klein thought of the econometric exercise not as a “once-and-for-all job,” but as a painstaking activity that should be continued every time new information became available.

### 3.4.1. Klein’s Methodological Structuralism

Klein’s methodological approach brings him closer to Keynes’s vision about modeling. In the same spirit of Haavelmo (1944), Klein thought that the world, which was composed of structural relations, could be investigated by means of models. His methodology might provide a middle ground position between Keynes and Tinbergen, because of the heavy institutional component present in his approach. In fact, Klein might not stand alone in his position. A very influential character in Klein’s view of how the econometric research should be undertaken is Haavelmo (Klein 1987; 1991):

Nell and Errouaki (2013) describe Klein’s Methodological Structuralism as an “ontological turn [...] that ensures that socioeconomic reality, understood through fieldwork [or rather survey data in Klein’s particular case], will be what defines the terms of the model, and not the other way around” (Nell and Errouaki 2013, xxiii). The idea is that models should give account of “what actually exists,” exhibiting, at the same time, relationships similar to those found in “reality” in a way susceptible of being manipulated or analyzed mathematically (xxiii):

[Klein] approach[es] the explanation of economic events in terms of a social world made up of institutions, roles, responsibilities, powers and so on [and] considers the socio-economic system to be made up of ‘structured objects’ whose powers exist independently of our knowledge or perception [...] The policeman *has the power* to arrest us, and the President *has the power* to call up the National Guard, whether we know it or not. These objects, relationships, powers and duties constitute the basis of the causal relationships that economic science describes. Employers *can* hire and fire workers and *can* order them around; firms *can* move capital from place to place

opening and closing plants (Nell and Errouaki 2013, 430, my emphasis)<sup>168</sup>

“An implication of [...] Klein’s methodological structuralism is that the social domain appears to be open, so it must be described by theories that reflect and acknowledge this openness” (Nell and Errouaki 2013, 430). Two examples of these theories reflecting and acknowledging this openness would be, for instance, Keynesian and Marxian theory. “‘Openness’ means that some of the key probability distributions could shift unexpectedly, for reasons that cannot be foreseen (430).

#### 3.4.2. *Taking a priori knowledge into account: another step closer to Keynes’s approach*

In order to improve his model, the researcher has the possibility (or the obligation) of introducing more accurate a priori information into the mathematical model, which reflects, for example, the relation of power between the employers and the employees in the labor market. A priori information is a kind of knowledge about the economy as a *whole* and “is [therefore] independent of the particular sample being used [and] may consist of economic theory, a knowledge of economic institutions, a knowledge of technology, or empirical results from independent samples” (Klein 1957, 2).

A priori information stemming from “knowledge of technology” means that some improvement could be attained by the development and refinement of more sophisticated methods of statistical inference. The improvement of this kind of knowledge would be much closer to the line of research of other researchers of the Cowles Commission, notably Marschak and Koopmans (Pinzón-Fuchs 2014). Klein did not think that these technical improvements would be decisive for the advancement of econometrics. Rather, he thought that it was the improvement of institutional reality and the refinement of data, which would decisively contribute to the improvement of econometric modeling:

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<sup>168</sup> Klein’s approach would not only be compatible with the Keynesian account but also with a Marxian account, since he describes society and the economy as functioning through relationships of power between different groups.

The building of institutional reality into a priori formulations of economic reality and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference (Klein 1960, 867).

This argument, would, of course, go quite well in line with Keynes’s thinking:

The more complicated and technical the preliminary statistical investigations become, the more prone inquirers are to mistake the statistical description for an inductive generalization (Keynes 1921, 373).

For Klein, then, as for Keynes, a priori information or knowledge would play a more important role in the understanding of the economy and in the building of models, compared to the role that statistical and mathematical methods would play.

### 3.4.3. Validity of Models restricted to time and space

A third point towards which all the three authors seem to converge is their belief that economic models (or even theories) were restricted to time and space. “Keynes believed that any parameters that might be measured in a particular study at a particular time would not apply to the economy in the future” (Keynes in Moggridge 1973, 296-9).

Klein, on the other hand, thought that:

A workable model must be *dynamic* and *institutional*; it must reflect processes through time, and it must take into account the main institutional factors affecting the working of any particular system. Different features must be built into adequate models of such diverse economies as the United States, Canada, the United Kingdom, Japan, the Netherlands, etc. Models of non- capitalistic societies will differ even more radically from the models of capitalist countries, with investment not an endogenous magnitude (Klein 1954, 279).

Koopmans (1941) emphasized the limited validity of Tinbergen’s (1939) work, as a means to defend it. Even if Tinbergen (1939) had made a point on this and recognized the limited character in time and space of his study, Koopmans insisted on that point and went on saying that:

The problem to be dealt with [...] may be narrowed down to that of finding a quantitative explanation of cyclical movements occurring in a given country during a given period in which no important, or only readily recognized, changes in

economic structure took place. Further, in so far as testing of business-cycle theories appears possible, it means testing the relevance of such theories with respect to country and period considered (Koopmans 1941, 158).

#### 3.4.4. Economic planning as the primary policy objective of economics

An aspect that would appear almost as a “natural” point of reconciliation between the three authors would be the political objectives they had. At a first glance, it would seem quite clear that all three authors were in favor of state intervention. As mentioned in the introduction, Keynes’s theoretical framework had set the bases for legitimizing economic intervention from the part of the state. Both Tinbergen and Klein’s works had been put in place with that objective (Tinbergen 1987; Klein 1991).

In terms of the kind of interventions that were promoted by the authors, however, Klein and Tinbergen seem to be a step closer when compared to Keynes. Intervention alone would not be satisfactory enough for Klein, since he thought that the social and economic problems were so profound that they had to be resolved at its roots. In *The Keynesian Revolution* Klein (1947a) had described, in general terms, “a practical program of economic policy [...] necessary in order to *reform capitalism* to a system of full employment” (Klein 1947a, 168, my emphasis). This program had a Marxian (and not a Keynesian) flavor, since it was Marx’s ultimate aim (and not Keynes’s), which would satisfy Klein’s image about intervention.<sup>169</sup>

For Klein, Marx’s aim was to “analyze the reasons why the capitalist system could not function properly, while Keynes analyzed the reasons why the capitalist system did not but could function properly. Keynes wanted to apologize and preserve, while Marx wanted to criticize and destroy” (131). For Klein the positions of Marx and Keynes were opposed; the former was a revolutionary and the latter just a reformer. Although Klein favored income distribution policies, he demonstrated, following Marxian arguments, that this policy would not be sufficient “to insure that capitalism will always provide uninterrupted full production

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<sup>169</sup> Remember that Klein had always been concerned with achieving social reform (see chapters 2 and 4), and also that Tinbergen had studied physics in the first place, but had abandoned it because he thought that economics was a more socially useful science (Tinbergen 1987; Morgan 1990).

and employment” (Klein 1947a, 131):

Full-employment planning (functional finance or compensatory fiscal policy) is not enough (Klein 1948, “The case of Planning,” quoted by Mirowski, 2012, 149).

Complete planning leads generally to a higher level of welfare than perfect competition even in the case where wealth redistribution is permitted in the latter system (149).

Tinbergen (1937) considers that the objective of econometrics is “its particular usefulness in decision making and policy formation, its ability to organize and structure thought, clarify the issues under dispute, use the available information efficiently, and provide a framework for action” (Pesaran and Smith 1985, 147).<sup>170</sup> Keynes would agree with Tinbergen in these objectives. When Keynes dealt with policy problems he also developed similar underlying procedures as economic modelers did, even if, in the end, Keynes’s resulting models would not be presented in a formal mathematical fashion (147).<sup>171</sup> This procedure would consist on the assumption of a certain amount of homogeneity of the economic relations, on the one hand, and on the fact of always keeping in mind the objective of predicting the consequences of policy actions as its primary goal, on the other.

Econometric models (or any other kind of economic model) and statistical exercises, however, would not provide a clear-cut argument on which to base economic policy decisions. For Keynes, the construction and use of this kind of models might provide a way for learning about the economy,<sup>172</sup> but, the results yielded by these models should not be considered sufficient as a solid support for making what one would call today “evidence-based policy decisions.”<sup>173</sup>

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<sup>170</sup> This objective of econometrics contrasts clearly with that expressed in Tinbergen (1939), where his emphasis was on the testing of economic theories.

<sup>171</sup> Some examples of Keynes’s works on policy are: “The Economic Consequences of Mr. Churchill” (1925), “Can Lloyd George do it?” (1929) and “How to Pay for the War” (1940).

<sup>172</sup> Morgan and Morrison (1999) provide an explanation of how scientists can learn from the construction and use of models in different research fields and, of course, in economics in particular.

<sup>173</sup> For a criticism of the use of experimentation techniques to provide evidence for policy recommendations in development economics see Favereau (2014). See also Leamer (2010).

### 3.4.5. Not a “once-and-for-all job”

Klein promoted the idea that macroeconometric modeling was not a “once-and-for-all-job” (Klein 1950; 1955), but rather a practice consisting on the rethinking, re-discussing, re-specification and re-estimation of the models, and on the inclusion of new relevant institutional information and data:

Quantitative economics is inelegant, very tedious, very repetitive, and capable of forward movement in small increment. I admired the elegant theorems that my associates produced, but [these] seemed to me [...] very strong and not very realistic. I felt that if one paid unusual attention to data – very much in the painstaking tradition of Simon Kuznets – replicated analyses regularly, looked at more detail for the economy, learned as much as possible about realistic economic reaction, and stayed in touch with the economic situation on a daily basis that it would be possible to use econometric models for guidance, both in the fields of policy application and in pure understanding of the economy (Klein 1991a, 115).

As already stated, Keynes would see the whole process of developing economic theories or models as a task that would never end. The changes in history and in the economy would oblige the economist to revise his theories and models permanently.

### 3.6. Concluding Remarks

In this chapter I have tried to take a fresher look at the controversy between Keynes and Tinbergen on the role of econometrics in testing economic theory. Some commentators of the controversy (see, for instance, Patinkin 1975; Morgan 1990) have dismissed Keynes’s arguments because of their lack of technical accuracy. Even if it is true that some of Keynes’s claims were not entirely well informed or were more or less surpassed in the following years after the controversy, this “defensive” attitude towards the critique might do more harm than good to econometrics. Together with Lawson (1985a; 1985b) and Carabelli (1988) I argued that Keynes’s critique of econometrics is relevant and worth taking into account. In order to understand Keynes’s position, however, one has to go back to his 1921 book *A Treatise on Probability*, and recall his position on probability, statistical inference, and science.

The argument of this chapter was to suggest that after the controversy between Keynes

and Tinbergen, Lawrence R. Klein developed an alternative approach to econometrics capable of partially reconciling Tinbergen’s pioneering, courageous, and optimistic effort with the more critical, skeptical, and pessimistic account of econometrics provided by Keynes. As an expert on Keynesian thought in the United States and as the new prodigy at the Cowles Commission who had been recruited to remake Tinbergen’s macroeconomic model of the US, Klein found himself in a singular position in 1944, only five years after the controversy. Even if Klein never referred explicitly to the controversy, his particular position, his works on economic theory, and his practice of econometrics obliged him to take a stand, even if unconsciously, on most of the matters discussed at the time, not only by Keynes and Tinbergen, but also by the nascent and active community of econometricians.

The importance of providing such a reconciliation in a time when the role of the economist in society and his own image were changing was crucial. On the one hand, this reconciliation allowed for the integration of the new “scientific” method of econometrics with the interventionist policies that the Keynesian system (the New Deal, the 1929 Crash, and the Wars) had provided. The econometricians, armed with these powerful methods, were able to talk to the politicians and to give economic policy advice, based on tools which did not appear to be “alchemy” anymore, but “science.” On the other hand, the literary discourse of economists like Keynes was abandoned, and legitimacy of economic models and theories swung from rhetorical and analytical sophistication to sophisticated mathematical and statistical methods.

But Klein did not provide reconciliation in every point. The most fundamental claim raised by Keynes, that using econometrics to test economic theories was not possible, was just irreconcilable. Since economic phenomena were of a changing and evolutionary nature, inference by means of statistical methods was just unthinkable for Keynes, and prediction as a criterion for choosing the best available theories made no sense. Even if irreconcilable, this



point is not destructive, but it points out to some of the limitations of econometrics, and cries out, not for the abandonment of the program (which Keynes actually encouraged), but for a more careful use of econometrics when providing economic policy advice or when evaluating the validity of economic theories.





# Macroeconometric models as pluralistic scientific tool for economic planning

The most refreshing aspect of Norwegian economic planning has been the attitude of the guiding economic theoreticians to disregard all preconceived notions about the supposedly optimal properties of a free-market economy and to look for direct or indirect controls that will lead to an event of higher level of economic welfare. They have done this work entirely in the democratic spirit so that one cannot find the slightest trace of the suppression of any fundamental human right in present-day Norway.

*Lawrence R. Klein, 1947*

### 4.1. Introduction

In 1991, Nobel Prize laureate in economics Lawrence R. Klein described the practice of “quantitative economics [as] inelegant, very tedious, very repetitive, and capable of forward movement in small increment” (Klein 1991, 115).<sup>174</sup> Yet Klein, who built his academic life around the practice and development of econometrics, did not intend to provide a pessimistic account of quantitative economics, but an historical description of the development of his particular *image* of econometrics, demarcating himself from the more rigid view held by his former colleagues at the Cowles Commission in the late 1940s.<sup>175</sup> Indeed, while Klein

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<sup>174</sup> Klein, in fact, had been awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* in 1980 for his application of econometric modeling “to the analysis of economic fluctuations and economic policies.”

<sup>175</sup> I draw on Leo Corry’s (1989; 2008) framework of image and body of knowledge to provide an account of Klein’s way of doing econometrics. Rather than focusing on Klein’s theoretical or technical contributions, I focus on the way Klein saw econometrics, how he conceived the practice of macroeconometric modeling, which elements he thought should be emphasized in order to improve the econometric results, and what should be the ultimate political purpose of econometrics. In short, I ask what was Klein’s image of knowledge of econometrics. For another application of Corry’s framework in the history of economics see Weintraub (2002).

“admired the elegant theorems that [his Cowles’s] associates produced,” these seemed “very strong and not very realistic” from the standpoint of an applied econometrician, and so he advocated for the necessity of another type of econometrics which would provide guidance in both “the fields of policy application and in pure understanding of the economy” (115). In a nutshell, the kind of econometrics that Klein promoted was one in which the econometrician would pay “unusual attention to data, [replicate] analyses regularly, [look] at more detail for the economy, and [stay] in touch with the economic situation on a daily basis” (115).

Klein built his image of econometrics during his early period as an econometrician (1944-1950), and in particular during the time he spent at Cowles (1944-1947).<sup>176</sup> In 1944, the Cowles Commission situated at the University of Chicago was a special institution with Jacob Marschak as its research director (1943-1948). Indeed, by that time, Marschak was gathering a select group of economists, mathematicians, and statisticians to develop an ambitious program in econometrics that would provide a powerful “precision-engineered tool [to] solve a host of econometric problems” (Morgan 1990, 251).<sup>177</sup> Yet, while the whole econometric program had been conceived as a teamwork effort and while all the team members shared a common background in econometrics that promoted Haavelmo’s (1944) probability approach, important differences in the conception of econometrics remained among some of the members of the Cowles’s team. Tjalling C. Koopmans, for instance, defended a special image of econometrics and of science that formed the basis of his critical review (Koopmans 1947) of Burns and Mitchell (1946), and which triggered the “Measurement without Theory” controversy between Cowles and the National Bureau of Economic Research (NBER). In his review, Koopmans (1947) criticized Arthur Burns and

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<sup>176</sup> For different accounts of the Cowles Commission’s period at Chicago see Christ (1952; 1994), Malinvaud (1983), Hildreth (1986), Morgan (1990), Epstein (1987), Qin (1993), Klein (1991), Bjerkholt (2015).

<sup>177</sup> Stalling Koopmans and Leonid Hurwicz were already at Cowles in 1944, and other important figures were still to be hired in the following years: Theodore W. Anderson (1945), Herman Rubin, Roy B. Leipnik and Trygve Haavelmo (1946), and Kenneth Arrow (1947).

Wesley C. Mitchell for their use of quantitative methods for “merely” observing economic phenomena in order to establish empirical regularities without actually providing any substantial explanation of the underlying mechanisms behind the movement of the observed variables. According to Koopmans who thought of the development of sciences as divided in two stages, the NBER found itself in the preliminary “observational” Kepler stage while the Cowles Commission was already in the more advanced “explanatory” Newton stage of development. Indeed, Cowles had already adopted the “most advanced economic theory,” i.e. Walras’s theory of general equilibrium, and so it could not only establish empirical regularities through the use of its econometric methods, but it could also provide an explanation of the underlying behavioral equations behind any economic phenomenon. Hence, it was clear to Koopmans that the Commission should first promote Walrasian theory, and then econometric methods.

Klein, on the contrary, understood the whole controversy in a different way. To him, the center of the controversy was less on the adoption of a particular theoretical framework to explain economic phenomena than on the positions of both Cowles and the NBER regarding intervention and economic planning. While the Cowles Commission promoted direct forms of intervening the economy and of conducting economic planning, the NBER was quite reticent to accept their political stance in an overt way, following its “fifth 1920 precept” that “the Bureau should carefully abstain from making recommendations on policy” (Fabricant 1989, 3). Yet Klein appraised certain aspects of the NBER’s methodology, in particular the willingness to examine all the available theories to build econometric models, as well as the painstaking preparation and continuous update of data and models. These practice-related aspects might have seemed secondary from the point of view of the elegant and abstract theorems that other members at Cowles were working at. To Klein, however, the insights from the NBER proved more than useful for his own modeling practice since he was involved

in the empirical and applied project of building a macroeconometric model of the US economy, and so these were problems he confronted on a daily basis. Indeed, throughout his whole life, Klein's own modeling practice promoted not only a pluralistic approach to economic theories, but also the maintenance of a sufficiently flexible model that would allow for the inclusion and update of new sources of information, and of "institutional reality." In this chapter, I want first to emphasize the existence of different conceptions of econometrics within the Cowles Commission during the 1940s, and then characterize Klein's particular image of econometrics not only as a tedious, repetitive, teamwork-based, and messy scientific practice, but most importantly, as a pluralistic scientific tool for economic planning.

#### 4.2. Discrepancies at Cowles: Klein's Pluralism and the "Cowles Creed"

##### 4.2.1. A new left wing "prodigy" at Cowles: Klein's appointment to "redo" Tinbergen's macroeconometric model

In November 1944, Klein was appointed research associate at Cowles. Marschak had reserved an important place for Klein in his ambitious econometric program "to redo Tinbergen's model of the US economy," recruiting him "explicitly to prepare model specification according to received economic theory, using both microeconomics, macroeconomics, and aggregation or index theory to bridge the gap between them" (Klein 1991, 109).

The Cowles Commission at the University of Chicago that Klein integrated in the mid-1940s was a fascinating place that gathered some of the brightest economists and statisticians of the time. Apart from Marschak, the team included other European *émigrés* like, Koopmans, Leonid Hurwicz, and Oskar Lange, who exerted a major influence in shaping the Commission's approach to economics and in pursuing the idea that economics should become a *wertfrei* social science (Craver and Leijonhufvud, 1987; Hageman 2011). Although Klein was supportive of a *wertfrei* economics himself, his political left wing orientation,

perhaps more radical than that of some other Cowles’s fellows (Mirowski 2012; Bjerkholt 2014b), might have raised doubts about his way of applying econometric methods. In particular, his use of Marxian theories to specify econometric models, and his brief membership to the Communist party affected his career both during his years at Cowles and during the McCarthy era (see Weintraub 2015).

Klein’s particular image of econometrics, however, had to do not only with his “dangerous” political predilections, but with what he thought should be the relation between economic theory and econometrics, and with his practices as an econometrician. In this sense, the particularity of Klein’s image of econometrics was his pluralistic approach to economic theory. Contrarily to the Cowles Commission, Klein presented himself throughout his whole life, especially between 1944 and 1950, as an econometrician willing to confront all the plausible available theories with data. For Klein, being an econometrician was not a matter of being a Keynesian, a Marxian, or a Neoclassical. Rather, abstract theories of all kinds provided fruitful and pedagogic ways of understanding some of the relationships between the variables studied, as well as an idea of how the economy worked. This, however, was true only when the confrontation with data could establish the usefulness of the particular theory (or hypothesis) in the specification of a model. In order to address the question of how this position contrasted with that of the Commission’s, I revisit two events that occurred at Cowles between 1946 and 1947: (1) the “Measurement without Theory” controversy, and (2) Koopmans and Marschak’s rejection of a paper written by Klein in which he clearly defended his pluralistic approach.

#### 4.2.2. Sir Isaac Newton and econometrics: Walras and Cowles on the discovery of general and fundamental laws

Koopmans’s (1947) review of Burns and Mitchell (1946) triggered what today is known as the “Measurement without Theory” controversy. Koopmans started his review by making an



analogy between the stages of development of economic theory and astronomy, i.e. between what he called the Kepler and the Newton stages of development of any science.<sup>178</sup> The Kepler stage consisted of a phase where researchers were still concerned with the discovery of the most fundamental relations between variables in order to understand the observed phenomena. And so, any discipline that found itself in this stage would be in need of purely observational and descriptive work. Alternatively, if a scientific field had reached the Newton stage of development, this meant that a fundamental and general theory explaining the most “elementary and general” relations between the variables studied existed (Koopmans 1947,161).

According to Koopmans (161-162), Burns and Mitchell – and the researchers at the National Bureau of Economic Research (NBER) – would be in the Kepler stage, while the Cowles Commission, which based its studies in the Walrasian framework, would be in the Newton stage of development.

It appears to be the intention of Burns and Mitchell – in any case it is the opinion of the present reviewer – that their book represents an important contribution to the “Kepler stage” of inquiry in the field of economics.<sup>179</sup>

To Koopmans, while the researchers of the NBER were still trying to establish empirical regularities between economic variables, the Cowles’s researchers had already solved this problem, and knew which were the most fundamental relations underlying economic variables and phenomena. The Walrasian general-equilibrium framework had provided them with the answer, and so, Cowles did not have to deal with the problem of multiple hypotheses

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<sup>178</sup> For detailed accounts of the controversy, see Morgan (1990) and Epstein (1999). See also Koopmans (1947) and Vining (1949) for the original controversy.

<sup>179</sup> Notice that Vining (the young NBER standard bearer in the Controversy) did not refute Koopmans’s analogy between the stages of development in economics and astronomy. Even if he did not accept that Walrasian theory could be treated as the equivalent of Newton’s theory in economics, he did neither refute comparing economics to the natural sciences, nor did he refute the notion of progress underlying the Kepler and the Newton stage framework. This position is quite representative of the image of economics of the time, consisting on the consideration that economics should converge to the natural sciences in order to “progress.”

anymore. To them, there was only one type of hypotheses that one could take for granted in the building of econometric modeling, and it stemmed from Walrasian economics.<sup>180</sup>

#### 4.2.3. Klein’s reading of the controversy: Methodological issues and intervention

Although he did not play an active role in this controversy, Klein’s position is worth mentioning for he had to deal with some of the consequences this controversy brought during his short stay at the NBER, in 1948. For instance, according to him (Klein 1991, 112), the controversy was not exclusively methodological, but the political element also played an important role.<sup>181</sup>

As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology. It was not purely methodological, however. A central issue was that we members of Cowles were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by [...] the National Bureau.

Although closer to Cowles than to the Bureau in terms of political orientations, in terms of methodology, Klein had admiration for the work of the NBER. He particularly appreciated that researchers such as Simon Kuznets, Burns, and Mitchell, attributed great importance to the role of data in the analysis of the business cycle.

I felt that if one paid unusual attention to data – very much in the painstaking tradition of Simon Kuznets – [...] that it would be possible to use econometric models for guidance, both in the fields of policy application and in pure understanding of the economy (Klein 1991, 115).

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<sup>180</sup> Duo Qin (1993, 63) provides a quite different story about the problem of multiple hypotheses and of the underlying reasons explaining why Cowles dismissed this problem. According to Qin, “since the central task of the Cowles Commission was to formalize the statistical methods applicable for econometric analyses, given economic theory, they consciously left open the issue of how to put particular economic theory into a particular structural model.”

I think that Qin misses the point here, because for the Cowles there was no doubt that the “given economic theory” could be no other than the Walrasian general-equilibrium framework. They did not leave the issue open, but they closed it before one could even think about it (see the letter that I quote in page 9 from Koopmans to Marschak).

<sup>181</sup> A third aspect worth of analysis in this controversy is the battle for funding between both institutions, treated in more detail in Mirowski (2002; 2012).

Klein felt that the main dispute between the NBER and the Commission had to do with their distinct political positions rather than with their methodological approaches. The NBER had more reticence in promoting governmental intervention to steer the economy, and tried to remain untied both to business forecasting and to studies requested by the government. More left wing oriented but not necessarily Keynesians, on the contrary, the members of the Cowles Commission were clearly in favor of governmental intervention (Miroswki 2012).<sup>182</sup>

But intervention alone, whether inspired by Keynes or Tinbergen, did not satisfy Klein. Indeed, Klein was convinced that the social and economic problems in the US were so profound that they had to be resolved at their roots. In *The Keynesian Revolution* Klein described, in general terms, “a practical program of economic policy [...] necessary in order to reform capitalism to a system of full employment” (Klein 1947b, 168, my emphasis). This program had a Marxian (and not a Keynesian flavor) since it was Marx’s ultimate aim, and not Keynes’s, the one that could satisfy Klein’s image on intervention. To Klein, Marx’s intention was to “analyze the reasons why the capitalist system could not function properly, while Keynes analyzed the reasons why the capitalist system did not but could function properly. Keynes wanted to apologize and preserve, while Marx wanted to criticize and destroy” (131). To Klein, Marx and Keynes were completely opposed in that the first was a revolutionary while the second was a reformist. Although Klein favored income distribution policies, he demonstrated, following Marxian arguments, that this policy was not sufficient “to insure that capitalism will always provide uninterrupted full production and employment” (Klein 1947a, 131):

Full-employment planning (functional finance or compensatory fiscal policy) is not enough [...] Complete planning leads generally to a higher level of welfare than

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<sup>182</sup> This is not to say that there were no important methodological differences between the members of Cowles and those of the NBER. Yet, when it came to macroeconometric model building, these two approaches seem to have less “fundamental” differences and they tend to partake of important methodological principles. For an account on the methodological differences between Klein and Milton Friedman see Pinzón-Fuchs (2016).

perfect competition even in the case where wealth redistribution is permitted in the latter system (Klein 1948, “The case of Planning”, quoted by Mirowski, 2012, 149).

In short, to Klein, it was clear that the different methodological approaches of Cowles and the NBER were not sufficient to understand the Measurement without Theory controversy. Other factors, closely related to their political visions and in particular with their opinion on intervention, had to be taken into account to explain the controversy. Between 1944-48, Klein himself, was in favor of a special kind of intervention, which would not make capitalism better, but actually reform the economic system. His position, however, not only contrasted with that of the NBER, but also with that held by some members of Cowles, especially by Koopmans.

#### 4.2.4. Koopmans’s Reaction to Klein’s Pluralism: “First sell modern economic theory, then econometric methods”

In 1946 Klein wrote a paper in which he attempted to compare the Walrasian, Keynesian, and Marxian theories of effective demand, in order to demonstrate that each one of them could lead to the same conclusion, and be used to build the same macroeconomic model.<sup>183</sup> This procedure, however, did not please Koopmans who advised Marschak not to publish Klein’s paper under the *Cowles Discussion Paper series*. According to Koopmans, Klein had not exclusively used “modern theories of economic behavior,” i.e. Walrasian general equilibrium theory, which was an argument strong enough to reject his paper:

[“Theories of effective Demand and Employment”] is an attempt to sell the idea of econometric model building to adherents of Marxian economic doctrine. I shall explain in these comments why I believe that such attempts, including the present one, are harmful to the objectives of econometric model building. The main reason is that econometric research of the type in which we are engaged in is essentially based on modern theories of economic behavior. *The way to sell it to anybody, including Marxian economists, is first to sell modern economic theory, then to sell econometric methods.* There are no short cuts (Koopmans to Marschak, memo December 10, 1946, my emphasis).<sup>184</sup>

Koopmans’s position clearly showed that the aim at Cowles was to promote the

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<sup>183</sup> In fact, the original title of the paper was “Marxian Theory of Effective Demand,” and in its first version the paper did not include the Keynesian framework (Bjerkholt 2014b).

<sup>184</sup> Jacob Marschak’s Papers, box 148, folder Klein, quoted in Mirowski 2002, 247.

Walrasian theory before promoting econometrics. According to Koopmans, only the Walrasian framework acted as warrantor of an accurate use of econometrics. This view implied that econometrics should deal only with the most advanced “modern economic theory,” and not with any other kind of obsolete pseudo-theory. In short, the Cowles Commission did not only seek to provide a powerful scientific tool for economists but to promote a particular theoretical framework and then to reinforce it by means of econometrics.

The rejection of Klein’s paper and Koopmans’s position in the controversy shows that the Commission was advocating for the promotion of Walrasian theory and that important divergences existed between Klein and his colleagues. On the one hand, the refuted paper is an indication that Klein’s pluralistic approach did not always fit perfectly well with the rigid position held at Cowles in terms of their use of economic theory. On the other hand, Koopmans’s definition of the Walrasian framework as the most advanced and complete economic theory clearly contrasts with the more flexible and pragmatic macroeconomic model building appraised by Klein, in which he promoted the use of economic hypotheses stemming from different economic theories to complete and estimate his models.

### 4.3. Klein’s way of building macroeconomic models

#### 4.3.1. Two ways of dealing with the problem of multiple hypotheses at the Cowles Commission

According to the Commission’s image, the problem of multiple hypotheses had already been solved, since economics counted with the most general and fundamental theory explaining economic phenomena: Walrasian general equilibrium theory. The real problem the Commission faced in its econometric venture was, as Mirowski (1987, 220-221) put it, to verify “the validity of *neoclassical* theory, and not all theory *tout court*.”

From within the Commission, however, Klein dealt with the problem of multiple

hypotheses in an entirely different way. He “desired to impress upon the reader that the models of [his] volume [were] put forth in full knowledge of the existence of the problem of multiple hypotheses” (Klein 1950, 122). More than a problem, the existence of multiple hypotheses represented for Klein an opportunity for both enriching the process of model specification, and demonstrating the power that the econometric tool represented in generating some kind of consensus out of a variety of theories.

Klein’s paper, “Theories of Effective Demand and Employment,” formerly rejected by Marschak and Koopmans, and published one year later, in 1947, in the *Journal of Political Economy*, is representative of the way Klein made use of different economic theories. In this paper, Klein discussed the possibility of building the same econometric model on the bases of any of the following theories: Neoclassical, Marxian or Keynesian.<sup>185</sup> Three years later, in 1950, in his first celebrated macroeconomic book, Klein (1950, 63-64) defended again his fundamental view on econometrics by presenting his pluralistic way of building macroeconomic models. He began by calling the reader’s attention to the resemblance between his Model I, Kalecki’s models of the business cycle (Kalecki 1935), “and some of the doctrines of Marxist economics,” referring to his own model as a “Marxian theory of effective demand” (Klein 1950, 63-64):

It is possible to develop this model [...] from the un-Marxian principles of utility and profit maximization, but it is also possible to develop this model from purely Marxian principles [Klein 1947a]. The same model can be consistent with a multiplicity of hypotheses.<sup>186</sup>

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<sup>185</sup> De Vroey and Malgrange (2012) present serious doubts about the Keynesian spirit of Klein’s models.

<sup>186</sup> Klein (1950, 63-64) also claimed in a somewhat defensive tone that “the problem of developing models from Marxian principles [was] of great interest from the point of view of the history of economic thought, but [that this was] not an essential problem of this book, which [was] concerned mainly with quantifying a true description of the structure of [the] United States economy [and that he mentioned] this relation only in passing, as a point of general interest.” It is worth remembering that, by 1950, Klein was not a member of Cowles anymore and that the econometric program had already lost much of its earlier enthusiasm, as well as its financial and institutional support within the Commission. In fact, the Cowles Monograph 11 was the result of Klein’s work at Cowles from 1944 to 1947, only published in 1950 (Bjerkholt 2014b).

In both works, Klein (1947a; 1950) wanted to convince the reader that irrespective of the economic beliefs econometricians might have, econometrics should be a tool adequate to lead the researchers to the same conclusions:

It is desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgements of a particular investigator. Econometric models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims (Klein 1947a, 111).

Klein's (1950) celebrated Model I is thus a good example of this model-building procedure. This simple model consists of "a completely determined system containing three statistical equations (i.e. three equations involving random terms and unknown parameters) plus some definitions or identities" (Klein 1950, 58). The model had the following form:

$$\begin{aligned}(1) \quad C &= \alpha_0 + \alpha_1 W + \alpha_2 \Pi + u_1 \\(2) \quad I &= \beta_0 + \beta_1 \Pi + \beta_2 \Pi_{-1} + \beta_3 K_{-1} + u_2 \\(3) \quad W &= \gamma_0 + \gamma_1 Y + \gamma_2 Y_{-1} + \gamma_3 t + u_3 \\(4) \quad Y &= C + I + G \\(5) \quad Y &= \Pi + W \\(6) \quad \Delta K &= I\end{aligned}$$

Where  $C$  is consumption,  $W$  is the wage bill,  $\Pi$  is non-wage income (profit),  $I$  is net investment,  $Y$  is output,  $t$  is time and  $G$  are the goods demanded by the government and foreigners, and  $u_i$  is a stochastic error term. All variables are expressed in constant dollars.

Let us take one equation – the investment function (2) – as an illustration of Klein's way of constructing his models. According to Klein, since in both the Classical and the Keynesian cases, the investment function is derived from the principles of profit-maximization, I will first describe the construction of equation (2) from these points of view, then, I will come back to the Marxian path leading to that equation.

The methodologies of classical and Keynesian economics do not differ [...] Both theories are based on [...] business-firm profit- (or utility-) maximization to get the demand for producer goods, labor and business cash-holdings (Klein 1947b, 117).

The principle of utility-maximization provides Klein with the possibility of developing equation (2) going through two different ways. On the one hand, he can derive equation (2) from “the heuristic principle [...] that profits are the mainspring of economic action in a capitalist society. Entrepreneurs expand when profits are anticipated to be high and contract when profits are anticipated to be low” (Klein 1947b, 60). Therefore, Klein establishes a positive relation between investment  $I$  and expected profits, which depends upon profits today  $\Pi$ , and precedent profits  $\Pi_{-1}$ . “However, not only the absolute size of profits but also their relation to the existing stock of capital is important; hence the variable  $K_{-1}$  is introduced” (Klein 1950, 60).

On the other hand, Klein also derives equation (2) by dividing the economy into two social groups: “workers and capitalists” or, in this case, consumers and producers, and assumes that both groups attempt to maximize their respective satisfactions. In short, Klein first looks into the behavior of the individuals within the social classes, then aggregates their individual equations in order to get the total demand for investment goods. The consumers’ satisfactions depend upon the current and future consumption of household goods and services, while the producers’ “satisfactions depend upon the use of current and future consumer goods and services and also upon the consumption of producer goods in their possession.” Producers’ income “can be used for two purposes, to spend on consumer goods and to spend on producer goods. They derive ‘pleasure’ from both types of spending” (Klein 1950, 60-61). Producers’ utility-maximization can thus lead to equation (7), which represents the demand for producer goods:

$$(7) \quad d_j = \beta_{0j} + \beta_{1j}\pi_j + u_j$$

Taking into account that in the classical world the use of capital is represented by:



$$d = d(i, k_{-1})$$

Klein finally derives to the individual's equation of investment (8).

$$(8) \quad i_j = \beta_{0j}^* + \beta_{1j}^* \pi_j + \beta_{2j}^* (k_j)_{-1} + u_j$$

When is aggregated over all firms, equation (8) becomes equivalent to equation (2). Note, however, that Klein's way of aggregating individuals' behavior was also of particular importance. Although Klein's method of aggregation is beyond the scope of this chapter, it is worth a brief mention, as it is key in Klein's model building strategy.<sup>187</sup> Samuelson (1983) himself denoted Klein's aggregation approach as "envelope aggregation." Rather than deriving a macroeconomic theory from the mere aggregation of microeconomic theory, Klein took "the existing macro and micro theories as given and then [measured] the economic variables in a way that [aimed] to insure consistency between both sets of theories [...] The distinguishing feature of Klein's approach is that measurement is what is endogenous rather than theory" (Marquez 1985, 3).

In the case of deriving the investment equation from Marxian principles, Klein recognizes that Marx's equations did not represent complete systems of equations but mainly definitions, and so, he searched "through Marx's literary explanations and numerical examples for the strategic hypotheses that [...] produce a determinate system of equations" (Klein 1947b, 120):

Our model is intended as an extension of the Marxian analysis to a logical conclusion in terms of a theory of effective demand. Actually, Marx laid the groundwork for a complete equation system to determine the level of income (effective demand) but did not build the complete system.

Furthermore, Klein (1947b, 118) recognized the methodological differences between the Marxian, and the classical and Keynesian cases, in which "instead of studying the behavior of individuals, Marx [studied] the behavior of classes directly," producing different kinds of

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<sup>187</sup> For a comprehensive discussion of Klein's aggregation methods see Klein (1946a; 1946b; 1950) and Hoover (2012).

macroeconomic systems. In the classical and Keynesian systems, the macrounits (producers and consumers) overlapped, failing “to bring out some essentials.” Since the macrounits of the Marxian system (workers and capitalists) were “practically exclusive,” this allowed for singling out their conflict of interest more easily, facilitating the study of “one of the most important moving forces in the system” (118), i.e. class struggle.

And so, in the case of the Marxian determination of the demand for investment goods Klein followed two steps: first, he derived “the demand relation for constant capital (capital used up) according to Marx and then [transformed] the demand for constant capital into investment” (120). Since workers only demand consumer goods in the Marxian system, the demand for constant capital is based entirely on the behavior of capitalists. Klein, then, searches for Marx’s numerical examples in *Volume II of Das Kapital* to establish the form of the Marxian demand for investment goods. First, the demand for investment goods will depend only upon the behavior of capitalists, because workers buy exclusively consumer goods. Since, as Marx explains, capitalists in Department I (the producer-goods industry) spend from surplus value  $S$  on constant capital  $C$ , then, Klein obtains the following equation:<sup>188</sup>

$$(9) \quad C = \beta_0 + \beta_1 S$$

Klein, then, attempts at a transformation of this equation in order to work with the variable  $I$  instead of  $C$ . He denotes the capital acquired during the  $p^{th}$  preceding time period by  $x_{-p}$ :

$$(10) \quad C = C(x, x_{-1}, x_{-2}, x_{-3} \dots)$$

Making a linear transformation of (10) he gets:

$$(11) \quad C = \delta_0 + \delta_1 x + \delta_2 x_{-1} + \delta_3 x_{-2} + \dots$$

Because of the impossibility of statistically measuring in a separate way the capital purchased during every preceding period, Klein approximates all these variables by means of a proxy

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<sup>188</sup> I keep Klein’s (and Marx’s) original notation, where  $C$  is constant capital, not consumption.

variable, representing all the capital accumulated until the period under consideration (Klein 1950):

$$(12) \quad C = \delta_0 + \delta_1 x + \delta_2' Z_{-1}$$

Furthermore, he writes the stock of existing fixed capital in terms of the net investment of all preceding periods as:

$$(13) \quad Z_{-1} = \sum_{t=-1}^{-\infty} I_t$$

And because it is net investment, and not gross investment that interests him, Klein writes:

$$(14) \quad x = I + C$$

The next two steps before arriving at the final result are algebra manipulations. He substitutes (14) in (12), and gets:

$$(15) \quad C = \delta_0 + \delta_1(I + C) + \delta_2' Z_{-1}$$

And finally, he replaces (9) in (15), getting:

$$(16) \quad I = \beta_2 + \beta_3 S + \beta_4 Z_{-1}$$

Note that equation (16) is equivalent to the original investment equation (2).

Beyond the algebra and the simple and elegant mathematical final form of the Marxian, Keynesian and classical model of effective demand, the important point is that Klein obtains the same equations and arrives at the same model departing from three different theories. Klein recognizes, however, that he could have obtained many different model specifications from each of the theories and that he had to select carefully the parts of the theory that were useful for his particular task. He had not used “Marx’s methods to their fullest extent,” but “only those aspects of Marx’s theories” which were “necessary to build a complete system of equations” (Klein 1947a, 125-126).

He carried on the transformations of the equations and expressed the hypotheses in a peculiar way, so that he could obtain the same final equations. However, he did not just select

any hypotheses carelessly. On the contrary, he confronted each hypothesis with data, and used its good fit as a criterion to reject or accept it.

Many of the parts of the Keynesian system have withstood the test of being consistent with observed data [...] the author has applied various methods of statistical estimation to the Marxian model and has found the estimated parameters to be very reasonable in size. Moreover, the model fits the observed data very closely (Klein 1947a, 116-127).

In short, Klein based the construction of his models on the multiplicity of hypotheses. In his process, which appears quite close to that proposed by Marcel Boumans (1999; 2005), Klein “baked” his models using different bits and pieces of distinct theories as “ingredients” for his model construction, without following any well-defined recipe. He developed mathematical models of the economy as a whole based on hypotheses of different, and sometimes fundamentally opposed economic theories. Then, he confronted these hypotheses to data, retaining those that seemed to fit the observations. Although I did not describe Klein’s confrontation of the hypotheses with data, one can imagine that the whole process of model specification was inevitably accompanied by arduous work, consisting on tedious, repetitive and sometimes even disappointing tasks and results, characterized by a highly collaborative teamwork effort. And yet, despite this painstaking effort, the first results of Klein’s macroeconometric model building proved somehow disappointing.

#### 4.3.2. Painstaking effort and disappointing results of the Cowles’s methods of estimation

Not only did the main figures of Marschak’s econometric program, such as Koopmans, Hurwicz, and Marschak himself eventually abandoned econometrics, but the Cowles Commission also ceased to pay attention to econometrics, and to macroeconometric model building in particular (Klein 1991; Epstein 1987; Mirowski 2002; 2012). The effort of the Commission to regain attractiveness for funding might be partly responsible for the abandonment of econometrics (Mirowski 2002; 2012), since other research programs such as

activity analysis became a priority in their agenda. Yet, there is another part of the story related to technical reasons, that accounts for the loss of enthusiasm and faith in the program.

Econometrics *à la* Cowles was a painstaking activity with “three joint lines of interest: 1. economics, 2. statistics, [and] 3. mathematics” (Klein 1991, 109). These lines of interest, combined with the high ambitions of Cowles, were not necessarily easy subjects to develop, for they constituted the source of a painstaking activity, heavy mathematical and statistical work, long and tedious calculations, repetitions, many drawbacks and disappointments when in the end, the whole econometric exercise had to be performed all anew again and again (Klein 1991). The emphasis that each author placed on the different lines of research is actually very informative of the images that every econometrician held about econometrics. As we have seen from the Measurement without Theory controversy, unlike Koopmans who accorded more importance to “statistics” and “mathematics,” Klein was willing to accord more importance to “economics.”

One has to keep in mind that the technical possibilities available at the time to perform the calculations involved were completely different from the ones available nowadays. Without the aid of advanced calculation machines such as the digital computer, “elaborate calculations were slow, complex and awkward to carry out” (Mirowski 2012, 147). The research field of econometrics was then seen as an activity that needed a teamwork effort to be successfully fulfilled, partly because of the burdensome calculations. The team assembled by Marschak at Cowles was divided in a very specific way, where each worker was in charge of an explicit task, so that the team could figure out the complex solutions in the most efficient way.<sup>189</sup>

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<sup>189</sup> Klein (1991, 109-110) gives a very clear account of the division of labor within the Cowles Commission: “When [Marschak] recruited [Klein], it was explicitly to prepare model specifications according to received theory [...] In addition [he] was assigned the task of data preparation and model estimation/testing [...]. Haavelmo was recruited to work on econometric theory, Anderson to work on the underlying theory of mathematical statistics, Koopmans to work on overseeing all the pieces but especially on implementation of the work, through computation that was very complicated and

When the program was initiated in 1944, Marschak claimed that Cowles would deliver powerful new results for economic analysis in just three years (Klein and Mariano 1987; Klein 1991). Unfortunately, the results of the first macroeconometric models did not prove very convincing, not only within Cowles, but also in the other institutions based at Chicago and Washington.<sup>190</sup>

Inside Cowles, Marschak’s reactions with respect to the results of Klein’s models were quite clear. “As early as 1946 [...] Marschak [...] did not wish to claim much for Klein’s early efforts” (Epstein 1987, 105):

The present admittedly very crude and preliminary results were tentatively applied to measuring the effects of policies; though it may have been wiser not to include the discussion even in a privately circulated monograph (Marschak 1946, quoted by Epstein 1987, 105).<sup>191</sup>

In 1948, Carl Christ, was hired by Koopmans to revise Klein’s Model III, re-estimating it for the period 1921-47. Christ’s revision consisted in two steps. First, he carried out two statistical tests on Klein’s model, which consisted in what he called “structural equation

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tedious, given available facilities of the day. Rubin worked on econometric theory and mathematical statistics; Hurwicz [...] contributed to all aspects of the work; Leipnik was a mathematical statistician for the project. Patinkin was assigned work on a sectoral model for manufacturing, but drifted more towards an interest in the underlying Keynesian macrotheory.”

<sup>190</sup> Albert Hart, a member of the Committee for Economic Development (CED), convinced Marschak and Klein to present, in 1945, the projections of their macroeconometric model to the Bureau of Budget, the Department of Commerce and the Federal Reserve Board. Klein was not very enthusiastic at the beginning because he not only thought that his results were too preliminary, but also because he thought that the results would be very pessimistic (Klein 1991). “To [his] surprise, this first exercise, though premature, was very bullish” (Klein 1991, 114). However, the reaction of the governmental agencies in Washington was not very optimistic. “The Cowles-CED projections were not taken seriously; the response in all cases was that we should wait until mid year 1946, when we would find 6 million unemployed again and a return to [the] conditions of the Great Depression” (Klein 1991, 114-115). The general belief of U.S. economists that the economy would fall back into a slump period after World War II, might explain this negative reaction to the Cowles-CED projections. If the Cowles projections were not being taken *seriously* by the governmental agencies it was because the Commission lacked credibility, and not because its methods were rudimentary or not robust enough.

<sup>191</sup> Klein (1991, 115) was of course aware of the skepticism that his macroeconometric modeling results provoked in the Cowles’s directorship. With hindsight he described the situation in the following way: “In general, the senior researchers at the Commission were not satisfied with the performance of models that had been constructed during the expansionary phase of the research program and there was relatively little carry-on activity in empirical model building with repeated applications over sustained time periods.”

tolerance test” (SETI) and a test by means of “naïve models,” for each structural equation. The SETI test consisted on constructing a tolerance interval for each structural equation, and rejecting the equation if its calculated disturbance fell outside the interval, while the test through naïve models consisted in comparing whether each disturbance of the structural model is larger than the error one would expect to make by a very simple naïve model (see chapter 5). Second, Christ estimated the model from a sample that included Klein’s sample and the years 1946 and 1947, and revised the estimated model against the new data for 1948.

His results, however, did not prove very promising either. Out of 5 of 16 equations of Klein’s original Model III were rejected and specified again. In the conference on business cycles held at the NBER at the end of 1949, Christ’s revision of Klein’s Model III was heavily criticized by prominent economists such as Friedman, Leontief, Schumpeter and Metzler. Schumpeter (1951, 161), for instance, was worried that the “elaboration of models of the type discussed by Mr. Christ, combined with the usual shortages in the supply of data as well as with the effort at prediction of the immediate future, results in a foreshortening of the time perspective of analysis that has dangerous consequences.” Even economists who had actively participated in the conception of the model – like Klein, Marschak and Koopmans – were skeptical about the results saying, for instance, that “the test of straight forecasting performance of the improved Klein model shows that, indeed, we are still quite far from a knowledge of the economic structure sufficient to give useful forecasts” (Koopmans 1951b, 144-145). It was, in any case, the prelude to the end of the econometric program at Cowles, which driven by new (military) funding, was being redirected towards different fields of research, such as activity analysis (Mirowski 2002). But Christ himself was also quite critical about Klein’s model. He described Klein’s predictions of the price level and disposable income for 1941 as “absurd” (Christ 1951, 125), reinforcing the already existing general

pessimism about macroeconometric empirical work from within the Commission.<sup>192</sup>

Although Klein himself had been quite self-critical about his own models, his criticism focused not so much on his results, but mainly on the estimation methods promoted at Cowles. With hindsight, Klein recognized that it was not the methods developed at Cowles during the 1940s that should be credited for the increased accuracy and usefulness of econometrics, but rather the inclusion of institutional reality and of a priori knowledge. According to Klein (1960, 867), “if econometric results are today more useful than in the past, this is only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature.” Furthermore, Klein “would expect marginal improvements of five or ten per cent through the use of more powerful methods of statistical inference,” concluding that “the adoption of more powerful methods of mathematical statistics is no panacea.” With hindsight, Klein (1991, 113-114) remembered that “great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of econometric analysis.” He, however placed “more faith on the data base, economic analysis (both institutional as well as theoretical), political insight, and attention to the steady flow of information.”

Another recurrent criticism of the Cowles’s methods of estimation increased the skepticism about the structural econometrics program. It was not clear for the economists why they should adhere to the methods of estimation like the maximum-likelihood-method

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<sup>192</sup> Other criticisms also strengthened this pessimistic environment towards structural econometrics. Herman Wold (1951), for instance, “continued to question the logical status of causality in the simultaneous model. Robert Solow (1951) wanted an explanation for why simultaneous equations estimators and OLS estimators seemed to yield similar results in practice. Theil (1954) “outlined a theorem showing that the generalized variance of least-squares estimates of the parameters in a single equation is at least as small as that of limited-information- maximum-likelihood estimates” (Klein 1956, 217). And, of course, also the NBER members attacked the lack of results of Cowles methods: “The only satisfactory test of the usefulness of [the Cowles] methods is their fruits, and these have not yet been attained, or if attained, have not yet been made generally available” (Vining 1949, 77).



(MLM) or the limited-information-maximum likelihood method (LIML), when more pedestrian methods such as the ordinary least squares (OLS) seemed to generate equally accurate results with a considerable smaller amount of effort. Economists as prominent as Robert Solow (1951, 359-360), expressed their doubts about the superiority of the Cowles's methods, in particular to the "remarkable fact that when [both methods] are applied to the same data, [they] often give similar results" and when results differ, "good sense often favors least squares" over Cowles's methods.

Beyond the technical issues largely discussed in Klein (1956; 1960) and Epstein (1989), it is worth mentioning that the economics community was just not convinced that they should undertake such a painstaking effort in order to get results that seemed to be just as good as those obtained by means of more pedestrian methods. Not all economists had the possibility of embracing such a burdensome task, as macroeconometric modeling required a great amount of institutional and financial support, the disposition and eagerness to undertake tedious and teamwork-based work, some kind of warranty that the results would return the effort, the trouble and the time invested, proving its usefulness in governmental affairs (or in the industry, as Klein would show from the 1960s).

In short, to Klein (1991, 115-116) the important lessons learned from his experience at Cowles are that the models should be flexible enough for the whole team to be able to introduce new relevant information, and that more sophisticated statistical methods did not necessarily mean that better results would be attained. In his own words (Klein 1991, 115-116) "it [was] important to grasp the simultaneity of the macroeconomy but not necessarily to tie statistical estimation methods exclusively to this property, and so it was "more important to be able to update, correct, or revise estimates on the basis of a steady flow of important new information, and very flexible methods of estimation are needed for this purpose." According to Klein, "the highly flexible methods can be more powerful in simple form than

the more complicated procedures that we were following at [...] Cowles.” In the end, it was clear to him that “the spirit of what we were trying to achieve [...] can best be reached by statistical methods that are simpler than those that we thought were most powerful at the Cowles Commission.”

#### 4.3.3. Klein’s appeal to institutional reality and his ‘methodological structuralism’

Nell and Errouaki (2013) labeled Klein’s appealing methodology “methodological structuralism.”<sup>193</sup> Contrasting with the Cowles’s methodological individualism, Klein’s methodological structuralism does not necessarily start with the individual as the one and only unit of analysis. Klein recognizes the existence of the individual, of course, and understands that her behavior affects the economy in an important way. But Klein’s focus is on the discovery of the underlying structure of the economy, and so, he understands that it is the social and economic institutions, which constitute the most fundamental pieces of this underlying structure. Klein provides an explanation of economic events based on a vision of the social world that is made up by institutions, roles, responsibilities and powers, and considers the economic system as one built on structured objects “whose power exist independently of [the individuals’] knowledge or perception” (430). It is these objects, relationships and powers which form the relations between economic variables. “Employers *can* hire and fire workers and *can* order them around; firms *can* move capital from place to place opening and closing plants” (430, my emphasis).

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<sup>193</sup> In fact, Nell and Errouaki (2013) have treated Klein’s methodology as if it presented no significant difference with the Cowles’s methodology. Their claim is that the structural econometrics program at Cowles was in the “right” way, and that econometrics today should take again this track and come back to the structural econometrics program. My aim is not to appraise whether Nell and Errouaki’s claim is just or not, nor is it to say what econometricians should do today. From an historical point of view, however, I disagree with Nell and Errouaki (2013) in their intention of bringing the Cowles and Klein’s methodology to a common level. I think that they wrongly equate Klein’s position to that of Cowles. Nell and Errouaki take Haavelmo’s position as if it were the Cowles’s official stand. Yet, even if Haavelmo visited Cowles for a few years, and even if he developed his 1944 paper during his time there (Bjerkholt 2007), he does not represent the Cowles’s official position. It is rather Marschak and Koopmans who, as research directors, better represent the Cowles’s official position.

These examples, however, could be also applicable from the perspective of methodological individualism: the employer could individually decide to hire somebody, or the firm, viewed from the Neoclassical point of view as an individual, could decide to reallocate its capital as a result of some kind of individual decision. Yet, what is relevant here is that the structure of the economy (and society) provides a special kind of institutionalized *power* to particular participants in the economy and society. To Klein, the econometrician willing to discover the underlying structure of the economy should not only describe the fact that the employer optimizes his choice in the model, but also that the employer has the power to hire or fire somebody, while the employee has no power at all to keep his job or to get a new one.

In order to improve his model, the researcher has the possibility (or the duty) of introducing more accurate *a priori* information into the mathematical model, which reflects, for example, the relation of power between the employers and the employees in the labor market. *A priori* information is a kind of knowledge about the economy as a *whole* and “is [therefore] independent of the particular sample being used [and] [...] may consist of economic theory, a knowledge of economic institutions, a knowledge of technology, or empirical results from independent samples” (Klein 1957, 2).

*A priori* information stemming from “knowledge of technology” means that some improvement could be obtained through the development and refinement of more sophisticated methods of statistical inference. As mentioned above, the improvement of this kind of knowledge would be much closer to the line of research followed by the Cowles Commission. Klein, however, did not believe that these technical improvements would be decisive for the advancement of econometrics. Rather, Klein (1960, 867) thought that it was the improvement of institutional reality and the refinement of data, which would decisively contribute to the improvement of econometric modeling:

The building of institutional reality into a priori formulations of economic reality and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference.

Klein did not just defend his intuitive idea that “the more that relevant information is used, the better are estimates that make use of it” (Klein 1985, 8). He actually found a formal way of expressing and demonstrating that the use of more relevant *a priori* information would produce more efficient statistical estimators. To demonstrate his result, he referred to a concept he had learned from his PhD supervisor, Paul Samuelson: Samuelson’s principle of Le Chatelier, where he “showed that the equilibrium values of the diagonal elements of a certain matrix of bordered second-order derivatives of a consumer’s utility function become smaller and smaller as more restrictions are placed on the maximization of the utility function,” demonstrating that “price sensitivity (elasticity) is reduced as additional restrictions are imposed” (Klein 1985, 8). Klein noticed that “there was an analogy between the matrices of second-order derivatives in utility theory and in maximum likelihood theory for econometric estimation,” arguing that “as a priori information is added, the bordering increases and the estimation-efficiency measures improve” (8).

Klein had realized the difficulties of applying the estimation methods of the Commission in everyday econometrics. He had also recognized that such methods did not generate clearly superior results when compared to more pedestrian methods like the OLS (Klein 1950; 1955). Although Klein respected and adopted to some extent the methods developed at Cowles during his stay there, it was clear to him that pedestrian methods were at least as effective as more sophisticated methods when faced with practical and applied problems. Klein was certain that doing econometrics was a matter of understanding the economy, and that econometrics required the integration of institutional reality into the mathematical construction of the econometric models.

The implementation of structural macroeconometric models in a particular country

provides a good example of the importance of taking into account institutional reality. Klein was aware of the fact that every country that wanted to build a macroeconomic model should undertake a serious study of the particular way in which its economic and social institutions really worked. Long before he embarked in Project LINK, Klein recognized that there were particular institutional factors typical of every country that had to be taken into account, in order to build adequate macroeconomic models.<sup>194</sup>

A workable model must be *dynamic* and *institutional*; it must reflect processes through time, and it must take into account the main institutional factors affecting the working of any particular system. Different features must be built into adequate models of such diverse economies as the United States, Canada, the United Kingdom, Japan, the Netherlands, etc. Models of non-capitalistic societies will differ even more radically from the models of capitalist countries, with investment not an endogenous magnitude (Klein 1954, 279).

#### 4.4. Concluding Remarks

Lawrence R. Klein conceived econometrics as a pluralistic scientific tool for economic planning. As a pluralistic tool, econometrics could integrate different and sometimes conflicting theories and hypotheses, providing the possibility of better understanding and intervening the economy. Intervention should, according to Klein, go beyond the reforms of the capitalist system, implemented in order to really change the economic system and its underlying structures of power. The daily practice of econometrics would imply a field of applied and theoretical research characterized by a great deal of teamwork and discussion, not only during an epoch of technical impediments like the 1940s and 1950s, but also in the computer era when calculations should not pose a real problem anymore (Klein and Mariano 1987). Econometrics would also be a discipline characterized by a great deal of tinkering and thinking, crossing the boundaries between theoretical and applied work in economics,

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<sup>194</sup> Project LINK started in 1968 and was initially founded by the Rockefeller Foundation. It “sought to integrate the macroeconomic models of different countries, which eventually included Third World countries and socialist nations, into a total simultaneous system” (Mariano 2008, 8). For a more comprehensive account of Project LINK see (Bodkin *et al.* 1991, chapter 14).

statistics and mathematics, and also between history and politics. As a method of inquiry, the scientific practice of econometric modeling, would provide the possibility for economists to find out about the world.

Klein did not just “look” at the model in order to learn from it; while building and manipulating the model, Klein was tinkering, thinking, adjusting, discussing and thinking again. On the one hand, Klein’s openness towards Neoclassical, Keynesian and Marxian economic theories played an important role in the building of his models, providing him with the possibility of enriching his hypotheses and of specifying his model equations in various ways. His methodological structuralism and the fact that he was taking into account institutional reality proved much more flexible and more applicable to a variety of contexts than the more rigid methodological individualism promoted at Cowles. On the other hand, Klein promoted the idea that “macroeconomic modeling was not a once-and-for-all-job” (Klein 1950; 1955), but rather a practice consisting on rethinking, re-discussing, re-specifying and re-estimating the models, and on including new relevant institutional information and data.

In a nutshell, two elements of Klein’s image of econometrics truly exerted important effects for the further dissemination of macroeconomic modeling. Klein not only softened the rigid econometric approach from Cowles by making it less theoretical and more familiar to the reality of economists’ practices. He also enriched the econometric approach by introducing elements of institutional reality in his models thus rendering econometric modeling not only a tool ready to intervene the economy, but also a practice allowing for the improvement of our knowledge of economic relations and phenomena. These elements shaped Klein’s image of econometrics, an image capable of being disseminated throughout the economics community, revealing econometrics as a powerful scientific tool, applicable to all economic streams (main or not), and providing some standards of how to actually

undertake econometric studies. Klein's image of econometrics captured in his early publications made econometric modeling a feasible and useful practice for economists, and became the source of inspiration and the methodological standard in the field of large scale macroeconomic modeling that flourished in the 1960s.







Part II:  
The Consolidation of Macroeconometric  
Modeling



Fabian Bachrach

LAWRENCE R. KLEIN  
President of the Econometric Society, 1960  
Vice-President of the Econometric Society, 1959



# The consolidation of macroeconomic modeling

The direction of work that seems to me to offer most hope for laying a foundation for a workable theory of change is the analysis of parts of the economy in the hope that we can find bits of order here and there and gradually combine these bits into a systematic picture of the whole. In the language of the model builders, I believe our chief hope is to study the sections covered by individual structural equations separately and independently of the rest of the economy. These remarks obviously have a rather direct bearing on the desultory skirmishing between what have loosely been designated the National Bureau and the Cowles Commission techniques of investigating business cycles [...] The difference between the two approaches seems to me much greater in abstract discussions of method than it is likely to prove in actual work [...] Thus, I predict the actual work of the two groups of investigators will become more and more alike.

*Milton Friedman, 1951*

As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology. It was not purely methodological, however. A central issue was that we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by [...] the National Bureau.

*Lawrence R. Klein, 1991*

Lawrence R. Klein was responsible for the creation of the new scientific practice of macroeconomic modeling that changed the way to produce macroeconomic knowledge in the second half of the twentieth century. Up to this point, I have shown how Klein forged his identity as a macroeconomist and how he set the bases for the development of macroeconomic modeling. Not only were the political, social, and academic contexts of the time decisive elements for Klein to become a macroeconomist, but so were too his formal education and training, as well as the people and institutions that crossed his life during these early years. I have also shown how Klein himself, became a figure who shaped both institutions and economists, in a way that was necessary for the further development of macroeconomic

modeling. In the second part of my dissertation, I want first to trace back the way in which Klein consolidated his new practice within the macroeconomics community; second, I want to understand the effects that the consolidation of macroeconomic modeling had in the production of economic knowledge.

Throughout the second part of my dissertation, I study the longstanding relationship between the NBER and the Cowles Commission as the opposition of two alternative empirical approaches to macroeconomics. Although they seem to be opposed at first sight, I argue that these alternative approaches partook of a common background that allowed the protagonists of both factions to embark in a productive scientific conversation. This common background is reflected in the way scientific practices were constructed and sheltered around institutions, scientific objectives, and methodological principles. Institutions such as Cowles and the NBER exerted important effects, which were not only of symbolic value, but also of material substance. In terms of the symbolic significance, the NBER was situated in New York, close to Columbia University, and represented a long tradition in empirical research going back to Mitchell, Burns, and Kuznets, to mention only a few.<sup>195</sup> Since its move to Chicago, the Cowles Commission represented a new approach that emphasized on a heavy-load theoretical work both in terms of economics and mathematical statistics, reinforced by the presence of European *émigrés* such as Marschak, Haavelmo, Koopmans, and Hurwicz. Although very different in their characteristics, both institutions offered a series of material conditions that are comprehensible only in the context of the interwar and postwar periods, in the rise of philanthropic institutions, and, most importantly for our purpose, in the idea of “big science” in which important laboratories composed by numerous researchers and collaborators were built across the United States with help of governmental, military, and private funds.<sup>196</sup>

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<sup>195</sup> For a historical account on the NBER see Fabricant (1989), Morgan (1990, chapter 2), and W. A. Friedman (2014, chapters 4 and 5).

<sup>196</sup> Despite the state and military provenance of important parts of funding, it is important to understand that the relationship between the sciences and the governmental agencies was very complex. As Cohen-

This common background was also characterized by a shared concern to provide a kind of macroeconomics that was necessarily empirically-based. It is true that these approaches differed in their emphasis on data and theory, as well as in their modeling strategies, and yet, they were both concerned with concrete observable problems, and not with pure theory alone. Also, the representative figures – Klein and Friedman – sustained heated debates about methodological issues that were based in reflections related to their modeling practices. In fact, an important point in this respect consisted in the discussion on the invention and adoption of the most adequate modeling strategy allowing for a better understanding of the economy, and for evaluating the intervention of the economy in concrete ways.

On the one hand, Klein advocated for a modeling strategy that considered the economic system as a *whole* at all stages of the modeling process, and which would progressively build in more detail of the economy to construct a model that becomes more complex. Klein labelled his methodology “Walrasian.” On the other hand, Friedman promoted a modeling strategy that *partitioned* the economic system into smaller portions that he could understand and study to its full extent; then, out of these partial models, Friedman was able to understand some underlying mechanisms of the economy that he extrapolated to other portions building, little by little, a more complex system as well. Although he acknowledged the inherent complexity of the system, Friedman (1951) maintained that “we know so little about the dynamic mechanisms at work that there is enormous arbitrariness in any system set down”; and that “until we can develop a simpler picture of the world, by an understanding of interrelations within sections of the economy, the construction of a model for the economy as a whole is bound to be almost a complete groping in the dark” (112-113). Friedman labelled his methodology “Marshallian.”

I call attention to the fact that it was Klein and Friedman who actually described their methodologies in Walrasian and Marshallian terms, inscribing themselves in older traditions in

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Cole (2014, 7) puts it “far from being devoted to fighting communism, or to command and control technologies, the open mind [concept in sciences] was intended to make America more liberal.”

the history of economics. Yet, their interpretation is one proper to the 1940s and 1950s US-American economists, and must be understood as labels, not as perfect vindications of Walras's or Marshall's actual programs in economics. To be sure, in the following chapters the labels "Walrasian" and "Marshallian" are accompanied by a prefix "US," and must be understood as historically and geographically placed, and not as universal categories.

Throughout the next three chapters, I show that there were important epistemological consequences brought by the construction, adoption, and consolidation of macroeconomic modeling. The way macroeconomic knowledge was produced changed with the use of the new macroeconomic tool, and with the scientific practice in which the tool was embedded. I argue that the discussions on economic theory, application, policy, and data did not occur at the abstract level or in separate spheres. Rather, these discussions took place necessarily within the macroeconomic modeling practice. Every theoretical matter, like the form and components of the consumption function, for instance, were discussed within the framework of a macroeconomic model, necessarily directing attention to empirical and methodological issues, and to policy consequences. The scientific value of a theory, the evaluation of its policy consequences, the mechanisms that linked a particular variable with the rest of the system, was conceived through the macroeconomic tool. Hypotheses, for instance, had to meet the criteria of statistical tests, in order to be included in the model. In short, macroeconomists started to talk in macroeconomic terms to each other, and so, whether they agreed or not on the use of large-scale models, they started to think in these terms too.

In chapter 5, I consider the debates that Friedman sustained with the members of the Cowles Commission since the mid-1940s, and show that the scientific object produced at Cowles, addressed in Friedman's criticism, changed during these years. Cowles passed from producing abstract scientific objects such as Lange's (1944) *Price Flexibility and Employment*, to produce empirically-based objects such as Klein's (1950) *Economic Fluctuations in the United States*,

1921-41. Whereas Friedman (1946) dismissed Lange’s book as “unreal and artificial,” he recognized the efforts made by Klein to bring the Commission’s approach closer to a “thorough objective scientific manner” to test the predictive value of econometric models, engaging in an attentive, though, no less vehement discussion (Friedman 1951, 107). One of the results of these discussions was the development of Friedman’s “naive models” as a standard to compare the predicting performance of macroeconometric models which would prove very influential in the subsequent years.

In chapter 6, I explore the debate from a methodological point of view, characterizing the Walrasian and Marshallian approaches adopted by Klein and Friedman as their modeling strategies. To do so, I take two points of departure: first, I discuss Friedman’s metaphor of “engines and photography” and illustrate the differences between US Walrasian and US Marshallian methodologies. Quoting Alfred Marshall, Friedman (1949; 1953; 1955) insisted that economic theory (and economic models) should be seen as “engines for the discovery of concrete truth,” allowing the researcher to study a part of the reality that is observable, understandable, and manageable. Friedman (1949) argued that a Walrasian approach, on the contrary, consisted on a “photographic description of reality,” because in that case “theory [was] to be tested by the accuracy of its ‘assumptions’ [and] not by the correctness of the predictions that can be derived from it” (490-491). Second, I consider these debates under the light of *The Cournot Problem*, which consists on “how to cope with economic analysis using *practical* methods,” given economic interdependence and complexity (see Hoover 1988, 218-220). Even if Friedman’s answer to this problem is his adoption of his Marshallian methodology, I show that also Klein with his Walrasian pragmatic approach, offered a true alternative way to tackle this problem.<sup>197</sup> In addition, given the empirical and pragmatic nature of the two alternatives to macroeconomics, I argue that both approaches yielded “engines” or systems of thought for

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<sup>197</sup> Pedro Duarte suggested to me the term “Walrasian pragmatist” to designate Klein’s approach to macroeconomics.



understanding and intervening the economy.

Finally, in chapter 7, I revisit the specific 1957-8 controversy on “statistical illusions” between three future Nobel Prize laureates in economics: Klein, Friedman, and the young Gary S. Becker. This controversy provides a good illustration of the common background on which the representatives of the alternative empirical approaches to macroeconomics relied, and of the way in which scientific practices are criticized, discussed, and refined. In particular, the adequate form of the consumption function and its effects on the performance to predict income, make an interesting case to study how to build and use macroeconomic models, and how to choose a criterion to evaluate model performance. This controversy also sheds light into the way the macroeconomics community of the 1950s discussed and continued to develop their scientific practices.

In short, in the second part of my dissertation I show, on the one hand, the effects that the adoption of the macroeconomic modeling practice exerted on the production of macroeconomic knowledge. Through the use of macroeconomic models, the spheres of economic theory, application, and policy were fused into the same tool. On the other hand, I consider the longstanding relation between Klein and Friedman as representative figures of two alternative approaches to empirical macroeconomics, and show how their discussions, which were based on a common ground, were also decisive in the further development of the scientific practices adopted by the community of macroeconomists.





# Friedman's longstanding debate with the Cowles Commission

My own hunch [...] is that [...] attempts to proceed now to the construction of additional models along the same general lines [of the Cowles Commission] will, in due time, be judged failures.

*Milton Friedman, 1951*

### 5.1. Introduction

Through his structural macroeconomic modeling approach Lawrence R. Klein pursued a precise lifelong purpose: to build large-scale macroeconomic models that would reflect the complete structure of the economy through a system of simultaneous mathematical equations. It was evident to Klein that structural macroeconomic modeling would little by little reveal the structure of the economy through painstaking team effort, through the use of improved economic data and much tinkering, as well as through the revision of model specification and re-estimation of parameters. For a very optimistic Klein macroeconomic models would, eventually, be able to provide an accurate description and representation of economic “reality.”<sup>198</sup>

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<sup>198</sup> “Representation” in the case of models can be understood in two senses: (1) the sense used in this introduction indicates representation as the establishment of a direct correspondence (resemblance or denotation) between the model and the “real world.” This is what Frigg (2010) calls t-representation – “t” for the target of reality the model seeks to correspond to. Another way of understanding “representation” stems from “pretence theory” as presented by Walton (1990) and also by Morgan (2012). Representation, in this sense, (2) indicates an object that is imaginable within a particular set of rules defined in a model system. Frigg (*ibid.*) calls this kind of representation p-representation, “p” for prop. “Reality” too, can be understood in two senses. Here reality will not be understood in the sense of classical ontology as an external and objective entity, but rather in the sense of “historical ontology”

Defending a Marshallian approach, Milton Friedman radically disagreed with this position characterizing Klein's Walrasianism as a "photographic description" of reality (Friedman 1949). His criticism, however, did not rely on the classical opposition attributed to Walras and Marshall of general equilibrium *versus* partial equilibrium (see for example De Vroey 2009). In fact, there is a far more profound methodological claim standing between Friedman's disagreement with Klein. Ever since Friedman started to attend the Cowles Commission seminars in the 1940s to discuss about econometric modeling (Epstein 1987), he continuously expressed his skepticism about this approach, claiming that structural macroeconomic models *à la* Klein would "in due time be judged failures" (Friedman 1951, 112). Friedman's main criticism actually derived from the fact that he had a fundamentally different view on the role that economic models should play both within the economics discipline and within the political sphere.<sup>199</sup>

Klein's conception of models, allegedly inspired by Léon Walras, focused on the idea that models should be capable to represent and capture the essential structure of the economy as a description of reality. Friedman, however, in his own conception inspired by Alfred Marshall, considered that economic theories – or models – should be perceived as a way to construct systems of thought through the observation of specific parts of the economy, but never as direct representations of the economic system as a whole.<sup>200</sup> Hence, economic theory and, in this case,

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(Hacking 2002) or even in the sense of some kind of "imagined ontology" that is possible to conceive, only under the rules that are set to build the model system.

<sup>199</sup> The debate between Friedman and the Cowles Commission can be understood also as the continuation of the oppositions between the empirical approach of statistical economics represented by Mitchell and that of econometrics represented by Frisch. On this opposition see Dupont-Kieffer (2001).

<sup>200</sup> In twentieth century economics, it is very difficult to make a clear differentiation between a "theory" and a "model" for at least three reasons. First, understood as "systems of thought," both theories and models fulfill a similar function: they serve as "engines" to produce knowledge, and allow for the understanding of the economy. Second, standardization of the term "model" was not reached by the 1940s and 1950s, and so economists like Friedman and Klein used these terms, sometimes, in an undifferentiated way. Third, and this is the point of the present chapter, the introduction of tools like econometrics reconfigured the relationship between pure theory, application, policy, and data in macroeconomics, leaving not much space for the differentiation between theories and models.

macroeconomic models, should be considered as introducing “systematic and organized methods of reasoning” (Marshall 1885, 159) allowing for a better understanding and analysis of the economy. These methods of reasoning would therefore constitute an “engine for the discovery of concrete truth” (*ibid.*).<sup>201</sup>

Friedman sustained a long debate with several members of the Cowles Commission between the early 1940s and the late 1950s, coinciding with a period of high proficiency in his work on economic methodology. Partially responsible for the remarkable quality of the methodological work Friedman produced is the empirical turn of economics, and more particularly, the emergence and further development of econometrics at Cowles.<sup>202</sup> Friedman’s discussions with Oskar Lange, Jacob Marschak, Tjalling Koopmans, and finally Lawrence Klein, elucidate in concrete terms the difference between the purposes and the uses of economic modeling between what one could call US-Walrasian and US-Marshallian economists.<sup>203</sup>

I argue that both Friedman’s and Klein’s modeling practices yield a system of thought (or an engine) allowing for the production of knowledge.<sup>204</sup> Following Frigg (2010), I use the term “model system” to refer to Marshall’s “engines.”<sup>205</sup> “Model systems can be (and often are) used to represent a target system, but the intrinsic nature of the model system does not depend on whether or not this is the case; model systems are objects of sorts and as such can be studied in themselves” (252). The way Klein and Friedman constructed their model systems is, however,

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<sup>201</sup> Strikingly enough, these methodological approaches would place Friedman in a closer position to John Maynard Keynes, while they would distance Klein from the English economist. See Lawson and Pesaran (1985), and Carabelli (1988) for a detailed discussion of Keynes’s methodology.

<sup>202</sup> Francisco Louçã (2007) calls this period, in fact, *The Years of High Econometrics*.

<sup>203</sup> For a more detailed account of Friedman’s debates with the members of the Cowles Commission see Boumans (2013), DeVroey (2009a; 2009b), and Epstein (1987).

<sup>204</sup> My argument goes well in line with Mary Morgan’s (2012, 38) thesis that “[m]odeling is not an easy way to find truths about the economy, but rather a practical form of reasoning for economists, a method of exploration, of enquiry, into both their ideas and the world.”

<sup>205</sup> I prefer “model systems” rather than “engines,” because “systems” refer to a more complex kind of object that seems to be more flexible, conceding for the possibility of evolution and adaption. “Systems” might be more “organic.” “Engines,” on the contrary, seem to be too close to mechanics, giving the impression of being rather inflexible, producing more or less the same output repeatedly.

very different. The difference relies not only on the purpose that the authors assign to their constructed systems (economic planning or mere understanding of the economy), but also on the actual target that the system is supposed to represent and explain (the whole economy or a fraction of it, like a particular market).

Contrary to Friedman's opinion, I argue that even if the goal of Klein's models was to provide a representation as close as possible of economic reality, Klein's modeling does not yield a naive "photographic description of reality." In fact, it produces a whole new scientific practice that considers the model system as an ever-evolving object. The system's evolution does, of course, not occur by itself, as it could arguably be the case for a biological organism. Rather, the assemblage of objects, practices and people around the system are the driving force of its evolution. Additionally, Klein thought that his model system should attain as high a degree of complexity as possible, since the model system would perform as a tool to understand and to act on the target system.<sup>206</sup>

Friedman's model system is different from Klein's in that the target system he wants to illuminate is less ambitious. Indeed, the ultimate goal of Friedman's model system is to understand the economy, not to act on it; this understanding of the economy can (and should) only happen through the exploration of a small part of it. This approach yields a model system allowing for the understanding of the most important fundamental relations that the author can subsequently extrapolate to the rest of the economy, without any ambition of representation of the whole system, but solely as an instrument to grasp reality.<sup>207</sup> In other words, Klein sees the model system not only as a complex tool for understanding the world and for acting on it, but also as an important (and necessary) element to persuade policy makers and other economists of his own policy recommendations. Friedman sees his model system as

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<sup>206</sup> To Klein, macroeconomic modeling was a powerful, scientific, and pluralistic tool for social planning (see chapter 4).

<sup>207</sup> Friedman's (1944; 1949; 1953) emphasis is on the capacity of the system to predict.

a simple tool considering only a modest target system from which to observe the behavior of variables. This partial observation of the modest target system should illuminate the researcher’s understanding of the behavior of relevant variables, depicting the underlying mechanisms and interrelations of the whole economy through a system of thought, or an engine, even if it does not provide a direct representation of the *whole* system itself.

The purpose of this chapter is to compare both Klein’s and Friedman’s methodological positions and to give an historical account of the controversy between these two authors (and the approaches they represent). This longstanding debate shows at least one clear thing; the fact that economics became a “tooled” discipline during these years completely changed the relations between the spheres of what Walras (1954) [1874] called “pure economic theory,” “applied economics” and “social economics.”<sup>208</sup> I will argue that rather than just “bridging the gap between theory and data” (Spanos 2014) the introduction of econometrics radically transformed the preeminence of theory over application, data and political issues in economics. Independently from the economist himself and from his purpose, the macroeconomic practice of the twentieth century (which implies adherence to the econometric tool) does not allow for a dissociation of theory, application and policy, but instead combines and fuses them into a single model system: macroeconometric modeling (whether structural or not).

## 5.2. The relevance of empirical work for the development and testing of theories, and the inapplicability of highly formalized systems

The debate between Friedman and the Cowles Commission occurred mostly during the time the headquarters of Cowles were based in Chicago, between 1939 and 1955. These years also coincide with Friedman’s most active period in his writing on economic methodology (Boumans 2011). Most importantly, however, is to situate this debate within a period where (at least) two programs of empirical research were confronted, each of which would claim to

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<sup>208</sup> Mary Morgan (2003) would prefer to refer to economics as a “tooled-based discipline” rather than as a “tooled” discipline.



provide the best empirical approach to economics (*ibid.*). This confrontation took particularly place between (1) the “statistical economics” approach stemming from the tradition of the National Bureau of Economic Research (NBER) and particularly from Wesley Clair Mitchell, and (2) the “econometrics program” inspired, among others, by the works of Ragnar Frisch, Jan Tinbergen, and the Cowles Commission.

Even if economic theory played an important role in the approach of the statistical economists of the NBER like Mitchell, their emphasis was rather on the observation of “facts.”<sup>209</sup> Taking into account the problem of “multiple hypotheses” for the particular case of the business cycles, Mitchell (1913, 19-20) considered that there was a “better prospect of rendering service if we attack” directly the task of observing, analyzing and systematizing the phenomena of prosperity, crisis, and depression, instead of taking the “round about way of considering the phenomena with reference to the theories.” On the contrary, the econometric approach led by Frisch, Tinbergen and the Cowles Commission emphasized on the predominance of theory (both economic and statistical) over the observation of “facts.” In a word, the empirical and observational phase would come only after the establishment of theory.<sup>210</sup>

The relation between Friedman and the Cowles Commission is an old and longstanding one in which a certain amount of mutual (academic) respect was a common denominator. Yet, the relationship between Friedman and Cowles has been, for the most part, one of conflict and disagreement, and therefore one of abundant fertility.<sup>211</sup> One cannot forget, on the one hand,

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<sup>209</sup> For a more detailed account of the statistical economics tradition see Morgan (1990, chapter 2.2) and Hammond (1996, chapter 1).

<sup>210</sup> It is difficult to provide a clear-cut definition and differentiation between these two approaches that makes justice to all the authors, since the real differences is a matter of emphasis between the relative importance that either economic statisticians or econometricians attributed to the theory or to the observation of facts. The differentiation of the two approaches becomes even more complicated when one thinks about “theory” in two different ways: economic and statistical theory.

<sup>211</sup> It is worth noting, for instance, that Friedman was nominated to be part of the Cowles Commission. In September 1942, when Th Theodore O. Yntema (research director of the Cowles from September 1939 to December 1942) resigned to the Cowles's research directorship, the economics department presented

the problematic relationship that existed between the Department of Economics of Chicago University and the Cowles Commission since its installment in Chicago, and, on the other hand, the difficult relationships between Cowles and the NBER.<sup>212</sup> One cannot forget either that Friedman was an emblematic figure in both the Economics Department and the NBER. In the same vein as Clifford Hildreth’s (1985) account, I like to see this longstanding relation as one in which “each group benefited quite a lot from the presence and activities of the other.” Although “extensive collaboration did not develop [...] there was extensive cross attendance at seminars” and “senior people in both groups [were] quite anxious to furnish constructive leads when approached with problems in their domains” (5).

One of the first encounters between Friedman and one of the members of the Cowles happened at the beginning of the 1940s after the publication of Oskar Lange’s (1944) *Price Flexibility and Employment*.<sup>213</sup> Lange’s goal “was to examine the Keynesian issue of whether a decrease in the money wage could restore full employment in the face of involuntary unemployment” (DeVroey 2004, 3). In 1946, Friedman seemed to be very concerned about the existence of a multiplicity of theories explaining economic phenomena – or as the econometricians put it, about the problem of “multiple hypotheses,” which concerns the specification of a model and so the problem of “model selection” (see chapter 4). Friedman was also concerned about the problem of “identification” of a theory, “whether statistical estimation could lead to the desired relationship derived from non-mathematical economic theory, and

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Friedman’s nomination (Bjerkholt 2015, 22). Besides Jacob Marschak and Paul Samuelson nominated by Yntema himself for the position of research director, the names of Milton Friedman, Allen Wallis, Arthur Burns and Abraham Wald were suggested by the department of economics (23). However, Friedman never made it to the short list of candidates, which was composed of Marschak, Gottfried Haberler, Arthur Burns and George W. Terborgh. In the end, Marschak was appointed research director starting on January 1, 1943.

<sup>212</sup> For a description of the relations between the Department of Economics and the Cowles see Mirowski and van Horn (2009). The complicated relations between the NBER and the Cowles are explained in Mirowski (2002).

<sup>213</sup> It is worth noting that Oskar Lange was a researcher at the Cowles Commission from 1938 to 1945, although he was absent between 1942 and 1944, since he went to Columbia University as a visiting professor (see Boumans 2013).

whether statistical estimation could help discover true economic relationships” (Qin 1993, 96). Both the specification and the identification problems are important since, they represent two of the stages where the econometrician needs to make proof of his economic intuition. In other words, the identification stage is when the pure statistical side of econometrics takes a secondary role and the model gets confronted with economic theory.

Friedman's criticism of Lange's approach, was that Lange's highly abstract and mathematical methods led him to forget about the real world, hindering him from being able to give “form and content” to his “abstract functions.” According to Friedman, Lange used only “casual observation” to evaluate the relevance of the proposed functions, producing systems with an infinite number of possible specifications obtainable just through the permutations and combinations of the equations. To Friedman (1946, 618), Lange used theory “as a taxonomic device,”

[starting] with a number of abstract functions whose relevance – though not their form or content – is suggested by casual observation of the world-excess demand functions (the orthodox demand schedule minus the orthodox supply schedule) for goods and money, the variables including present and future (expected) prices. He [Lange] then largely leaves the real world and, in effect, seeks to enumerate all possible economic systems to which these functions could give rise. The kind of economic system and the results in that system will depend on the specific character of the functions and their interrelations, and there clearly are a very large number of permutations and combinations.

Friedman criticized Lange for focusing too much on the formal structure and on the logical interrelations of the parts, considering “unnecessary to test the validity of his theoretical structure except for conformity with the canons of logical analysis, [and] not empirical application or test” (619). Lange reached conclusions that no observed facts could contradict,

providing formal models of imaginary worlds rather than generalizations about the real world. In a nutshell, “the resulting system of formal models has no solid basis in observed facts and yields few if any conclusions susceptible of empirical contradiction” (619). This emphasis and “inappropriate” formalizing of theories by the Cowles’s approach proved the irrelevance of their models for policy advice. If the researcher using this abstract approach wanted to give some policy advice or understand the world, he was obliged “to escape from the shackles of formalism” and to abandon the (highly abstract) theory, being confined to commit disastrous errors of logic. That kind of theory or modeling worthless in Friedman’s opinion (1946, 631):

A man who has a burning interest in pressing issues of public policy, who has a strong desire to learn how the economic system really works in order that that knowledge may be used, is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks. He will escape the shackles of formalism, even if he has to resort to illogical devices and specious reasoning to do so.

According to Friedman “[a] far better way [to escape the shackles of formalism] is to try to devise theoretical generalizations to fit as full and comprehensive a set of related facts about the real world as it is possible to get” (631). Friedman’s criticism of Lange’s work contains two major points that will be brought up throughout his debates with the Cowles Commission. First, the exaggerated focus on internal logical rigor and on the formalization of the system; second, the neglect of the role that empirical observation should play both in the construction of a theory or model and in its testing procedure, rather than as a mere indicator of the relevance of including a particular variable or not in the model (as Lange allegedly did).

### 5.3. The Cowles Commission and the construction of a tool for economic planning

The debate between Friedman and the Cowles Commission evolved as well, mainly because the “scientific objects” and the approaches both sides produced were in constant evolution. Lange’s theory was, in fact, not completely in line with the approach adopted in the mid and late 1940s at Cowles, when empirical work enjoyed of a short but fruitful push. Since Jacob Marschak’s appointment as research director in January 1, 1943, the Commission had set a clear new goal: to advice firms and government agencies, or as Marschak himself put it, to perform “social engineering” (Epstein 1987, 61).<sup>214</sup> <sup>215</sup> To Marschak, the major problem in economics was that there was no economic theory accounting for a complete and causal explanation of macroeconomic phenomena.<sup>216</sup>

Furthermore, in the case of business cycles, there was a plethora of theories and pseudo-theories pretending to provide some accurate explanations of each phenomenon. Yet economists were not equipped with the necessary tools to distinguishing between “good” and “bad” theories:<sup>217</sup>

Any specification of *the* theory would, at present, mean merely setting one’s mind on preconceived ideas affected by emotional preference, as in the case of the role of wage rigidity, monopolies, income distribution, and public spending (Marschak to Robert Redfield, February 1944, quoted by Epstein 1987, 65)<sup>218</sup>

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<sup>214</sup> For a more detailed account of Marschak’s life and career see Hagemann (1997; 2011).

<sup>215</sup> The term “social engineering” was rapidly “toned down to economic policy, [however,] probably to avoid connotations of ‘central planning’” (Epstein 1987, 61-62).

<sup>216</sup> This plethora in economic ideas, however, might be attributable to a certain image of the economic discipline of the time, characterized by a high degree of pluralism. As Morgan and Rutherford (1998, 4) put it: “It was genuine pluralism, to be taken in a positive sense. Pluralism meant variety, and that variety was evident in beliefs, in ideology, in methods, and in policy advice [...] Economists felt at liberty to pursue their own individual combinations of ideas. Pluralism [...] describes not only the difference between individuals; pluralism was in each economist.”

<sup>217</sup> See Morgan (1990) in particular Part I for an account of the situation with business cycle theories in the 1920s and 1930s. See also Haberler (1937) for a contemporary account of this plethora of theories.

<sup>218</sup> To Klein too, “it [was] desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgments of a particular investigator.

In other words, the “problem of multiple hypotheses” and of model selection was a major and urgent issue for economists to resolve, since it represented an obstacle for the main purpose set by Marschak: to make sound economic policy recommendations. Economists were in need of a tool that allowed them to choose between theories. The first goal of the Cowles Commission during the 1940s was, therefore, to provide economists with that particular tool.

The theoretical construction of the method needed the active participation of highly skilled groups of economists and statisticians. By 1944 with the publication of Haavelmo’s “The Probability Approach in Econometrics,” the purpose of Cowles was, if not reached, at least on good track, allowing for optimism about the possible future applications and results of the program. By 1949, however, actual application and results were still expected by the community. Although the Cowles’s methods seemed to fulfill an important gap, they were rather confusing for economists in general (Wilson 1946, 173), and their usefulness was still to be demonstrated. As Vining (1949, 77) put it,

While these [Cowles’s] methods are intriguing and the results of their application will be awaited with keen interest, they are as yet untested. Acceptance of them as the only, or the best, method for reaching economic truth must hinge on results, not on any advance statement, no matter how persuasive, of their potential merits. Until such evidence is available, they must be considered an exceedingly narrow class of methods, and an insistent appeal to use them, and them alone, as an invitation to put a strait jacket on economic research.

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Econometric models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims” (Klein 1947, 111).

#### 5.4. The NBER Conference on Business Cycles and the conception of “naïve models”

With the arrival of Marschak to the research directorship of the Cowles Commission, a research seminar was established, which was initially held every three or four weeks but which was soon organized on a bi-weekly schedule (Hildreth 1985, 6; Boumans 2011, 3). Not only the members of the Cowles Commission were regular attendants to this seminar, however. There were also numerous researchers coming from other institutions that presented their own work at the seminar.<sup>219</sup> Eminent economists like Jan Tinbergen, John von Neumann, Richard Stone, John R. Hicks, Ragnar Frisch, Karl Menger, Harold Hotelling, John Nash, or Collin Clark, among many others, did participate in that seminar. The most active presenters and participants at the seminar were, of course, the members of the Commission themselves, and then those of the University of Chicago (mainly, but not exclusively, from the Economics Department). Jacob Viner, Donald M. Fort, Martin Bronfenbrenner, Rudolph Carnap, James Savage, Louis Thurstone, and later Earl Hamilton and Gary S. Becker, among many others, also participated at the Commission's seminar.

Milton Friedman, not only presented papers in the Cowles's seminar too, but was, apparently, one of the most active and assiduous participants. It was precisely in attending the Cowles's seminars that Friedman came to develop his idea of naive models (Epstein 1987, 109).<sup>220</sup> The aim of Friedman was to establish a standard to compare the predictive performance of a structural macroeconomic model, as that provided by Klein, with the

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<sup>219</sup> A complete list of the papers presented in the seminar from 1943 to 1955 can be found on the website of the Cowles Foundation under the heading “Commission Seminars”: <http://cowles.yale.edu/commission-seminars>.

<sup>220</sup> In this seminar, Friedman presented a paper “Utility Analysis of Gambling and Insurance” on October 23, 1947, and two papers in 1952, the first on January 10 “Price, Income, and Monetary Changes in Three Wartime Periods” and the second on November 20 “The Effect of Individual Choice on the Income Distribution” (Cowles Commission, 1947; 1955). Most importantly, Friedman's idea of the “naïve models” might have appeared when criticizing the Cowles's approach.

predictive performance of a very simple “naive model.”<sup>221</sup> Friedman’s emphasis on testing the predictive performance of the macroeconomic models pointed out a very sensible issue, casting doubt, indeed, on the results obtained by the Cowles Commission and especially by Klein’s models.

According to Klein (1951c, 1), his

main objective [was] to construct a model that [would] predict, in the [broader] sense of the term. At the national level, this means that practical policies aimed at controlling inflationary or deflationary gaps will be served. A good model should be one that [would] eventually enable us to forecast, within five percent error margins roughly eighty percent of the time, such things as national production, employment, the price level, the wage level, and the distribution of the major shares of national income – wages, industrial profits, and agricultural income.<sup>222</sup>

Even though the first attempts in macroeconomic modeling obtained some important results, both Klein and the other members of Cowles had serious reservations about the validity of these results (Marschak 1946 quoted by Epstein 1987, 105; Klein 1991). It was, of course, a first attempt in which a lot of effort had been invested, but much work had still to be done. Klein, however, remained optimistic long after the Cowles’s “retreat” from the macroeconomic modeling project (see Epstein 1987; Bjerkholt 2014a), and continued pursuing his life-long goal of with conviction. Yet, Klein had still to face important criticisms on his modeling approach.

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<sup>221</sup> Roy Epstein (1987, 109) claims that naïve models were developed “to compete with the structural models and [that Friedman] even claimed a structural interpretation for it.” However, as I will show later, the naïve models were conceived from the beginning just as a way of assessing the predictive performance of macroeconomic models.

<sup>222</sup> “Related objectives [of Klein’s modeling project were] the testing of alternative business cycles and the description of history.” (*ibid.*).



One of these criticisms came from inside Cowles itself, and it happened once Klein had left the Commission.<sup>223</sup> Carl F. Christ, whose background was in physics, entered the Commission in 1947 as a Social Science Research Council (SSRC) Fellow (Bjerkholt 2014b, 779), with the explicit task of revising Klein's models. Christ claimed that his "problem was to choose the 'best' [structure]" that would yield "the most accurate predictions of the future" (Christ 1949, 3). Following Friedman's suggestions – as well as Andrew Marshall's<sup>224</sup> (1949) – Christ (23) added that:

In order to be completely happy with a model, we would like to know that it meets one additional qualification: its errors of prediction, i.e. its calculated reduced forms disturbances  $v^*$ , should be no larger, on the average over a number of years if not in every year, than the errors made by the same naïve noneconomic hypothesis such as 'next year's value of any variable is equal to this year's value plus a random disturbance.'

Christ argued that "if this condition is not met, then we will want to use the naïve model (as [Andrew] Marshall calls it) instead of our complicated econometrics, or at least to revise our econometric analysis at certain points." Even if his tone seemed that of a discouraged and unconvinced researcher (at least compared to Klein's optimistic tone) he made another statement to defend the econometric approach: "even if such a naïve model does predict about as well as our econometric model, our model is still preferable because it can predict consequences of alternative policy measures and of other exogenous changes, while the naïve

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<sup>223</sup> Klein left the Cowles Commission and Chicago in the middle of June 1947 for a fruitful sojourn in Europe, where he met among others with Ragnar Frisch, Trygve Haavelmo and Jan Tinbergen. See chapter 2 and Bjerkholt (2014b).

<sup>224</sup> Andrew Marshall was a student in Chicago whose Masters' thesis consisted on the testing of Klein's model. It was Marshall who coined the name of "naïve models" to refer to the kind of models proposed by Friedman to test the accuracy in prediction of macroeconomic models.

model cannot” (23). Christ used two naïve models to test the accuracy of prediction of Klein’s models:

Naive Model I:  $y_t = y_{t-1} + \varepsilon_t$

Naive Model II:  $y_t = y_{t-1} + (y_{t-1} - y_{t-2}) + \varepsilon_t$

In November 1949, the NBER organized a *Conference on Business Cycles*, where Christ’s results were presented. Both Friedman and Klein commented on these results. Klein (1951a, 114-115) was not particularly pleased with Christ’s conclusions, reacting forcefully and rejecting “any personal responsibility” for this work. His reaction was based on three counterattacks:

Carl Christ has presented a splendid methodological account of a procedure for testing the validity of econometric models, *but like many other econometric contributions of recent years it is weak in empirical or substantive content*. I shall argue that his time series data contain an obvious gross error, that he has not chosen a desirable postwar revision of my prewar econometric model, and that his forecasting technique is both wrong and inefficient. Let me make matters quite clear at the outset, I do not accept any personal responsibility for anything that Christ has done. I participated to a negligible extent in his work. (My emphasis).

Klein was willing to accord an important part of the modeling activity to the empirical content of the model, recognizing that econometric work had hitherto been poor in terms of empirical results. In this sense, Christ’s results were poor, because he had made important errors in the empirical content in the revision of Klein’s model.

During the late 1940s, the U.S. Department of Commerce had undertaken an important revision of the national accounts (Klein 1951, 115).<sup>225</sup> Klein claimed that the “most serious deficiency in Christ’s work is in the data he used for 1946-47 to revise [his] model and bring it up to date. These are critical observations since they provide the basis for revisions and in samples of 20-25 annual observations can play an important statistical role. In addition, these data enter as lags in the forecasting for 1948” (115). To Klein (1951, 116), Christ’s results could not be accepted as a standard to judge the accuracy in prediction of macroeconomic models, since Christ’s use of the method was not efficient:

If we want to make a sound judgment about the use of econometric models for predicting some of the main economic magnitudes, we ought to reserve opinion until the most efficient use of the technique with available information has been tested. To forecast in the social sciences is difficult, and it is not likely that we shall get useful results with an inefficient application of any method. Christ’s paper represents an inefficient application in many respects, and on the matter of data alone there are numerous things that he must do before he can draw any conclusions. The only really satisfactory approach open to him in the interests of efficiency is to revise all his series to agree with the new data of the Department of Commerce.

Not only did Klein not accept Christ’s results and way of using the econometric approach; he also rejected Friedman’s comments about Christ’s paper. In his comments on Christ’s revision, Friedman (1951, 107) claimed that “[t]he fact that the results suggest that Klein’s

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<sup>225</sup> This would not be the last time that the data revisions of the U.S. Department of Commerce obliged Klein to undertake major revisions of his own models. This was also the case of Klein and Goldberger’s (1955) model. This kind of data revisions were not those of the routinary type made by these institutions to get more accurate figures; these were revisions that changed the basic concepts to be measured and the definitions of national accounting identities. On the history of this change in the production of national accounts in the US see Carson (1975) and Duncan and Shelton (1978). For the contemporary debate that this change produced see Gilbert, Jaszi, Denison, and Schwartz (1948), and Kuznets (1948).

experiment was unsuccessful is in some ways less important than the example they set the rest of us to go and do likewise. After all, most experiments are destined to be unsuccessful; the tragic thing is that in economics we so seldom find out that they are.” In a word, after Christ’s results Friedman suggested that the whole econometric program *à la* Klein should be abandoned. But, this claim was unacceptable for Klein, since “Christ ha[d] not shown that econometric models break down as forecasting devices” (Klein 1951, 117) and, to Klein, it was only Christ’s revision that had proved a poor application of the econometric method.

Friedman had accused large-scale macroeconometric models of being able to fit only the data from which they had been derived. To him this was “a test primarily of the skill and patience of the analyst; it [was] not a test of the validity of the equations for any broader body of data.” He continued saying that “such a test [of the validity of the equations] is provided solely by the consistency of the equations with data not used in their derivation, such as data for periods subsequent to the period analyzed” (Friedman 1951, 108). But Klein (1951, 117), again, claimed that even if this might be the case of Christ’s models, it was not the case of his own models:

The only things for which I [Klein] assume any responsibility are the construction of the prewar model and the forecasts, from it, for 1946 and 1947. My extrapolation to 1946 (*Econometrica*, April 1947, 134) estimated net national product in 1934 prices to be \$121.6 billion. Christ’s figure for the observed value is \$115.2 billion. In terms of the customary accuracy involved in economic forecasts, this is not a bad correspondence. It is certainly in the right direction for the postwar situation. My forecast for fiscal 1947 (*ibid.*, 133) was \$104.5 billion. Christ’s figure for calendar 1947 is \$103.3 billion, showing that my fiscal year forecast of real output was undoubtedly near the observed value. Since both my forecasts were made before the events occurred they had to use estimates of the relevant predetermined

variables. Some of the estimates were not correct, but that, of course, is the case in any realistic forecasting situation.

In any case, the introduction of naive models was the only way of assuring strong results of prediction for Friedman. He described these naive models not as “techniques for actually making predictions” or “competing theories of short-time change,” but rather as a way to “provide a standard of comparison, to set the zero point, as it were, on the yardstick of comparison” (Friedman 1951, 109). If the “appropriate test of the validity of a hypothesis is the adequacy with which it predicts data not used in deriving it [...] how shall we assess the adequacy of prediction? Obviously, we need not require perfect prediction; so the question is when are the errors sufficiently small to regard the predictions as unsuccessful?” The purpose of the naive models was then to provide this “standard of comparison” without which the researcher would not know how big is big, or, in this case of errors, how small is small.

In the end, Friedman's criticisms of large-scale macroeconomic modeling pointed out an important methodological problem. According to Friedman: however high the degree of complexity the econometrician could accomplish in his model in terms of specifying the most “complete” economic theory; however large the number of variables and relationships; however, sophisticated the mathematical forms of the equations and of the methods of estimation, no econometric model would ever be able to get rid of the arbitrariness that this sort of complex approaches to economics carried with them. Since there was – and probably there would never be – any theory able of representing reality in an accurate way, the most important efforts in this direction would look pretty skinny compared to the real world. In Friedman's (1951, 112) words:

we know so little about the dynamic mechanisms at work that there is enormous arbitrariness in any [economic] system set down. Limitations of resources – mental, computational and statistical – enforce a model [of simultaneous equations] that,

although complicated for our capacities, is yet enormously simple relative to the present state of understanding of the world we seek to explain.

To Friedman, the only way of getting rid of this arbitrariness was by changing this complex empirical approach of the Cowles Commission (allegedly inspired by Walras), and by embracing an empirical approach based in his own Marshallian view. This Marshallian methodology consisted on the illumination of a specific part of the economy through careful observation of it, allowing the economist for the construction of an “engine” or model system. The study of this model system, constructed from a particular part of the economy, could, little by little, reveal some of the most fundamental mechanisms of the whole economy. In the next chapter, I come back to this issue and provide a more detailed account of Friedman’s US-Marshallian approach, contrasting it with Klein’s US-Walrasian approach.

### 5.5. Concluding Remarks

The debate between Klein and Friedman demonstrates that empirical work was the key issue marking the development of macroeconometrics during the 1940s and the 1950s. In fact, the relation between Friedman and the members of the Cowles Commission enriched the discussions of the economists of the time, raising questions on methodology, methods, evaluation of results, and practices.

In terms of methodology, the modeling strategies developed by these alternative approaches discussed whether the macroeconometrician should consider the economic system as a whole to be able to understand the mechanisms beneath the behavior of economic variables and agents (Walrasian approach); or whether the macroeconoemtrician should consider only particular (and smaller) parts of the economy that he could observe and understand better, and from which he could then build an “engine” or a “model system,” allowing him to infer some of the most relevant underlying mechanisms, and to extrapolate them to the rest of the system (Marshallian approach).

The use of methods and the way to evaluate the performance of models was also an intensely debated issue. In concrete terms, these debates and interactions yielded the important “naive models,” which became of popular and common use in econometric practice. Friedman proposed these naive models not as an attempt to seriously compete with large-scale macroeconomic models, but rather as a way to establish a standard of comparison. Using these models, macroeconometricians had a reference to compare whether the results that the large-scale model produced in terms of prediction were accurate enough. I will come back to these discussions in the next two chapters.

Finally, both empirical approaches to macroeconomics yielded two different scientific practices, two ways of doing macroeconomics, which created two whole new “engines” and systems of thought, allowing for thinking about the economy, for understanding it, and for acting on it in alternative ways. To both, Friedman and Klein, it was clear that the model itself, i.e. the system of equations or the single economic equations, were only one component of the system of thought; the rest of the components were constructed around an institution, and were made up by the interactions that a team of experts, data, and a priori information (economic and statistical theory, institutional, political and historical knowledge) had with the model. I will go into more detail about all these elements in the following two chapters.







# Two empirical approaches to macroeconomics: the Walras-Marshall divide in the United States

Mathematical sciences [...] go beyond experience as soon as they have drawn their type concepts from it. From real-type concepts, these sciences abstract ideal-type concepts from which they define, and then on the basis of these definitions they construct a priori the whole framework of their theorems and proofs. After that they go back to experience not to confirm but to apply their conclusions. Following the same procedure, the pure theory of economics ought to take over from experience certain type concepts [...] From these real-type concepts [it] should then abstract and define ideal-type concepts in terms of which it carries on its reasoning. The return to reality should not take place until the science is completed and then only with a view to practical applications.

*Léon Walras, 1874*

Facts by themselves are silent. Observation discovers nothing directly of the action of causes, but only of sequences in time [...] The economist [...] must not be content with mere facts [and] must be suspicious of any direct light that the past is said to throw on problems of the present. He must stand fast by the more laborious plan of interrogating facts in order to learn the manner of action of causes singly and in combination, applying this knowledge to build up the organon of economic theory, and then making use of the aid of the organon in dealing with the economic side of social problems. He will thus work in the light of facts, but the light will not be thrown directly, it will be reflected and concentrated by science.

*Alfred Marshall, 1885*

En réalité, le système est un ensemble dont toutes les parties se tiennent et réagissent les unes sur les autres [...] Il semble donc que dans la solution complète et rigoureuse des problèmes relatifs à quelques parties du système économique, on ne puisse se dispenser d'embrasser le système tout entier. Or ceci surpasserait les forces de l'analyse mathématique et de nos méthodes de calcul, quand même toutes les valeurs des constantes pourraient être numériquement assignées. L'objet [...] est de montrer jusqu'à quel point on peut [...] éluder cette difficulté, et faire encore avec le secours de signes mathématiques une analyse utile des questions les plus générales.

*Antoine-Augustin Cournot, 1838*

### 6.1. Introduction

The Walras-Marshall divide is a central issue in twentieth century economics. The common idea that economists retain of this divide is that the differences between Walras and Marshall are just a matter of general *versus* partial equilibrium, but that both visions are complementary.

Yet, the Walras-Marshall divide has more profound methodological consequences, which make these approaches true alternative ways of doing macroeconomic modeling. To be sure, I consider the Walras-Marshall divide important for the history of macroeconomics, because it depicts decisive differences in the modeling strategies adopted by the leading macroeconometricians of the time: Klein and Friedman. Again, adopting a Walrasian or a Marshallian methodological stand in postwar macroeconomics has not much to do with visions on partial or general equilibrium; rather, it has to do with the way in which the macroeconometrician actually models the economy, and with the use he wants to give to his model. In fact, complementarity of the Walrasian and Marshallian approaches is not at stake here. Instead, two different strategies to build model systems is what results from this divide.<sup>226</sup> In this chapter, I study the Walras-Marshall divide not as an abstract discussion that happened in the minds and blackboards of the economists, but as a material and concrete discussion that happened in the context of both the longstanding relationship between Friedman and Klein, and the actual practice of macroeconomic modeling.

Friedman (1949; 1955) was one of the first economists who made an enriching distinction between Marshallian and Walrasian methodology, going further than the standard distinction between partial and general equilibrium. To illustrate his point, Friedman (1949) used a metaphor of engines and photography in which he claimed that the Marshallian modeling approach sought to construct an “engine for the discovery on concrete truth,” which allowed to introduce “systematic and organized methods of reasoning,” whereas in the Walrasian modeling approach “theory is to be tested by the accuracy of its ‘assumptions’ as photographic descriptions of reality, not by the predictions that can be derived from it.” From the Marshallian point of view, then, economic theory “has two intermingled roles: to provide ‘systematic and

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<sup>226</sup> Note that, even if Michel De Vroey (2012; 2015) also argues against the idea of complementarity between Walras’s and Marshall’s approaches, his argument is different than mine. De Vroey takes as his point of departure the difference between general and partial equilibrium, and not the difference in modeling strategies.

organized methods of reasoning’ about economic problems; to provide a body of substantive hypotheses, based on factual evidence, about the ‘manner of action of causes.’” From the Walrasian point of view, however, “abstractness, generality, and mathematical elegance have in some measure become ends in themselves, criteria by which to judge economic theory.” In this sense, according to Friedman, in the Walrasian approach “facts are to be described, not explained” (490-491).

However illuminating Friedman’s differentiation of the Walras-Marshall divide might be, I think that his metaphor is unfortunate in its photographic sense. Indeed, when the metaphor is contrasted with the actual Walrasian modeling practice adopted by Klein, little is left from the idea that what matters to Klein is only “abstractness, generality, and mathematical elegance.” Quite on the contrary, Klein’s modeling approach proves that a particular kind of Walrasianism can be deeply rooted in heavy empirical work and observation, despite Walras himself.

Therefore, it is important to underline that these Marshallian and Walrasian views are specific to the US economists of the 1940s and 1950s. In fact, the terms “Marshallian” and “Walrasian” can be better described as “labels,” rather than as actual direct interpretations of the works of Marshall and Walras. The labels “Marshallianism” and “Walrasianism” must be situated historically and geographically. In a way, one could make a parallel between the situation of US-Keynesianism, Walrasianism and Marshallianism. Albert O. Hirschman (1988) explains that US American economists received, interpreted and then created a particular kind of Keynesianism adapted to the US context and to the US necessities, which was later re-exported to the world. I want to present both Walrasianism and Marshallianism as following a similar path of reception, reinterpretation, reconstruction (and eventually re-exportation) of a particular reading of two methodological approaches. These reinterpretations would produce almost completely new approaches, making “Walrasianism” and “Marshallianism” appear,

again, just as “labels.” In fact, there are good reasons to think that rather than following a well-defined tradition these interpretations allowed US economists to free themselves from a strict European heritage.<sup>227</sup> In any case, the protagonists of our story (Klein and Friedman) did use these Walrasian and Marshallian labels, redeeming themselves as Walrasians and Marshallians. I think that their use and vindication of these filiations is not only significant, but also enlightening to understand some of the fundamental methodological differences between these two alternative empirical approaches to macroeconomics. It is in this sense that I use these labels too: to explain the differences between these alternative approaches.

## 6.2. Friedman’s unfortunate metaphor of the “photographic description of reality”

Friedman’s (1949) metaphor of the “photographic description of reality” is not very appropriate to designate Klein’s macroeconometric approach at least for two reasons:

(1) If a theory is completely general, it cannot be photographically exact (Hoover 1988, 276). A “photographic description of reality” means that the description of the theory must be completely exact in the sense that the theory should be able to “capture” reality in an instant, characterizing every single detail of the “target system,” i.e. of economic reality. This complete exactness in description hinders the theory from being general. Indeed, a general theory is expected to describe the main tendencies or laws, hence it cannot fully account for exactness. At the same time, Friedman’s metaphor does not really match with Walras’s ideal purpose. The photographic description seems too “empirical” and it establishes a relation of direct representation between the model and reality. Walras’s concept of pure theory is not empirical, but ideal, and his purpose is not positive, but clearly normative.<sup>228</sup> Furthermore, as Hoover

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<sup>227</sup> There was, of course, at least another tradition that can be labeled as US-American: American Institutionalism. On the history of this movement see Rutherford (1996; 2001; 2011).

<sup>228</sup> I will come back to a more detailed discussion of Walras’s project. See also Lallement (2000) for a concise description Walras’s methodology and theory.

(1988, 276) puts it, “generality permits numerous possibilities; a photograph presents just one of them.”

(2) I argue that the Walrasian approach eventually created a model system or an engine to “produce knowledge.” Even if the construction of this model system, its purpose, and its use might be completely different from the model system that Friedman wanted to build on Marshallian bases, this does not mean that a system, constructed on Walrasian grounds, did not yield a “practical form of reasoning, [...] of enquiry, into both [...] ideas and the world” (Morgan 2012, 38). I will come back to this point.

### 6.3. Engines versus Cameras: Friedman’s view of the divide between Marshallian and Walrasian methodologies<sup>229</sup>

Although the separation between Walras and Marshall has been mostly understood as a divide between general and partial equilibrium, both approaches have often been considered complementary. Prominent economists such as John R. Hicks or George J. Stigler adopted this “complementary view” between the two approaches (DeVroey 2009b; 2012a; 2015). Complementarity would consist on the assignment of “the study of isolated parts of the economy to the Marshallian approach and [on the assignment of] the task of piecing these partial results together to the Walrasian approach” (2009b, 711). Yet, Friedman provides a more comprehensive explanation on the differences between Walras and Marshall, which has nothing to do with this widespread opposition, and which “separates [Friedman himself] from the new classicals” (Hoover 1988, 219). According to Friedman (1949), both Walras and Marshall viewed economic phenomena as being very complex and depending on “everything else.” Both authors, then, thought in terms of general equilibrium.<sup>230</sup>

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<sup>229</sup> Note that the sense of the metaphor of “engines and cameras” is very different from the sense used in Mackenzie’s (2008) book, where “engines” are supposed to have a performative effect on reality. The sense of “engines” in this chapter is that of “model systems” provided in the introduction, not that of theories performing reality.

<sup>230</sup> DeVroey (2004; 2009a) argues against the complementary vision. For him, as for Friedman, the approaches of Walras and Marshall are incompatible. And so, Friedman preferred the Marshallian

The distinction commonly drawn between Marshall and Walras is that Marshall dealt with ‘partial equilibrium,’ Walras with ‘general equilibrium.’ This distinction is, I [Friedman] believe, false and unimportant. Marshall and Walras alike dealt with general equilibrium; partial equilibrium analysis as usually conceived is but a special kind of general equilibrium analysis – unless, indeed, partial equilibrium analysis is taken to mean erroneous general equilibrium analysis (Friedman 1949, 490).

Friedman also quotes Marshall’s 1908 letter to John Bates Clark, in which the English author explains that “[his] whole life has been and will be given to presenting in realistic form as much as [he] can of [his] Note XXI” (Marshall 1956, 417). As noted by Friedman (1949, 490), Note XXI “presents a system of equations of general equilibrium.” In this note, Marshall “take[s] a bird’s-eye view of the problem of joint demand, composite demand, joint supply and composite supply when they all arise together, with the object of making sure that [his] abstract theory has just as many equations as it has unknowns, neither more nor less” (Marshall 1890 [1895], 808). He ends his note saying that: “however complex the problem may become, we can see that it is theoretically determinate, because the number of unknowns is always exactly equal to the number of equations which we obtain” (809). Marshall’s general understanding of the economy is a complex one where everything depends on everything else: basically, a general equilibrium framework.

“The important distinction between the conceptions of economic theory implicit in Marshall and Walras” according to Friedman, “lies in the purpose for which the theory is constructed and used. To Marshall [...] economic theory is ‘an engine for the discovery of

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approach because it would not only yield a simpler model, but it would also be more “‘useful’ than its rival for dealing with practical problems” (Yeager 1960, 54).

concrete truth.’ The ‘economic organon’ introduces ‘systematic and organized methods of reasoning’” (Friedman 1949, 490).<sup>231</sup>

In his review of William Jaffé’s (1954) translation of Walras’s *Elements of pure economics*, Friedman (1955) characterized Walras’s problem as one “[...] of form, not of [empirical] content,” and as one “of displaying an idealized picture of the economic system, not [as one] of constructing an *engine* for analyzing concrete problems” (Friedman 1955, 904, my emphasis).<sup>232</sup> Friedman always doubted that Walras could have been able to solve what Hoover (1988, chapter 9) calls Cournot’s problem and thought that “there is a fundamental, if subtle, difference between the task Cournot outlined and the task Walras accomplished.” Furthermore, Friedman thought that “failure to recognize the difference seems to [him] a primary source of methodological confusion in economics” (904). To understand Friedman’s claim about the difference between Augustin Cournot and Léon Walras, it is necessary to present *Cournot’s problem* in an explicit way (see also Hoover 1988, 218-220). Cournot (1838) describes the following methodological problem:

So far we have studied how, for each commodity by itself, the law of demand in connection with the conditions of production of that commodity, determines the price of it and regulates the incomes of its producers. We considered as given and

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<sup>231</sup> As noted in chapter 5, Friedman and Keynes found themselves in a closer position in this case. Keynes understood and assessed economic models depending on their usefulness as an *instrument of thought*. A model was not a direct representation of the whole reality, but rather a way to inquire and to act on that reality. Furthermore, according to Keynes, it was dangerous to think that an instrument like an econometric model might be turned into something rigid and general, since, in this case, the usefulness of the model as an instrument was lost. Keynes, critically referring to Tinbergen’s macroeconomic modeling illustrates this idea in the following way: “In [the] natural sciences the object of experiment is to fill in the actual values of the various quantities and factors in an equation or a formula; and the work when done is once and for all. In economics that is not the case, and to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. Tinbergen endeavours to work out the variable quantities in a particular case, or perhaps in the average of several particular cases, and he then suggests that the quantitative formula so obtained has general validity. Yet, in fact, by filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it” (Keynes CW, XIV, 300, quoted by Lawson 1985a, 129)

<sup>232</sup> See also Hoover (1988, chapter 9).



invariable the prices of other commodities and incomes of other producers; *but in reality the economic system is a whole of which all the parts are connected and react on each other* [...] *It seems, therefore, as if, for a complete and rigorous solution of the problems relative to some parts of the entire system, it were indispensable to take the entire system into consideration.* But this would surpass the powers of mathematical analysis and of our practical methods of calculation, even if the values of all the constants could be assigned numerically (Cournot, quoted by Friedman 1955, 903-904)

To Friedman, the primary source of methodological confusion in economics committed by Walras and Walrasians is that the economist must know the entire economic system in order to be able to study any particular phenomenon. No economic phenomenon, in the Walrasian view, could possibly be studied in a separate and independent way.<sup>233</sup>

One possible solution to Cournot's problem, more than a century after its statement, could be that the development of a more sophisticated mathematical analytical approach and better practical methods of calculation would allow for its overthrowing. This way of looking at the problem, however, overlooked the "fundamental methodological confusion" that Friedman claimed. Klein, for instance rejected this way of solving the problem. Instead, Klein was convinced of the fact that the economists should seek to analyze the economy taking into account all of its complexity from the beginning, and was never too optimistic about the benefits that the evolution of the mathematical power and of the calculating tools would bring about.

If econometric results are today more useful than in the past, this is only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature [...] I would expect marginal

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<sup>233</sup> However, this is not the methodological approach adopted by Cournot. The object of chapters XI and XII of Cournot's *Recherches* is to show to what extent one can elude this difficulty and provide an approximate account of the system that would allow for a useful analysis of the most general questions in economics through the use of mathematics (Cournot 1838, 146-147).

improvements of five or ten per cent through the use of more powerful methods of statistical inference [...] *The adoption of more powerful methods of mathematical statistics is no panacea* (Klein 1960, 867, my emphasis).

Great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of econometric analysis. I [Klein], personally, place more faith on the data base, economic analysis (both institutional as well as theoretical), political insight, and attention to the steady flow of information (Klein 1991, 113-114).<sup>234</sup>

Klein did not think that Cournot’s problem could be solved by means of a higher sophistication in their techniques, indirectly criticizing Friedman’s point that Walrasians sought only to improve their mathematics.

But there were also other responses to Friedman’s interpretation of the divide between Walras and Marshall. In a translator’s note, Jaffé criticized this interpretation, because Friedman did not focus on the *really* important difference between the two authors. According to Jaffé, “[a] more valid and important distinction between [Walras and Marshall] resides in the fact that [Walras] always took great care not to confuse pure theory with applied theory, while [Marshall] gloried in fusing the two” (Jaffé in Walras 1954, 542). Friedman (1955, 905) responded to this criticism by casting doubt on the “superiority” of Walras’s pure theory,

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<sup>234</sup> This was also the view of Ragnar Frisch, one of the founding fathers of econometrics: “I do not claim that the technique developed in the present chapter will, like a stone of the wise, solve all the problems of testing ‘significance’ with which the economic statistician is confronted. No statistical technique, however refined, will ever be able to do such a thing. The ultimate test of significance must consist in a network of conclusions and cross checks where theoretical economic considerations, intimate and realistic knowledge of the data and a refined statistical technique concur” (Frisch 1934, 129, quoted in Boumans 2013, 5).

deviating attention from the important issue raised by Jaffé about the “fusing” pure and applied theory. To him, Jaffé was speaking “as a true Walrasian in methodology,” who:

first constructs a pure theory, somehow on purely formal considerations without introducing any empirical content; [...] then turns to the ‘real’ world, fills in empty boxes, assigns numerical values to constants and neglects ‘second-order’ effects at this stage. As I have argued extensively elsewhere [particularly in Friedman (1946; 1953)] this seems to me a basically false view. Without denying the importance of what Jaffé and Walras call ‘pure theory’ [...] I deny that it is the whole of ‘pure theory’ (Friedman 1955, 905).

Noteworthy, is the eagerness of both Jaffé and Friedman to stick to the concept of “pure theory.” Apart from Jaffé’s comment on Marshall “fusing” pure theory and “applied science,” there is not much reflection on the effect that the empirical turn and the new econometric tools had on the traditional separation between theory and application. And yet, Jaffé’s claim about the “fusion” of pure and applied theory would hold not only for Marshall, but also for both Klein’s and Friedman’s tooled approaches. Economists in the mid-twentieth century, made part not only of an “empirical turn” in the discipline, but, most importantly, they also made part of a reconfiguration of the (hierarchical) relationship between theory, application, and policy, or between what Walras called “science, art and morals” (Walras [1874] 1954).

#### 6.4. The Cowles Commission and its particular interpretation of Walras’s work

One of the ways general equilibrium theory entered the United States in the 1930s was through Harvard University, more exactly through the “Pareto Circle” (Cot 2011). The version of general equilibrium that made its way through Harvard was not Walras’s, though. It was actually Vilfredo Pareto’s version. Pareto, who was supposed to represent Walras’s intellectual inheritance in Lausanne, thought, like many others, that the *Elements of pure economics* was

Walras’s sole important contribution. His *Trattato di sociologia generale* published in 1916, gained considerable importance in Harvard through a seminar organized by Lawrence J. Henderson, “biochemist and polymath of great note” (Merton 1985, quoted in Cot 2011, 132). According to Cot, the importance of the “Pareto Circle” is that it transformed general equilibrium into a “boundary object.” Boundary “objects [...] are plastic enough to be adaptable across multiple viewpoints, yet maintain continuity of identity” (Star 1989, 37, quoted by Cot 2011, 150). As a “boundary object,” general equilibrium travelled from one discipline to another providing the bases for the creation of an “epistemological credo,” praying that “[w]ithout a conceptual scheme, thinking seems to be impossible” (Henderson 1970, 86, quoted in Cot, 145). The conceptual scheme for any science was, of course, general equilibrium as Pareto and Henderson understood it.

Not only had Walras’s work been interpreted partially, but also the reception of general equilibrium theory had happened through Pareto’s filter. And yet, these are not, I think, the most relevant facts impeding the macroeconometricians of the 1940s to fulfill Walras’s original program. Instead, the most important event in this sense is that economists were armed with a tool, and hence with a system of thought, with which they could not possibly view either economics or the economy as Walras did.<sup>235</sup> The fact that economics became a “tooled-based” discipline (Morgan 2003; see also general introduction) determined a particular way in which economists could understand Walrasian economics and the economy. The tool did not make it possible for macroeconometricians to remain faithful to the original project of Walras, since the econometrician was not able to separate pure, applied, and social economics. The three spheres were embedded within the tool.

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<sup>235</sup> Of course, this is not the only aspect hindering the macroeconometricians to share Walras’s views. Their “scientific cultures” were completely different as well, let it only be because of the differences in the political problems they were facing, or because of the social position that economists occupied in the late XIX century France and in the mid XX century United States, among many other reasons.

The purpose of structural macroeconometrics was, again, to produce a system of simultaneous equations within a general equilibrium framework. However, in doing so through the use of a tool like structural macroeconometrics, econometricians were necessarily reconfiguring the relationship between theory, application and policy. This relationship certainly understood as a whole by Walras, but identifiable in a separate way, was fused inside the econometric model. The empirical turn that occurred in twentieth century economics generated a change in the hierarchical relation between these three spheres.

Walras's normative project was very different from Klein's pragmatic and political projects.<sup>236</sup> Economic theory or econometric models, in Klein's view, did not represent the ideal towards which society should tend, as did theory in Walras's project; they rather represented an instrument to act and to intervene the economy.<sup>237</sup> Throughout the second half of the twentieth century, the macroeconomic tool became not only the model system to understand and to act on the economy through a "scientific" approach; it became also a necessary rhetorical element that economists could not dispense of, in order to be credible both in the academic and political arenas.

### 6.5. Klein's large-scale macroeconomic model system

When it comes to assess the effect of Walras's work on twentieth century economics most attention is directed to Walras's legacy as a source of inspiration for models of the Arrow-Debreu-McKenzie-type.<sup>238</sup> This, of course, is more than understandable since it is in this field where the Walrasian approach is more evident and visible. Yet, I am interested in another sub-discipline where the Walrasian approach exerted an important influence that might seem less

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<sup>236</sup> I am associating the term policy in Klein's thinking with what would be the sphere of morals in Walras's theory although this might be quite problematic. Again, I do not seek to make a completely accurate description of Walras's project.

<sup>237</sup> Note that for Walras economic theory is also an instrument that allows for the understanding of the economic world, not to be used to act on the economy. Economic theory is also a normative reference towards which the economy should converge.

<sup>238</sup> See for instance Weibtraub (2002).

evident: macroeconometric modeling. Following Renault (2016), I argue that Walras’s influence in macroeconometrics was that of establishing an anchor and a reference framework from which to build models and understand the economy: the general equilibrium framework.<sup>239</sup> Every macroeconometrician, and especially Klein, was obliged to refer to this “pillar” as a way of understanding the economy providing a solid framework from which to construct and structure the macroeconometric model. Therefore, if the macroeconomists wanted to build macroeconometric models *à la* Klein, he had to conceive the economic system “as describable by a set of simultaneous equations expressing all the interrelationships among the measurable economic magnitudes which guide economic behavior” (Klein 1950, 2).

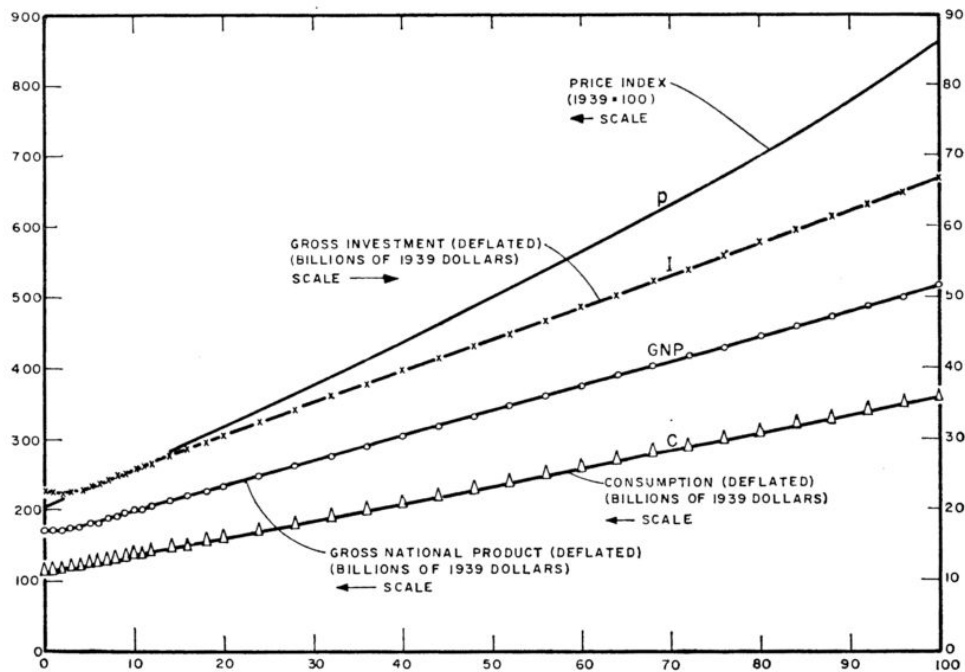
This kind of models yielded systems that, in the absence of external shocks, were essentially monotonic, stable and linear. The econometric tool, in this sense, introduced a dynamic component into the general equilibrium system, but this dynamic component remained stable. This can be seen in a clearer way through Irma and Frank Adelman (1959) examination of the Klein-Goldberger (1955) model by means of the IBM 650 high-speed calculator. The Adelmans’ examination of the dynamic properties of the model consisted in extrapolating the exogenous variables from the model and in solving the equations for a hundred years. The first stage of this examination – relevant for my purpose here – consisted in asking “what sort of time path will these equations generate in the absence of additional external constraints or shocks?” (Adelman and Adelman 1959, 601). As figure 3 shows, the “first machine data decided the issue unequivocally. After a brief ‘settling-down’ period, the system is quite monotonic and essentially linear. There is no hint whatsoever of any internally generated business cycle, and, indeed, even in the first few years, the shock of start-up is not sufficient to induce more than a

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<sup>239</sup> Renault makes a similar claim in the case of macroeconometricians of disequilibrium, especially in the case of Edmond Malinvaud.

single turning point in any variable” (602). In a nutshell, in the absence of external shocks, the system represents a system of general equilibrium.

Figure 3: Klein-Goldberger Time Paths (without any shocks).



Source: Adelman and Adelman (1959, 601).

The choice of treating the economy in general equilibrium terms, then, was not only a matter of a preference in economic theory, but also (and perhaps more importantly) a matter of technical adequacy. Klein (1950, 11) explained that:

Formerly, econometricians singled out an isolated equation of the economic system and attempted to estimate the structural parameters by the methods of least squares or some other simple method whose statistical properties were not usually satisfactory. When the earlier statisticians fitted their equations to the data by the method of least squares they seldom knew in which direction they should minimize the sum of squares; i.e., which should be the ‘dependent’ and the ‘independent’ variables. They were aware of the problem of identification, but they failed to solve it adequately. Now many of these difficulties are eliminated. If we specify both the

economic and statistical properties of the model and treat the set of equations as a unit, instead of treating each equation in isolation from the rest of the system, we are not faced with the problems that formerly were so troublesome.

Also, in a more explicit way (Klein 1957, 1-2):

Following the great ideas of Walras, we view the economic system as capable of being described by a system of simultaneous mathematical equations. In actual practice we shall, of course, drop the enormously refined detail of the Walrasian system but retain the main as picturing the economy as model written in the form of a system of mathematical equations. Our problem then will be to estimate the parameters of these equations.

Note that Klein’s “Walrasian” project is of a special kind. It mainly consists on considering the economy as a whole, discarding the individual analysis of equations. The idea of a mathematical representation is, of course, very important too.

Even Friedman (1955, 908) recognized that Walras’s method provided “a framework to organize ideas” in a logical way, allowing for a (particular) understanding of the economy.

Walras has done more than perhaps any other economist to give us a framework for organizing our ideas, a way of looking at the economic system and describing it that facilitates the avoidance of mistakes in logic.

But the organization of ideas is not something that happens only inside the mind of the researcher or econometrician. There must be a materialistic component with an explicit procedure that the econometrician should follow. An explicit example of this kind of procedures is what Bjerkholt and Dupont (2010, 34) call Frisch’s “five types of mental activity” (see figure 4). The important term is “activities,” since it is the description of a procedure or a practice, that the econometrician should follow in order to be able first to understand, then, to act on



the world. It is a series of questions that do not “naturally” come to the mind of the econometrician, but that make part of a kind of protocol the econometrician must follow to obtain his results.

Figure 4: Frisch’s five types of mental activities.

- 1. The descriptive procedure.**  
One set of questions the scientist has to answer is, What happened? What is the situation? What course did the events follow? In order to answer these questions he has to engage in descriptive, historical, and experimental work. In some sciences, such as economics, direct experiment is more or less impossible and the scientist must rely largely on the descriptive and historical answers to the questions here considered.
- 2. The understanding procedure.**  
Another set of questions that the scientist has to answer is, Why did it happen? Why did this situation exist? Why did the events follow the course they did? The answers to these questions constitute the rational part of the investigation. By the power of his mind the scientist tries to discern or impose some reasonable order onto what happened and the things he observed.
- 3. The prediction procedure.**  
The questions here are, What will happen? What will the course of events be in the future? For these sorts of questions to have a meaning, the phenomenon must be such that it cannot easily be controlled by man. If it can be fairly completely controlled, no forecasting problem really exists.
- 4. The human purpose decision.**  
Here the questions are, What do we wish shall happen? What do we wish the situation to be? The three first sorts of questions were exclusively of an intellectual character. On the contrary the sorts of questions here considered are ethical or moral. They cannot be answered unless we adopt a standard of social values. If the answer to such a question shall be socially significant, it must, of course, in some way or another weigh the opinions of the several individuals who make up the society. It is not a question of what you or I personally think in this matter, but of what is a socially fair position.
- 5. Social engineering.**  
The question here is, What can we do to produce certain outcomes or certain situations? This last sort of question is the most complicated we can ask. In order to give a significant answer to this sort of question, we have to build on an analysis of all the first four sorts of questions.

Source: Bjerkholt and Dupont (2010, 35).

In the case of Klein’s modeling, this “mental activity” or practice became even more explicit and materialized. Klein was conscious about the fact that macroeconomic modeling was something that had to be done in a team, following both a particular scheme and a specific division of labor. Specific procedures had to be respected for the whole “Kleinian model system” to work.

The whole group was broken into subgroups. There was one team working on the treatment of simultaneous equation problems. Another group worked on putting the model together, some from the point of view of economic theory and some from

*Economics as a “tooled” discipline*

the point of view of data availability. Another group worked on computing. We carved up the problem. We had very heated and intensive seminars, and everybody was extremely enthusiastic, but it was very well orchestrated (Klein 1987, 413).

“That indeed was the way we started out in the Cowles Commission, but it then became a routinized team effort. Somebody had to be responsible for the data files, someone had to be responsible for system design, and someone had to be responsible for forecasting and applications” (415). Whether it was during his time at Cowles, in Michigan, in Pennsylvania, or during his participation in the construction of the Brookings model or of Project LINK, Klein conceived econometric modeling as a team effort that established clear tasks for each of the team members, in which everyone had to follow a specific (although not necessarily explicit) procedure.

Figure 5: Cowles Commission’s seminar around 1946.



Herman Rubin, Gershon Cooper, Lawrence Klein, Jacob Marschak, Jack Hartog, and Tjalling Koopmans. Source: Cowles Commission *Economic Theory and Measurement. A Twenty Year Report, 1932-1952* (98).

Figure 6: Cowles Commission Computational Laboratory in Chicago.



Source: Cowles Commission *Economic Theory and Measurement. A Twenty Year Report, 1932-1952* (98).

This practice also needed of a specific location (almost a laboratory), of human computers first, then of specific material conditions and machines, like mechanical or electronic calculators, and later of electronic computers. It also needed of a specific method and of a particular methodology. The setting of this kind of model system produced an “engine” for understanding phenomena, but also for the development of new methods and ideas.

An interesting development in connection with the Brookings model project was that it functioned as a team effort in which each person on the team had responsibility for a certain piece of the model. Although we did not put together the definitive model we wanted, I think we learned a tremendous amount about model building from that venture. In particular, we developed best practice methods for parts of the economy. The work on the investment function was Dale Jorgenson’s and Bob Eisner’s contribution, best practice for dealing with housing was Sherman Maisel’s contribution on the relation between starts and completions

[...] We learned from the Brookings experience how to operate models, how to maintain them, and how to test them (Klein 1987, 431-432).

Another important feature in Klein’s model system was his preference for large and complex systems, opposed to systems that were simple and parsimonious. Friedman’s system sought for simplicity, “a methodological virtue [consisting on the idea that] the most significant theories explain ‘much by little’” (Caldwell 1984, 228). “Instead of the rule of parsimony, [Klein] prefer[ed] the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use” (Klein 1992, xl- xli).

#### 6.6. Friedman’s Model System

Christ (1949, 2-3) provides a general view of an “idealized procedure” to “find a system of structural equations” that was suggested to him by Friedman around 1948. The goal, of course, was to find a structure, which not only explains past observation, but also allows for making accurate predictions. “Of course there is an infinity of structures which explain any given set of observation” and so the “problem is to [...] choose the ‘best’” structure. The “best” structure is the one that provides “the most accurate predictions of the future, [but] we cannot know which one this is until afterwards. Therefore if we are to choose now we must do so on the basis of intermediate available criteria [...]:

- 1) generality
- 2) simplicity
- 3) correspondence with our theoretical ideas of what to expect (but if we have a poor theory, this criterion will mislead us)
- 4) accuracy of explanation of past observations (though we must be careful with this criterion, because it is necessary but not sufficient...remember that it is always possible to fit an  $n^{\text{th}}$  degree polynomial *exactly* to a set of  $n + 1$  plain points, and that this very seldom makes for good prediction).

In more concrete terms, however, Friedman's construction of a model system also involved a "laboratory" where several people worked together in the analysis of data, information and knowledge. This laboratory took the form of the NBER. Contrary to the Cowles Commission's projects, which were conceived rather as short-term projects, the NBER hosted long-term projects that endured for decades. This was; for instance, the case of Friedman and Kuznets's (1954) joint project *Income from Independent Professional Practice*. In fact, Kuznets had begun this investigation in 1933 and Friedman took it up in 1937, producing a "definitive" publication only in 1954.<sup>240</sup> Work at the NBER was highly influenced by Mitchell's methodology of "descriptive analysis." This means that empirical work played a major role in these investigations, which in no way was deprived of theory as Koopmans suggested in 1947 in his famous "Measurement without Theory" review. To Friedman (1952, 237), there was no dichotomy or neat separation between the "empirical scientist and the theorist." Instead, the differences between the empirical scientist and the theorist were just a matter of degree and emphasis. To him, the "ultimate goal of a science in any field is [to provide] a theory – an integrated 'explanation' of observed phenomena that can be used to make valid predictions about phenomena not yet observed." This was the task in which Mitchell and all the researchers at the NBER were involved, which goes well in line with the five 1920 original precepts of the Bureau, summarized by Fabricant (1989, 2-3):

- 1) [The NBER's] research should concentrate on determining facts, and the connections among facts, that are important in dealing with major problems of economic policy.
- 2) The knowledge sought should be quantitative in character, whenever possible.
- 3) The research should be in accordance with scientific principles.

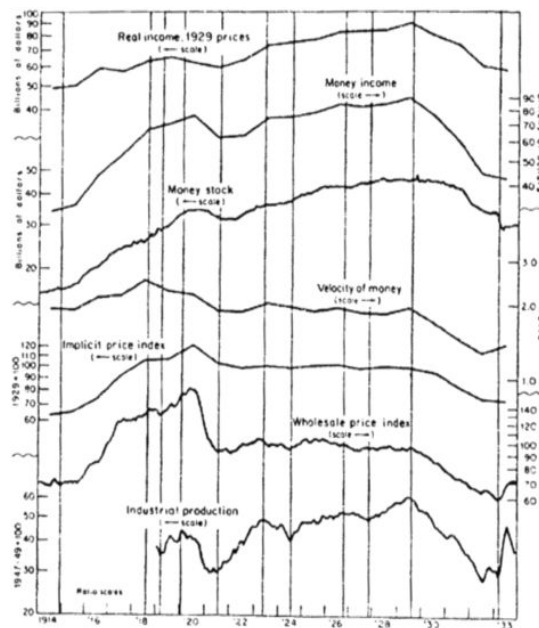
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<sup>240</sup> This was also the case of Mitchell's (1913; 1927) and Mitchell and Burns's (1946) work on the business cycle, as well as that of Friedman and Schwartz's (1963; 1965; 1970), which started in the 1920s and was completed only in 1982 (Fabricant 1989, 27).

- 4) The research should be done, and the findings made known, under auspices of and with safeguards that would assure the public of their impartiality.
- 5) To this end in particular, the Bureau should carefully abstain from making recommendations on policy.

Apart from the five precepts, the NBER analyses also included different kinds of “charts” that were used as tools of analysis to understand the cycle (see Figure 7). Work at the NBER yielded important volumes with a huge amount of descriptions of data. These descriptions were accompanied by these kinds of charts, helping the researcher and the reader to get a better picture of the movements of particular variables in the different phases of the cycle.

Figure 7: Money Stock, Income, Prices, and Velocity, in Reference Cycle Expansions and Contractions, 1914-1933



Source: Friedman and Schwartz (1965, 2).

Yet, by 1951 Friedman was rather willing to express a conciliatory position between his NBER approach and the Cowles’s econometric approach. In his concluding remarks of his comments of Christ’s paper, Friedman (1951, 114) attempted to bring these approaches to a closer position. His conclusions are worth quoting at length.

These remarks obviously have a rather direct bearing on the desultory skirmishing between what have loosely been designated the National Bureau and the Cowles Commission techniques of investigating business cycles. As in so many cases, the difference between the two approaches seems to me much greater in abstract discussions of method than it is likely to prove in actual work. The National Bureau has been laying primary emphasis on seeking to reduce the complexity of phenomena in order to lay a foundation for a theory of change; the Cowles Commission on constructing the theory of change. As the National Bureau succeeds in finding some order, some system, in the separate parts it has isolated for study its investigations will increasingly have to be concerned with combining the parts – putting together the structural equations. As the Cowles Commission finds that its general models for the economy as a whole are unsuccessful, its investigators will increasingly become concerned with studying the individual structural equations, with trying to find some order and system in component parts of the economy. Thus, I predict the actual work of the two groups of investigators will become more and more alike (Friedman 1951, 114).<sup>241</sup>

## 6.7. Concluding Remarks

Friedman's longstanding relationship with the members of the Cowles Commission was not only conflictive, but it was also fruitful in terms of methodological discussions. Both factions involved in this enduring relationship developed two empirical approaches that marked postwar economics. From the point of view of

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<sup>241</sup> Many years later, Klein (1991, 112) would describe this tension in a somewhat different way: "As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology. It was not purely methodological, however. A central issue was that we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by [...] the National Bureau."

macroeconometrics, Klein and Friedman embodied a reinterpretation of the ancient Walras-Marshall divide that ended up providing two ways of conceiving model systems characterized by their differences in their purposes, size and set up, as well as by their procedures and routinized practices. The most important point, however, is that because of the “tooled” nature of twentieth century economics, a reconfiguration occurred in the relationship between pure theory, applied theory, data and policy issues. These spheres lost their hierarchical character and were necessarily “fused” into the macroeconomic model that could no longer account for a clear distinction between them. The macroeconomic tool changed the nature of economic theory and the way economic knowledge was produced.

Friedman’s criticism of US-Walrasianism pointed to the idea that this approach was be empty mathematization of economics. Klein’s Walrasian approach, however, shows that a mathematized framework of general equilibrium, combined with statistical theory, a great amount of empirical work, and framed within a specific scientific practice, could be useful to provide a tool of reasoning for understanding and intervening the economy.

It is important to underline that Walras’s influence in twentieth century US-Economics is weaker than suggested by the label. In fact, the careful study of the practices and visions of economists, and especially of econometricians in the United States during the 1940s and 1950s, suggests the development of a particular and new approach that carries the word “Walras” only as a label. This new approach was built around an “epistemological credo” of general equilibrium, rather than around



Walras's original project, which provided a solid framework from which to think about the relations between the economic variables.

Furthermore, structural macroeconometricians were particularly bound to this credo. The macroeconomic "tool" itself allowed for the conception of the economy only as a set of simultaneous equations, always expressing the relationships between the variables in a general equilibrium framework. Econometrics did not establish only a method from which to produce a particular model system allowing for understanding and intervening the economy, but as Vining (1947) put it, econometrics also put a "strait jacket" to the possibilities of explaining phenomena. If the researcher sticks to a particular way of producing a model system in order to produce knowledge from a reasoned and logical procedure, then he also restricts himself to a number of possible explanations he can find in that particular system only. Other explanations that are not conceivable within the general equilibrium framework will then just be excluded, or they will not even be possibly conceived within the rules of this particular model system.





# Friedman and Klein on statistical illusions

Contrary to Friedman's tastes, other researchers have sought improvement in the Keynesian consumption function through the introduction of new variables. There are great limits to the extent to which one can come upon radically improved results by juggling about the same old variables in a different form. Instead of adhering to the 'rule of parsimony,' we should accept as a sound principle of scientific inquiry the trite belief that consumer economics, like most branches of our subject, deals with complicated phenomena that are not likely to be given a simple explanation [...] I venture to predict that much good work will be done in the years to come on adding new variables to the consumption function and that it will not be illusory.

*Lawrence R. Klein, 1958*

### 7.1. Introduction

In the 1957 February issue of the *Journal of Political Economy*, Milton Friedman and Gary S. Becker published their paper "A Statistical Illusion in judging Keynesian Models." In this work, their purpose was to question Keynesian macroeconometric models for their inappropriate treatment of the consumption function, and for their inability to yield accurate predictions of income. Becker and Friedman (1957) claimed that in replacing "the ultimate objective of predicting income [...] by the proximate objective of predicting consumption" (64) Keynesian modelers had fallen into a statistical illusion. According to them, the illusion resulted from the adoption of the relative error in predicting consumption as a criterion to judge the performance of macroeconometric models. In a nutshell, Becker and Friedman's (1957) criticism of the "statistical illusion" in judging Keynesian models consisted in two

points. While their first point focused on how to correctly evaluate model performance, their second point was related to the specification of the consumption function itself.

John Johnston (1958), Edwin Kuh (1958), and Lawrence R. Klein (1958) provided three separate responses in defense of the Keynesian approach. Their defenses comprised four common claims. First, the “Keynesian” consumption function used in the critique was at best a pedagogical device, and did not make justice to the functions used in actual modeling. Second, Becker and Friedman had discovered nothing new, and were raising problems already known to econometricians. Third, Becker and Friedman’s results could be easily improved through the use of alternative consumption functions. Fourth, there had been no statistical illusions in judging Keynesian models.

The controversy on statistical illusions was not an isolated event in the history of macroeconometrics, though. This controversy was embedded within a larger debate that echoed discussions from previous decades, and anticipated other discussions to come. This larger debate opposed two research programs, each of which claimed to provide the best empirical approach to economics (see Boumans 2013; Pinzón-Fuchs 2016). More specifically, this debate took place between the “statistical economics” approach stemming from the tradition of the National Bureau of Economic Research (NBER), and the “econometrics program” inspired, among others, by the works of the Cowles Commission.

As it is often the case for scientific debates, the nature of the issue underlying the controversy on statistical illusions was methodological (although technical and theoretical matters were also discussed). In this occasion, important questions were raised about the establishment of a rigorous criterion to evaluate the performance of macroeconomic models, which would improve both model specification, and predictions. Indeed, a powerful criterion to evaluate model performance would guide the macroeconometrician in his choice and adjustment of the model structure. Apart from its function as model selector, this

criterion would also act as an instrument to “shape and mold” macroeconomic models – or theory.<sup>243</sup>

Surprisingly enough, Becker and Friedman (1957) constituted an important contribution to large-scale macroeconomic modeling, anticipating the dissemination of a method to evaluate model performance: full model simulation – later routinized as computer dynamic simulation.<sup>245</sup> Through their insistence on the use of the *relative error in prediction of aggregate income* as the adequate criterion to evaluate model performance (instead of the relative error in prediction of aggregate consumption), Becker and Friedman advocated for the evaluation of structural equations to rely on *full model simulations* (and not on predictions based on single equations). According to this, in order to evaluate their models, macroeconometricians should emphasize on the simultaneous equations characteristics of structural equations (in particular on the accuracy of their predictions) rather than on their single equation characteristics (Bodkin 1995, 53-54).

Yet, by the 1950s, macroeconometricians were not necessarily convinced about the requirement of this criterion. Some considered, instead, that *out-of-sample predictions* or *extrapolations* (even when performed on single equations bases) would be more adequate to evaluate models than *sample predictions* (even if performed as full model simulations). While macroeconometricians’ idea of prediction focused on what Klein (1958) called the “painful [test] of experience,” i.e. the comparison between observed values and (*ex-ante* or *ex-post*) out-of-sample “concrete forecasting results” (543) of alternative models, full model simulation could be understood as a sort of Turing test, or as an “imitation game” (Turing 1950).

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<sup>243</sup> Here, I draw on Marcel Bouman’s (2005) notion of “mathematical moulding of economic theory,” and on Kevin D. Hoover’s (2013) consideration of “the role of statistical tests in [...] *shaping* or *molding* economic models.”

<sup>245</sup> I say “surprisingly enough,” primarily, because of Friedman and Meiselman’s (1963) later presentation of their simple reduced form equation to compare the effects of fiscal and monetary policy that prolonged and heated up the debate between Keynesians and Monetarists. But also because of Friedman’s earlier opposition *vis-à-vis* the econometrics program, dating back to the 1940s. On this last point see Boumans (2013) and Pinzón-Fuchs (2016).

The increasing availability and improvement of computational methods, as well as the expansion of large-scale macroeconomic modeling in general, provided some of the necessary conditions for the dissemination of full model simulations (see Ando and Modigliani 1969; Bodkin 1995, 54; Bodkin, Klein, and Marwah 1991, 108-ff.). The successful application of this kind of simulations was also an important push in this direction. The particular case of Irma and Frank Adelman's (1959) simulation of the Klein-Goldberger (1955) model certainly exerted an important effect on the establishment of this method as a standard criterion to evaluate model performance.

Although simultaneous equations characteristics represent only some among a whole battery of criteria, this controversy certainly constitutes an important illustration of the way macroeconometricians discussed, developed and adopted standards to judge (and improve) the performance of their models. Independently of Becker and Friedman's critical claims and clear methodological differences regarding the Keynesian modelers, a common background of general principles turned this controversy (and the larger debate) into a fruitful and revealing episode of the history of macroeconomics. In this chapter, I will give an account of the early discussions on the evaluation of the performance of macroeconomic models.

## 7.2. The Controversy's milieu: Marshallian and Walrasian approaches to macroeconomic modeling

As mentioned in the introduction, the controversy on statistical illusions was embedded within a larger methodological debate confronting two empirical approaches to economics: the statistical economics and the econometrics program. While the NBER appeared as the most visible and important stronghold of the statistical economics approach, the Cowles Commission, during its years in Chicago (1939-1955), constituted the bastion of the econometrics program.<sup>246</sup> At least since the mid-1940s, Friedman had sustained longstanding

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<sup>246</sup> During the 1950s, however, Friedman (in the case of the NBER) and Klein (in the case of the econometrics program) established themselves as the two leading figures of these approaches,

discussions with various members of Cowles, in which he had expressed his concern about the necessity of finding a sound and rigorous criterion to evaluate the model performance.<sup>247</sup> In fact, this was one of the main arguments in his 1946 debate with Oskar Lange (see Friedman 1946).

Departing from a situation of involuntary unemployment, Lange’s (1944) *Price Flexibility and Employment* investigated whether a decrease in money-wages could reestablish full employment using, according to Friedman (1946), an “unreal and artificial” (613) approach. Friedman’s claim was not so much against abstraction, but rather about the way Lange used abstraction. According to Friedman, Lange attributed too much importance to the conformity of the model structure with the canons of logic, ignoring the model’s “empirical application or test.” Lange’s use of “casual observation” (618) as his only method to evaluate his model, was not a sufficiently rigorous criterion to evaluate the relevance of the proposed functions, and resulted in systems with no “solid basis on observed facts,” yielding “few if any conclusions susceptible of empirical contradiction” (619). Friedman’s concern, again, was about Lange’s (and the Commission’s) lack of a rigorous method to evaluate model performance based on a sound empirical approach.<sup>248</sup>

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embodying a reinterpretation of the ancient Walras-Marshall divide as two alternative ways to build macroeconomic models.

<sup>247</sup> It is worth noting that, in most cases, Friedman took the role of the critic. Klein (and the Cowles’s members), instead, acted in general as defenders. This might have been the case, because large-scale macroeconomics enjoyed of a “dominant” position only from the 1950s onwards. During 1940s, however, the econometrics program was on a preliminary state, and the NBER enjoyed of a particularly good position within the economics community. Judging from the *Measurement without theory* controversy between Tjalling C. Koopmans and Rutledge Vining, critics and defenders would have switched positions at the end of the 1940s.

<sup>248</sup> Another important controversy between Friedman and the Commission took place at the occasion of the *NBER Conference on Business Cycles* in November 1949. This time, Friedman advocated, again, in favor of the establishment of a criterion to evaluate model performance. In particular, he proposed his “naïve models” as standards (or null hypotheses) to compare the predicting performance of macroeconomic models. I will come back to this subject in the third section of this chapter. For a more comprehensive account of this controversy see (Pinzón-Fuchs 2016, 10-16).



Some important methodological works published during this period resulted, partly, because of the exchanges between Friedman and the Cowles's members.<sup>249</sup> A recurrent subject in these works, notably in the case of Friedman's, related to the ancient Walras-Marshall divide. To Friedman, the important element of this divide did not consist on the classical opposition of general and partial equilibrium attributed to Léon Walras and Alfred Marshall. The ulterior motive of this divide relied on a more profound methodological question. Both Walras and Marshall understood that the economic system was fundamentally complex and that all its parts were interdependent. Thus, given this interdependency, the question was whether the economist, interested in finding concrete solutions relative to some parts of the economy, should take the *entire system* into consideration (as Walras suggested) or focus only on a few parts of it (as Marshall proposed).

Friedman's response to this question pointed clearly in Marshall's direction. His Marshallian approach was based on the idea that economic theories – or models – should be perceived as a way to construct systems of thought – or “engine[s] for the discovery of concrete truth” (Marshall 1885, 159). Since both the economist's knowledge and his capacity to observe the world were partial, these systems should be constructed through the rigorous observation of specific parts of the economy and of the “real” world, and not through elegant but empty abstractions of the entire system. In brief, these systems should provide “generalizations of the real world” instead of “formal models of imaginary worlds” (Friedman 1946, 618). This approach, however, recognized that the economy was a complex system with interdependent relations, which had nothing to do with the notion of partial equilibrium.

Klein's Walrasian approach, on the contrary, was based on the idea that the economy should be considered as a whole, despite the economist's inability to observe or understand the system in all its complexity. Independently of the economist's capacity to build a simple

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<sup>249</sup> See for instance Friedman (1946; 1949; 1953; 1955; 1958), Haavelmo (1943; 1944; 1947), Koopmans (1950; 1957), and Hood and Koopmans (1953) among other works.

(or complex) model, his point of departure to build macroeconomic models should be the entire economic system. Klein’s claim was that the combination of a mathematized framework of general equilibrium with statistical theory and with rigorous empirical work, would be useful to provide a tool of reasoning for understanding and intervening the economy.<sup>250</sup>

It is worth noting that neither Friedman nor Klein explicitly alluded to the Walras-Marshall opposition during the controversy on statistical illusions. The larger methodological debate, however, was the background and the pillar of the controversy on statistical illusions, and has to be bared in mind at all times in the study of this controversy.

To be clear, I believe that there are two different levels in this controversy. The first level, again, goes up to the larger debate (and to the opposition) between two alternative empirical approaches in macroeconomics. At this level, given the nature of Becker and Friedman’s paper, there is no doubt that the authors insisted on the abandonment of the “econometric program,” even if they did so in a somewhat subtle way.<sup>251</sup> It is at this first level, too, that Friedman would pick up his later debates with the Keynesian approach, in particular in Friedman and Meiselman (1963), and in Friedman and Schwartz (1963). Because of the scope of this chapter I will not scrutinize further in this direction, but these later Keynesian-Monetarist debates should be kept in mind in this discussion. At the second level, it seems as if Becker and Friedman had accepted to play within the rules of the “macroeconometricians’ game,” which was, by that time, the dominant approach in macroeconomics (see Bodkin et al. 1991). At this level, their critique could be qualified as constructive, for it sought to establish a criterion to evaluate model performance and to improve model specification.

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<sup>250</sup> Through his illustration of what he calls “Cournot’s Problem,” Kevin D. Hoover (1988, section 9.2) provides a clear exposition of this methodological divide between Marshall and Walras. See also Pinzón-Fuchs (2016, section II).

<sup>251</sup> “Subtle” compared to other papers where Friedman was much more radical. See, for instance Friedman’s (1951, 112) conclusion: “My own hunch [...] is that [...] attempts to proceed now to the construction of additional models along the same general lines [of the Cowles Commission] will, in due time, be judged failures,” (see also Friedman 1946; 1953).

7.3. Economics as an empirical and modeling science: a common background

In his review of the controversy on statistical illusions, Ronald G. Bodkin (1995) contended that any commentator on this discussion should “take into account [...] whether there was [...] a genuine disagreement, or whether the issues among the participants were principally semantic” (45).<sup>252</sup> I think that this contention should be reformulated in a somewhat different way. There is no doubt that there were issues of substantial disagreement between the participants, as Bodkin recognized at the end of his paper.<sup>253</sup> But more importantly, there was also a certain degree of agreement on fundamental points that allowed the participants to actually engage in a productive conversation. I claim that in order to make a contribution to the history of macroeconometrics, anyone commenting on this controversy should take these points of agreement seriously into account, for these points allowed the participants to engage in a discussion in the first place.

It is clear that for any scientific controversy to take place and for it to be fruitful, some agreement must exist between its participants. This agreement, reflected in some general, but fundamental points, can be understood as a sort of “common background.” Without this common background, fertile scientific controversies would just become semantic misunderstandings (at best) or fruitless and meaningless attacks. A brief characterization of this background is useful then, at least, for two reasons: first, it sheds light into some of the important issues at stake, making them more visible; second, it provides the possibility of

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<sup>252</sup> Judging from Becker and Friedman’s (1958a; 1958b) responses to Klein, Kuh, and Johnston alone, one possible interpretation of this controversy, would be, indeed, that the differences between the participants were semantic and not substantial. In fact, Becker and Friedman considered the Keynesian responses “less [as] a criticism [...] than a valuable supplement” of their comment (1958a, 545). To them, “if [Klein’s, Kuh’s and Johnston’s] comments nonetheless give the impression of being a criticism, it is [...] because, they interpret our note as having a different aim and different content than we intended it to have, and still think it has” (1958b, 298). The Keynesian modelers, however considered their responses neither as “supplements” of Becker and Friedman’s paper, nor as semantic misunderstandings. To them, there were substantial points to be defended.

<sup>253</sup> Bodkin (1995, 53) termed “the debate” these issues of substantial disagreement.

historicizing the controversy, putting some flesh on the methodological skeleton of the discussion.

In the particular case of the controversy on statistical illusions, no less than three fundamental points of agreement built the common background between Friedman and Klein (and the other participants). Both factions considered that:

- (1) Economics was a science whose foundations should be grounded on an empirical approach.
- (2) A way of integrating a rigorous empirical approach in economics was through the practice of econometric modeling (in the large sense).<sup>254</sup>
- (3) Statistical techniques could help economists to develop rigorous criteria to judge the performance of their models, helping them in the discovery and further specification of the underlying economic mechanisms.

These general principles established the point of departure of the controversy between Friedman and Klein. There are, of course, important (and sometimes irreconcilable) differences between the methodological positions of these authors concerning the way to tackle each of these principles. Yet, the important point here is that this common background was not exclusive to Friedman and Klein, but was part of the economists' scientific community.

#### 7.4. The “simple Keynesian model” and the Controversy on statistical illusions

To illustrate their criticism, Becker and Friedman (1957) used a Keynesian model “in its simplest form,” specified according to the absolute income hypothesis.<sup>257</sup> This simple model

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<sup>254</sup> Econometric modeling in the “large sense” means that the definition of econometrics is not reduced to the structural econometric approach developed at the Cowles Commission, but it can also embrace other definitions of econometrics like, for example, the statistical approach of the NBER. See Boumans and Dupont (2011) for a further discussion of the definition of econometrics in a larger sense.

<sup>257</sup> This means, according to John Maynard Keynes's (1936) formulation that “men are disposed, as a rule and on the average, to increase their consumption as their income increases, but not by as much as the increase in their income” (96).

contained a consumption function and a national income accounting identity. Investment was considered autonomous:<sup>258</sup>

$$(1) \quad C_t = a + bY_t + \dots + u_t \quad (\text{Aggregate consumption function})$$

$$(2) \quad Y_t = C_t + I_t \quad (\text{National income accounting identity})$$

with  $C_t$  and  $Y_t$  the aggregate consumption and income in a given year, while  $a$  and  $b$  are parameters,  $u_t$  is the stochastic perturbation of the consumption function, and  $I_t$  is realized investment (which, by definition, is also realized savings).

The common forecasting practice consisted in the use of the reduced forms of the model. In this case, the reduced form equations (3) and (4) are obtained by replacing the structural equation (1) in the accounting identity (2):

$$(3) \quad Y_t = \frac{a}{1-b} + \frac{1}{1-b} I_t + \frac{u_t}{1-b}, \text{ and}$$

$$(4) \quad C_t = \frac{a}{1-b} + \frac{b}{1-b} I_t + \frac{u_t}{1-b}$$

Forecasts of the level of consumption obtained through the reduced form equation (4), would present a small relative error of prediction  $V_c$  (see the first column of figure 8 on page 266), according to which macroeconometricians (under the statistical illusion) tended to consider that their model, as a whole, would perform well.<sup>259</sup> This was a misleading consideration, though, since conclusions on the performance of the whole model were drawn, based on single equation characteristics. This procedure, of course, was not necessarily consistent and entailed misleading results. Becker and Friedman (1957) showed, for instance,

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<sup>258</sup> It is worth noting that Becker and Friedman recognized that this is the simplest expression of the Keynesian theory of consumption. Chapters 8 and 9 of the *General Theory*, show that Keynes's conception of the factors affecting the consumption behavior was much more complex than it is explicitly claimed in this controversy.

<sup>259</sup> To measure the relative error in predicting  $C$ , the standard deviation of the errors  $\sigma_u$  is divided by  $\bar{C}$  (the average value of  $C$ ):  $V_c = \frac{\sigma_u}{\bar{C}} = \frac{\sigma_u}{a+b\bar{Y}}$ ,  $\bar{Y}$  is the average value  $Y$ . The quantity  $V_c$  can be understood as a measurement of the "usefulness" of equation (1) as a predictor of consumption.

that, if macroeconometricians kept this (wrongly) specified model structure, forecasts of the level of income would present dramatically higher relative errors of prediction, revealing that the model, as a whole, did not perform well. This was a relevant point, since the ultimate objective of macroeconometricians was, indeed, to forecast the level of income, while forecasting the level of consumption was only a secondary objective.

Based on the data published in Raymond Goldsmith’s *A Study of Savings* (1955), Becker and Friedman performed a numerical exercise to illustrate their point and to compare the performance of six different consumption equations, following four distinct procedures of prediction (see the annex).<sup>260</sup> Figure 8 describes the relative error in prediction of these consumption functions.

Figure 8: Errors in the Prediction of Consumption and Income from Different Consumption Functions.

CONSUMPTION FUNCTION	RELATIVE ERROR IN PREDICTING			
	Consumption from a Known Level of Income	Income from a Known Level of Investment	Income from an Estimated Level of Investment* (Consumption Known with Certainty)	Income from Estimated Consumption and Investment*
	(1)	(2)	(3)	(4)
(12) $C(t) = 70.2 + .780Y(t) + u_1$ .....	.049	.196†	.100	.242‡
(13) $C(t) = .890Y(t) + u_2$ .....	.057	.462†	.200	.551‡
(14) $C(t) = (.879)(.4) \int_{-\infty}^t e^{-38(T-t)} Y(T) dT + u_3$ ...	.040	.051	.029	.059
(15) $C(t) = 162.0 + 237.0e^{.02t} + u_4$ .....	.066	.059	.020	.062
(16) $C(t) = C(t-1) + u_5$ .....	.068	.061	.020	.065
(17) $C(t) = e^{.02}C(t-1) + u_6$ .....	.064	.057	.020	.061

\* The relative error in predicting investment is assumed to be .20.  
 † Instead of .196 and .462, these figures would be .147 and .420, respectively, if more efficient estimates of the parameters of these consumption functions had been used.  
 ‡ Instead of .242 and .551, these figures would be .178 and .465, respectively, if more efficient estimates of the parameters of these consumption functions had been used.

Source: Becker and Friedman (1957, 67).

In predicting consumption from income (column 1) the Keynesian functions, equations (12) and (13), presented a small relative error of prediction (0.49 and 0.57 respectively), showing superior results (in terms of smaller relative errors) compared to all the other

<sup>260</sup> The alternative consumption functions are presented in the annex 1, and are given here under equations (7) – (12). They are equivalent to Becker and Friedman’s (1957) equations (12) – (17), reproduced here as figure 8. The six alternative consumption functions can be grouped in three categories: (7) – (8) are “Keynesian functions,” (9) is a function specified according to the permanent income hypothesis, and (10) – (12) are naïve models. Also, note that the consumption functions (8) and (9) do not take into account the autonomous part of consumption.

consumption functions, except for (14), whose relative error is the smallest of all (0.40). This was the source of the statistical illusion that would misleadingly justify the maintenance of the Keynesian consumption function.

As opposed to the conclusions that could derive from the evidence that was first visible – the small relative error in predicting consumption – the simple Keynesian model actually presented important flaws in its consumption function. These flaws were translated into misspecifications that led the model to perform poorly in its forecasts of the level of income (see column 2 of figure 8), and was the result of the interaction of single equation random errors. “[A] given error in predicting consumption [was] magnified by the multiplier process into a much larger error in predicting income” (Becker and Friedman 1957, 66). This was so, because “the accuracy of the estimate of income depends not only on the accuracy of the estimate of consumption *but also on the form of the consumption function* – in particular, on the fraction of consumption which [the consumption function] designates as ‘autonomous’” (64, my emphasis). The problem was that the statistical illusion would hide the misspecification of the whole model, and that small relative errors obtained from the single equations built up to produce even larger relative errors in predicting other variables of the model. The presence of multipliers in the Keynesian models made these errors even more important.

This argument set the basis for Becker and Friedman (1957) to conclude that “improvement in the consumption function [...] may [...] make a greater contribution to our ability to predict income than improvement in the estimates of investment” (75). The problem of Keynesian models was not one of statistical or mathematical adequacy, but one of “the structure [these models] attribute to the economic system” (73). Since Keynesian modelers did not apply the correct criterion for judging model performance, they fell into a statistical illusion that led them to accept a misleading model specification, hence the wrong structure. Becker and Friedman did not limit their claim to this (already important) point, but went

further to claim the general principles to be taken into account to specify a correct consumption function. These principles, which were, of course, related to Friedman’s (1957) *A Theory of the Consumption Function*, his Marshallian approach, and his “rule of parsimony,” were put forwards in order to build macroeconometric models.

It is worth noting, that even if the particularly simple Keynesian system used in the critique constituted a pedagogical tool only, the predicting procedure denounced by Becker and Friedman was a common practice among Keynesian macroeconometricians during the 1950s (Bodkin 1995, 46). Becker and Friedman were then correct both in criticizing this practice and in raising this important methodological concern. Indeed, the fact that the relative errors in predicting the level of consumption were small, was not a sufficient condition to consider the whole model accurate in predicting income. Statistical adequacy based on the prediction of a single equation did not immediately mean that the complete model would perform well.

#### 7.5. Keynesian Responses to Becker and Friedman

As mentioned in the introduction, Johnston (1958), Kuh (1958), and Klein (1958) provided three separate responses to Becker and Friedman (1957). Although each one referred to other important aspects of the controversy, four common claims were made. The first reflected a shared feeling that the macroeconometric approach had been unfairly criticized. Indeed, given the obsolete nature of the functions and the time period (1905-1951) to which the simple model had been fitted in the critique, Johnston (1958) declared that it was “hardly [...] surprising to read the final conclusion that these simple functions give an inadequate specification of dynamic characteristics of the economic structure” (296-297). According to Johnston (Kuh and Klein), the type of functions used in the critique had been abandoned in actual practice at least since the last ten years, and in particular after the publication of Franco Modigliani’s (1949) paper.



In this paper, Modigliani criticized Jacob Mosak's (1945) and Arthur Smithies's (1945) forecasting methods because they relied in simple Keynesian functions similar to the one used by Becker and Friedman (1957) to illustrate the Keynesian approach. Modigliani highlighted the difficulties related to the use of these "simple" functions in economic forecasting. Indeed, economists faced a complex task in their forecasting objective, given the "pronounced discrepancy between the cyclical, or short-run, and the secular, or long-run" forms of economic relations in a period of "violent cyclical fluctuations" like the first half of the twentieth century (particularly in the US). Particularly, Modigliani suggested a method "by which it seem[ed] possible to estimate" both the secular and the cyclical forms of economic relations (371). Based on Modigliani (1949), Johnston (1958) estimated two consumption functions that allowed for the differentiation of (a) the short-run or cyclical marginal propensity to save, and (b) the long-run average and marginal propensity.

$$(5) \quad C_t = a + bY_t + Y_t^o, \text{ and}$$

$$(6) \quad C_t = a + bY_{t+1} + Y_t^o$$

where  $Y_t^o$  is the highest income experienced before time  $t$ .

Judged according to Becker and Friedman's (1957) criteria, both equations (5) and (6) presented a well predicting performance. The relative error in predicting consumption and income from equation (5) was  $V_C = 0.0304$ , and  $V_Y = 0.053$  respectively. In the case of equation (6) these relative errors were of  $V_C = 0.53$ , and  $V_Y = 0.0485$ . With this estimation, Johnston not only made his point that the simple model used by Becker and Friedman as an illustration of the Keynesian model was no longer used in actual practice, but also that alternative consumption functions that could easily improve the results obtained by Becker and Friedman (which was another common claim of the respondents) existed. This improvement of the consumption functions did not consist on envisaging a future possibility

of refining existing functions. In fact, far more sophisticated consumption functions which yielded more adequate results, even according to Becker and Friedman’s criteria, had already been used. In this respect, Klein (1958) asserted that in the actual practice, those “who have seriously attempted forecasting from Keynesian models had used much more complicated systems in which the reduced-form equation for consumption is vastly different from and [...] superior to [Becker and Friedman’s]” (540).

The fourth common claim consisted on the macroeconometricians’ consideration that there had been no statistical illusion. The three respondents recognized, however, the existence of simultaneous equations biases in the forecasts obtained from reduced form equations. Yet, these problems were already known to econometricians and, according to the respondents, Becker and Friedman had discovered nothing new. Klein, for instance, claimed that “when Becker and Friedman showed that the variance of  $[\frac{u_t}{1-b}]$  in the multiplier equation is much larger than the variance  $[u_t]$  in the consumption equation (both expressed as a percentage of average consumption or income), [Becker and Friedman] are not proving the general inadequacy of Keynesian models or even of the consumption function.” To Klein, they were “simply giving a laborious demonstration of the fact, already well known, that the simple multiplier model is not suitable for more than pedagogical use in the classroom” (540), and acknowledging relative errors in prediction would accumulate in models presenting important multiplier effects.

The fourth common claim stated that there had been no statistical illusions in judging Keynesian models. Although, seen from a pure methodological perspective, this would concede the argument to Becker and Friedman, in his response, Klein seemed to intentionally deviate attention from it by providing a different interpretation of what the critique actually meant. Referring to Haavelmo (1947), Klein claimed that “the appropriate way to estimate the structural parameters of equations in a complete system is to regard the whole set of

equations simultaneously from a statistical point of view” (539-540). In the case of the consumption function, this meant “the propensity to consume should be estimated from the reduced forms and not by direct reference to the consumption function, to the neglect of the accounting definition” (540). Because at first sight, Klein’s response seems to go in the sense of their criticism, one understands why Becker and Friedman (1958) considered the macroeconometricians’ responses as a “supplement” (545) of their article. Still, the problem raised was that, although Klein was probably right in evoking the strength of his models, he focused too much on the ability of his reduced forms to yield accurate *extrapolations* of the economy. Klein, however, misses the point that this criterion does not always constitute a sufficient measure of the accuracy of model performance.

#### 7.6. Brown’s anticipation of the permanent income hypothesis

Another important point of the controversy has to do with Klein’s (1958) claim that “Brown’s work on lags in consumer behavior is truly a complete anticipation of the Friedman-Becker article” (541).<sup>261</sup> T. M. Brown (1952) had developed his “habit persistence theory,” where, contrarily to Modigliani (1949), previous real consumption, and not previous income played an important role in the determination of the level of consumption. Brown claimed that the “lag effect in consumer demand was produced by the consumption habits which people formed as a result of past consumption. The habits, customs, standards, and levels associated with real consumption previously enjoyed become ‘impressed’ on the human physiological and psychological systems and this produces an inertia or ‘hysteresis’ in consumer behaviour” (539). A similar feature between Brown (1952) and Modigliani (1949) was that both approaches allowed for distinguishing between short-run and long-run marginal propensities to consume (see Brown’s model in the annex).

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<sup>261</sup> Klein quoted C. Christ (1951), S. J. Pairs (1951), L. M. Koyck (1954), Klein and A. Goldberger (1955), and R. Stone and D. A. Rowe (1956) also as anticipating Becker and Friedman’s proposed consumption function:  $C(t) = k\beta \int_{-\infty}^t e^{(\beta-\alpha)(T-t)} Y(T) dT + u_3$ .

Friedman (1958) actually conceded this point to Klein (1958) by stating that he “did not recognize that [his] procedure was equivalent to Brown’s use of the consumption of the preceding year, and [that] Klein [was] quite right in criticizing [him] for this error of omission” (549). As noted by Bodkin (1994, 55), Friedman might have hastened too much in conceding this point of the controversy to Klein. Balvir Singh and Aman Ullah (1976) showed “Brown’s model [was] by no means ‘truly a complete anticipation of Friedman’” (101-102). In fact, “even though the Friedman model [...] looks quite similar to the one suggested by Brown, the models differ in terms of the nature of the regressors, interpretation of the error term, and the nonlinearity in parameters” (101).

In my opinion, the essential point here is not whether Brown anticipated Friedman or not. The truly important point is whether the Keynesian and the non-Keynesian models present specifications close enough to be considered similar. This is important in order to understand the extent to which other criteria to judge model performance and model relevance, apart from the methodological approach itself, play a role in molding and shaping economic models.

Economists (like other scientists) rely on a battery of criteria to choose among different model specifications and to shape their models – or theories. This battery of criteria is not completely rigid, and might be composed of a variety of elements such as statistical tests, adequacy of mathematical forms and procedures, the nature of data, political convictions, or even particular material and historical conditions like the existence and availability of digital computers, or the access to funding.<sup>262</sup>

### 7.7. Differences in the concepts of prediction

In terms of methodology, the most important point discussed in the controversy was about how to adequately evaluate model performance. Macroeconometricians considered that *ex-*

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<sup>262</sup> These questions are not answered in this version of the chapter, however.

*ante* or *ex-post* forecasts or *extrapolations* from the reduced form equations, were an adequate way to evaluate model performance. As discussed above, both the critics and the econometricians knew that this procedure could produce biased results. Keynesians thought of these biases as the result of a statistical problem, particularly simultaneous equations biases. Becker and Friedman, however, insisted that the biases were caused by a poor model specification, considering them a result of an economic theoretic problem. According to the Keynesians, the best way to avoid these problems was through painstaking tinkering, re-estimation, and actualization of the model, and through the inclusion of more explanatory variables in each equation. Klein (1958, 545) for instance, claimed that:

Researchers have sought improvement in the Keynesian consumption function through the introduction of new variables. There are great limits to the extent to which one can come upon radically improved results by juggling about the same old variables in a different form. Instead of adhering to the ‘rule of parsimony,’ we should accept as a sound principle of scientific inquiry the trite belief that consumer economics, like most branches of our subject, deals with complicated phenomena that are not likely to be given a simple explanation [...] The addition of extra predetermined variables (not lagged incomes) that are not correlated with income or that do increase the multiplier are likely to improve the fit of the multiplier equation at the same time that they are improving the fit of the consumption equation. I venture to predict that much good work will be done in the years to come on adding new variables to the consumption function and that it will not be illusory.

Becker and Friedman, on the contrary, made a plea for econometricians to adopt a different economic structure, based on a consumption function specified according to the permanent income hypothesis. Their plea though, was also directed towards the development of parsimonious models that would explain more through simpler equations. In this particular case, both critics and respondents, were exclusively focusing on how to solve the problem of obtaining biased results. However, none of them actually attempted to identify if the origin of the problem was statistical or economic.

To detect and define the nature of the problem, Keynesians and critics had to find an adequate measure, allowing for the understanding of the kind of problem they were facing. In

this case too, they still could not agree on what an adequate criterion would be, since they valued different kinds of *predictions*. On the one hand, the Keynesians thought that *extrapolation* was a more important kind of *prediction* in that it constituted the more adequate criterion to judge model performance. Yet, by the 1950s, these extrapolations were performed only on the bases of reduced form equations and not on the bases of whole model simulations, partly because computers were still not available for economists in general, but more importantly, because macroeconometricians really believed that reduced forms forecasts were adequate criteria to judge model performance.

On the other hand, Becker and Friedman suggested that predicting did not necessarily mean extrapolating. To them retrodiction and extrapolation were equivalent in evaluating model performance, and the really important criterion was to find out whether extrapolation or retrodiction was performed on the basis of reduced form equations, or on the basis of full model simulations. To them, again, only full model simulations would provide adequate ways of evaluating model performance.

#### 7.8. Klein’s emphasis on extrapolation

As mentioned in the introduction, a somewhat surprising way of interpreting Becker and Friedman’s (1957) paper is to consider it as an important contribution to large-scale macroeconomic modeling. In fact, their critique can be understood as an anticipation of full model simulation as an important method to evaluate model performance. In this sense, again, the evaluation of the simultaneous equations characteristics of structural functions would be a better criterion to judge model performance, compared to single equations characteristics.

Although Klein adopted this criterion later on, as the rest of the discipline did (see Klein (1969)), in the 1950s, Klein was skeptical about Becker and Friedman’s proposition. His skepticism relied on his different views on the nature of predictions. To Klein, Becker and

Friedman's proposition put too much emphasis on the goodness-of-fit measures obtained for *sample period predictions*, or *retrodictions*. Instead, Klein appraised other criteria like *out-of-sample predictions* or *extrapolations*, like the model's capacity to predict turning points in the economy. Making a clear reference to Friedman (1953), Klein considered that Becker and Friedman's proposition was rather "strange," since "Friedman [had], on many occasions, stressed the criterion of predictive ability as a suitable test of theory." And yet, in his defensive effort, Klein disregarded the fact that full model simulation provided also a kind of prediction. Only, this was a different kind of prediction.<sup>263</sup>

Klein's idea of prediction focused on the "painful [test] of experience," i.e. the comparison between observed values and *ex-ante* or *ex-post* out-of-sample "concrete forecasting results" (543) of alternative models. For instance, to Klein and Goldberger (1955, 72),

The severest test of any theory is that of its ability to predict. Our equation system presents a theory of economic behavior in the aggregate. We have fitted the model to the sample, and although it may be an achievement to find a structural system which does fit the observed facts, we cannot be satisfied with the performance of the system solely with reference to the sample data [...] In a broad sense, we mean, by prediction, the ability of the equations to explain aggregate economic behavior for sets of observations outside the sample.

Klein considered both *ex-ante* and *ex-post* extrapolations as important constituents of his predictions. These were complementary types of predictions and both had to be conducted in order to evaluate model performance. Yet, between *ex-ante* extrapolations, Klein considered that policy simulation was more useful than mere forecasts of the level

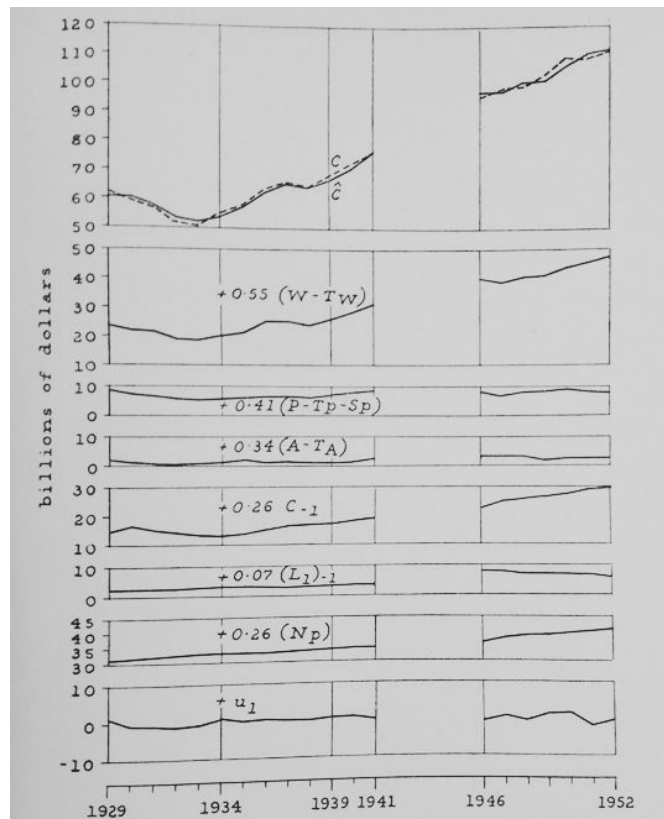
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<sup>263</sup> It is important to note also, that full (or even block) model simulations represented a huge technical challenge for econometricians, considerably increasing the bulk of their calculations. Furthermore, computers were not only in an early stage of development, but they were not available to every economist. As Irma Adelman (2007) recalls, "the first application of digital computers to the solution of a macroeconomic model occurred through happenstance" (29), since the computer actually belonged to the Berkeley radiation laboratory, and since Irma's husband, Frank, was actually hoping to use the first general purpose computer (the IBM 650) in a physics problem. Yet, given "that all physics problems were too complex," they decided to look for a more "suitable model": the 25-equations Klein-Goldberger macroeconomic model.

of particular endogenous variables. Klein and Goldberger, for instance, explained this procedure in the following way (Klein and Goldberger 1955, 72):

We have approached this problem empirically from two points of view. In an *ex-post* sense we may insert observed and essentially correct values of predetermined variables in the model and solve it algebraically for the values of endogenous variables in the forecast period. One interested in the degree to which our model represents a true picture of behavior should base this judgement of performance on this *ex-post* type of extrapolation. This is a case of testing the model outside the confines of the sample and determining how well it fits actual observations when there is no statistical forcing towards conformity [...] If the *ex-post* extrapolations have shown a system to give a good explanation of economic behavior, we can then place a measure of reliance on the use of this system to show what *would* happen if exogenous variables *were* to be placed at particular assumed levels. We might show, for example, the sensitivity of aggregate activity to variations in government tax-expenditure plans. This is a form of *ex-ante* forecasting and may be a more useful econometric application than pure forecasts of the levels of endogenous variables (emphasis in the original).

Figure 9: Klein-Goldberger (1955) Time Paths Consumption Equation



Source: Klein and Goldberger (1955, 93).



Figure 9 is an illustration of the type of *ex-post* forecasts performed by Klein and Goldberger (1955). Klein and Goldberger presented ten figures of this type, where they examined the behavior of ten most important equations of their model individually. Each graph consisted of a first chart (on the top of the figure) where the observed and forecasted trends of the endogenous variable for the whole period (1929-1952) were presented. The remaining lower charts presented the individual contributions of the exogenous variables and the errors in explaining the behavior of the endogenous variable. Here, I reproduce the example of the consumption function.

### 7.9. Full model simulations as a Turing Test

Full model simulation could be understood as a sort of Turing test, or as an “imitation game” (Turing 1950).<sup>264</sup> In order to pass the Turing test, the model should be able to generate data series that are indistinguishable from the data actually observed in the economy. In other words, the model should be able to imitate reality. If the model-generated data produced a pattern that was comparable (although not necessarily equal) to the “real-world” data, then the model would fulfill this test.

This was, in fact, the objective of the Adelmans’ (1959) simulation of the Klein-Goldberger (1955) model. In particular, the Adelmans examined whether the model “really [offered] an endogenous explanation of a persistent cyclical process [...] whether the system [was] stable when subjected to single exogenous shocks, what oscillations (if any) accompany the return to the equilibrium path, and what is the [response] of the model to repeated external and internal shocks” (597). To do this, the Adelmans simplified the 22 simultaneous equations model through algebraic substitutions, into a “set of four simultaneous equations in four unknowns” (600). This allowed them to significantly reduce the computational time of the model. The Adelmans performed at least four different simulations in which they asked

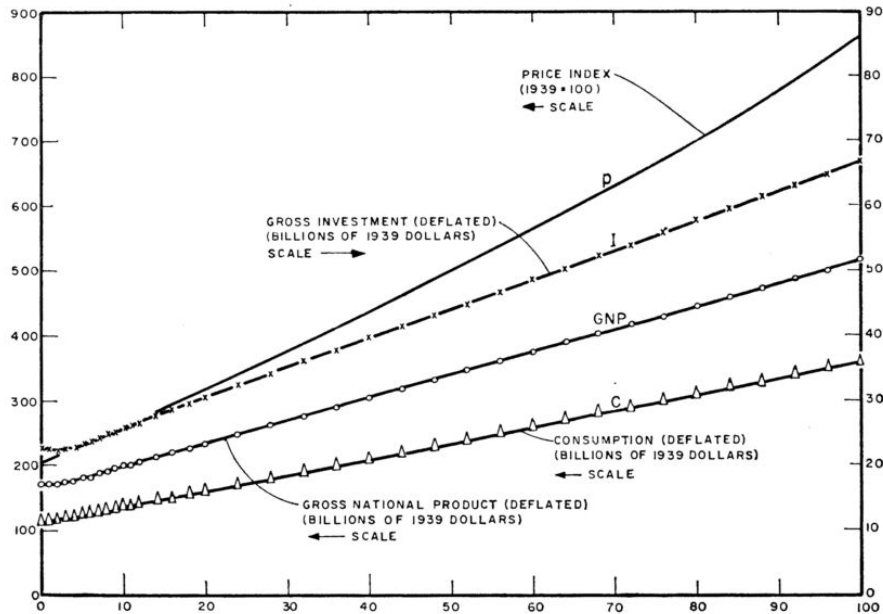
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<sup>264</sup> The contemporary discussion was held neither in terms of conducting a “Turing test” nor an “imitation game,” of course.

different questions. The first simulation dealt with the deterministic form of the model, and showed that in absence of external shocks, the system was monotonic and essentially linear.

Figure 10 presents the results of the first simulation:

Figure 10: Klein-Goldberger Time Paths (without any shocks).

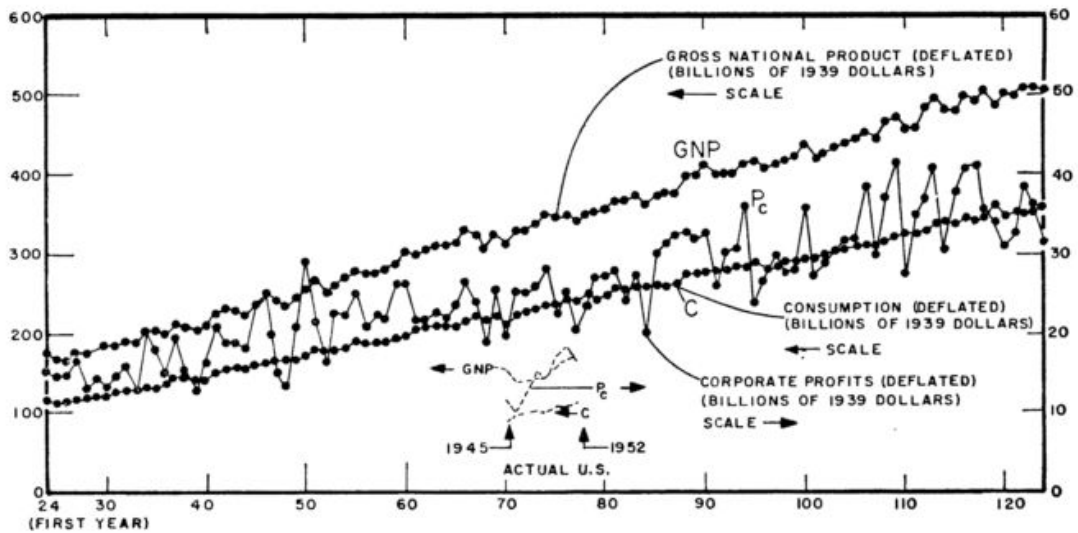


Source: Adelman and Adelman (1959, 601).

The other simulations included different types of shocks. First, they simulated the behavior of the system when “shocks of type I” were introduced. This type of shocks consisted on the introduction of a severe and sudden shock in terms of the reduction of government expenditures. Because this simulation produced cycles that were “unrealistically small” (609), another type of shocks was introduced.

Shocks of type II, transformed the deterministic system into a stochastic system through the introduction of errors into each of the estimated equations. This time (figure 11), the model appeared to be more realistic since it did not only produced three- to four-year cycles, but most importantly, the magnitudes of the cycles “here compare favorably in magnitude with those experienced by the United States economy after World War II” (611).

Figure 11: Klein-Goldberger Selected Time Paths under Type II impulses.



Source: Adelman and Adelman (1959, 611).

The importance of the Adelmans' simulations, especially of those of the stochastic Klein-Goldberger system, resided on its resemblance to the behavior of the US economy. This resemblance, however, was not only qualitative in nature, but also quantitative.

All in all, it would appear that there is a remarkable correspondence between the characteristics of fluctuations generated by the superposition of random shocks upon the Klein-Goldberger system and those of the business cycles which actually occur in the United States economy. The resemblance is not restricted to qualitative parallelism, but is, indeed, quantitative, in the sense that the duration of the cycle, the relative length of the expansion and contraction phases, and the degree of clustering of peaks and troughs are all in numerical agreement (within the accuracy of measurement) with empirical evidence.

#### 7.10. Concluding Remarks

In a nutshell, Becker and Friedman's (1957) criticism of the "statistical illusion" in judging Keynesian models consisted in two points. The first focused on how to correctly evaluate model performance. According to Becker and Friedman, the performance of the entire macroeconomic model should not be evaluated on the basis of the "apparently" good performance in prediction of the consumption function alone. Implicitly, this point was directed towards the idea that the single equation characteristics are not sufficient to evaluate the performance of the whole model. Instead, to make a sound evaluation of the performance

of their models, econometricians should focus on the structural equations characteristics, and undertake whole model simulations. This argument would become common ground in the subsequent years among macroeconometricians, but at the time, some confusion existed about the necessity of this criterion. Adelman and Adelman's (1959) simulation of the Klein-Goldberger (1955) model contributed to the establishment of whole model simulation as a criterion of model evaluation. Other important contributions in this direction were the further development of the digital computer, and the dissemination of the macroeconomic modeling approach.

The second point was related to the specification of the consumption function itself. In this respect, Becker and Friedman insisted that the Keynesian function had to be changed and improved. The important points here are that Becker and Friedman proposed two statistical criteria (the relative error in predicting income from the reduced form equations, and the naïve models discussed in chapter 5) to guide the specification of the new consumption function, but also advanced another argument in favor of the permanent income hypothesis proposed by Friedman (1957).

These points, however, have to be understood at two different levels. First, as a claim inscribed within a larger debate between two empirical approaches to macroeconomics, and so as a claim directed towards the abandonment of the large-scale macroeconomic approach. More importantly, however, is the other level, which interprets Becker and Friedman's (1957) claim as a constructive critique and as precursor of a criterion to evaluate model performance that would become common ground around macroeconometricians: full model simulations, or dynamic model simulations. Regardless of its outcome, this controversy opens a huge exploratory window to historians of economics allowing for a better understanding of the theoretical and methodological developments of the discipline in the

second half of the twentieth century. And yet, to my knowledge, this particular controversy has never been taken into account in the history of macroeconomics and econometrics.

## Annex to chapter 7

Becker and Friedman's alternative consumption functions:

$$(1) \quad C(t) = a_1 + b_1 Y(t) + u_1 \quad (\text{Keynesian consumption function 1})$$

$$(2) \quad C(t) = b_2 Y(t) + u_2 \quad (\text{Keynesian consumption function 2})$$

$$(3) \quad C(t) = k\beta \int_{-\infty}^t e^{(\beta-a)(T-t)} Y(T) dT + u_3$$

(Permanent income hypothesis)

$$(4) \quad C(t) = a_2 + Ae^{at} + u_4 \quad (\text{Naïve model 1})$$

$$(5) \quad C(t) = C_{t-1} + u_5 \quad (\text{Naïve model 2})$$

$$(6) \quad C(t) = e^a C_{t-1} + u_6 \quad (\text{Naïve model 3})$$

Brown's (1952) Model

$$(7) \quad C = a_0 + a_1 Y_w + a_2 Y_\pi + a_3 C_{t-1} + a_4 A^c + u_1,$$

the consumer demand equation, with  $A^c$  a "shift variable" that shifts the whole consumer demand equation upward in the postwar period,  $Y_w$  and  $Y_\pi$  income from wages and profits, respectively.

$$(8) \quad Y_w = b_0 + b_1(Y + S_1) + b_2(Y + S_1)_{-1} + b_3 t + u_2,$$

$$(9) \quad Y = Y_w + Y_\pi$$

$$(10) \quad (Y + S_1) = C + S_0 + S_1$$

where  $S_1$  represents  $T + D - J + \frac{1}{2}R$ ;  $T$  = government disposable income, which equals total taxes from, less total transfers to, the private sector of the economy;  $D$  = depreciation;  $J$  = inventory valuation adjustment;

$\frac{1}{2}R$  = residual error of estimate on GNP side of national accounts;  $t$  = calendar year - 1926;

$S_0 =$  total spending offsets to the savings of the private sector = net home investment + net foreign investment + the government deficit – R;

$S_0 + S_1 =$  government spending + gross private domestic investment net foreign investment – R .











# Conclusion: economics as a “tooled” discipline

Contrary to what many people seem to think, it is in the practical application of theories to facts, in attempts to draw conclusions on the concrete level, that the need for stringent logic and fancy mathematics really shows up.

*Trygve Haavelmo, 1958*

### 8.1 Klein and the formation of a new scientific practice

Through an examination of Lawrence R. Klein’s early years (1939-1959), we have seen how a new scientific practice (large-scale macroeconomic modeling) was formed and crafted, and have considered six elements central to this formation: (1) Built around specific institutional configurations, macroeconomic modeling produced a system of reasoning that embodied (2) macroeconomic models (which provide a component of mechanical objectivity to macroeconomic analysis), and (3) macroeconomic experts (who provide judgement, context-dependent analysis, and skillful agency at every stage of the practice); in turn, (4) an institutionalized combination of models and experts generated a new way to produce rigorous and scientific knowledge in macroeconomics. In addition, this system of reasoning operated around (5) a setting of specific objectives (understanding and intervening the economy), and (6) a conception of ongoing projects in which teamwork (with a specific division of tasks and a place for creativity), allowed not only to build increasingly complex models (to improve the understanding of the economy), and to build in new a priori knowledge (to improve the intervention on the economy), but also to provide a training *milieu* for the experts, who,

immersed in the practice not only learnt about both the economy and the model through constant revision of theories, data, and a priori information, but also intervened the model through continuous adjustment, tinkering, and experimentation.

By the end of the 1950s, the macroeconometric modeling practice was gaining importance, and was defining its institutional configuration at places like the Wharton School, and Michigan University, two sites where Klein had planted a definitive seed. Conceived as forecasting devices since their materialization in the Cowles Commission, macroeconometric models had been able to insinuate their potential to find out about the economy, although concrete forecasting results remained scarce and quite timid. On the one hand, the 1945 episode when Klein successfully used Model III to make the first macroeconometric projections of the US economy after World War II, remained somewhat hidden for a good part of the economics community and for the public and private spheres. After all, some members of the Cowles Commission considered the forecasts too optimistic and the model too preliminary. On the other hand, although Klein and Goldberger's (1954) press article published in the *Manchester Guardian* contradicting Clark's predictions attracted the attention of a broader and, this time also, international audience, macroeconometric modeling *à la* Klein had not yet found the institutional bases allowing for the periodic production of forecasts, nor had it captured the public's credibility or the ears of the right government authorities.

That this was the case is not surprising at all when one thinks about the novelty of the whole macroeconometric modeling practice. The reasons for a fragile credibility and dissemination were related not only to the fact that macroeconomics was still in the process of being institutionalized; or that econometrics was a very recent method not very well understood even by economist, which presented numerous methodological and practical problems inherited from the ambitious project of bringing together statistical, probabilistic, mathematic, and economic reasoning; or that modeling was still struggling to become the most important

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form of reasoning and method of inquiry in economics; or that it was hard for the economists to understand that macroeconometric modeling had to be undertaken as a teamwork effort, and that a complete institutional infrastructure of econometric laboratories with seminars, equipment, and personnel had to be constructed. In addition to all that Klein, too, as architect and executer of macroeconometric modeling, was experimenting and groping in the dark, discovering, reinventing, and adjusting his practice to the disciplinary, institutional, and political conditions around him.

Despite the relative limitations of the first results, the meager institutional infrastructure, and the difficulties entailed by the political atmosphere in the era of McCarthyism, Klein's optimism about macroeconometric modeling never decreased. Instead, he remained convinced that this practice provided not only a better understanding of the economy, but also specific tools to help decision makers evaluate the possible effects of intervening the economy. Indeed, through his macroeconometric model building (as Klein preferred to call it), Klein was able to depict the structure of the economy in a systematic and rigorous way, understanding the behavior of important economic variables as well as their interrelations. Once the structure of the economy was specified, Klein was then able to experiment with his models, simulating the effects of economic policies. This whole exercise, again, was accompanied not only by a rigorous (statistical) interpretation of the results by the experts as teams, but also by a constant intervention of the team members in the construction, maintenance, and adjustment of the model, in the gathering and preparation of data, and in the discussion of ideas in research seminars. By the end of 1950s (and even by the beginning of the 1960s), however, the seminars and procedures were still very new, somewhat fuzzy, and clumsy. Daniel Suits recalls macroeconometric modeling in Michigan, in the 1950s, as a rudimentary practice in which the students “lived” in the econometric laboratory, and where the calculation techniques were quite painstaking (and sometimes even amusing):

The real pros lived down there. They argued with each other and settled all the problems of the universe. The seminar would tool up in September when the students arrived and the assignment was to take the model apart and see where it had functioned poorly last year and what should be done about it to improve it, with the notion that come the second or third week of November [...] somebody had to stand up in front of that Conference on the Economic Outlook and produce a forecast from this model [...] Then for the next semester we'd do whatever came to our heads to do.

The first calculations [...] were all done by hand [...] you had to iterate with a desk calculator [...] and the first forecast I ever made with it I was working until about four o'clock in the morning and I couldn't get the iteration to converge. And it happened that [Susumu] Koizumi was coming home from a date [...] and [...] he saw the light on in the seminar. He came in to see what was going on, and here I was trying to get this darn thing to converge [...] this is four o'clock in the morning [and] four o'clock this afternoon I've got to be on. And so he said, 'Well, I'll get on one calculator and you get on the other one, and I'll start low and you start high, and [...] we'll converge on it.' So we kept on going [...] and, by God, if we didn't pass each other and it still hadn't converged. And then I discovered that the calculator that I was using, an old Monroematic [...] was making an error.

So then we got more sophisticated, of course, and began to do the forecasting with a computer. And I remember one time [...] when Saul Hymans was in Washington at the Council [...] he called me up to find out what [the forecast] was [...] just as I got the sheet out of the computer. I looked at it, and somebody had got a big blooper in it. And I said 'Saul this shows that the increase in GNP next year is going to be about 200...trillion dollars. There was a pause on the end of the line, and then Saul said, 'Is that constant or 1964 dollars?'<sup>264</sup>

Yet, the development and further consolidation of macroeconomic modeling as a scientific practice did not happen separately in each particular institution, seminar, or laboratory where the practice was carried out. Since these institutions, seminars, and laboratories belonged to a broader space of the economics discipline, macroeconomic modeling had to be assessed by the disciplinary community for its scientific value. I therefore focused on important controversies in this dissertation. In fact, the assessment of macroeconomic modeling occurred through rigorous discussions and controversies that were held in seminar rooms or conferences, through personal communication or

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<sup>264</sup> Suits, quoted in Brazer, "The Economics Department of the University of Michigan: a centennial retrospective," TUMA, box 5, 143.

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correspondence, and through publications in peer-reviewed journals with replies and rejoinders. These discussions, however, did not take place exclusively between economists *qua* economists; rather, they also came about between persons, who might well have been economists, but who represented institutions like a university department, or a philanthropic foundation, or a government agency. In this sense, the assessment did not occur exclusively in academic terms (rejection for publication, engagement in formal discussion), but also in more institutional terms expressed, for instance, in the provision of financial aid, the approval of a new seminar, or the organization of a conference.

In other words, the creation, assessment, and further consolidation of macroeconomic modeling must be understood in two different spheres: (1) in the local sphere, this assessment consists in the institutional organization of teams of experts with a specific division of tasks to build models; and, (2) in the disciplinary sphere, it consists in the broader space of academic and professional economics that evaluates whether the practice meets the standards of science within the economists’ community. Although one might have the impression that these standards are rigid, universal, and timeless, they are, in fact, flexible, and historically and geographically situated, which means that they can (and do) change.

Whereas the first part of my dissertation (except for chapter 2) dealt primarily with the “local sphere” and with the construction, conception, and first attempts to put in practice macroeconomic modeling, the second part dealt with a characterization of the “community sphere.” Indeed, macroeconomic modeling did not happen in an isolated *milieu*, or in a bunker in the Cowles Commission, the Survey Research Center, or the Wharton School. Instead, the work that Klein and others performed seemed to be so important and innovative that the community had necessarily to react and figure out what this econometric project was all about, and whether it met the scientific standards expected by the community. In the case of macroeconomic modeling, Friedman was a paramount figure in this opposition. On the



one hand, he had an important institutional position both at the NBER and the University of Chicago, which gave him great visibility within the community. On the other hand, he considered seriously the work made by Klein, making important criticisms that ended up being constructive for the whole practice. For instance, Friedman and Becker's (1957; 1958) insistence that the best way to evaluate the performance of macroeconomic models was through the assessment of the system as a whole (or through a simulation of the model as a whole), had an important effect on the profession, which later, and partially due to the improvements of computerization, ended up adopting that criterion of evaluation. This debate, however, also reinforced the consolidation of a way to assess model performance that focused on criteria of statistical consistency and adequacy with economic theories, rather than on the historical performance of models. This emphasis on statistical adequacy to assess the performance of models would prove decisive in the debates of the 1970s when large-scale macroeconomic models became the target of a series of attacks notably by Robert E. Lucas.

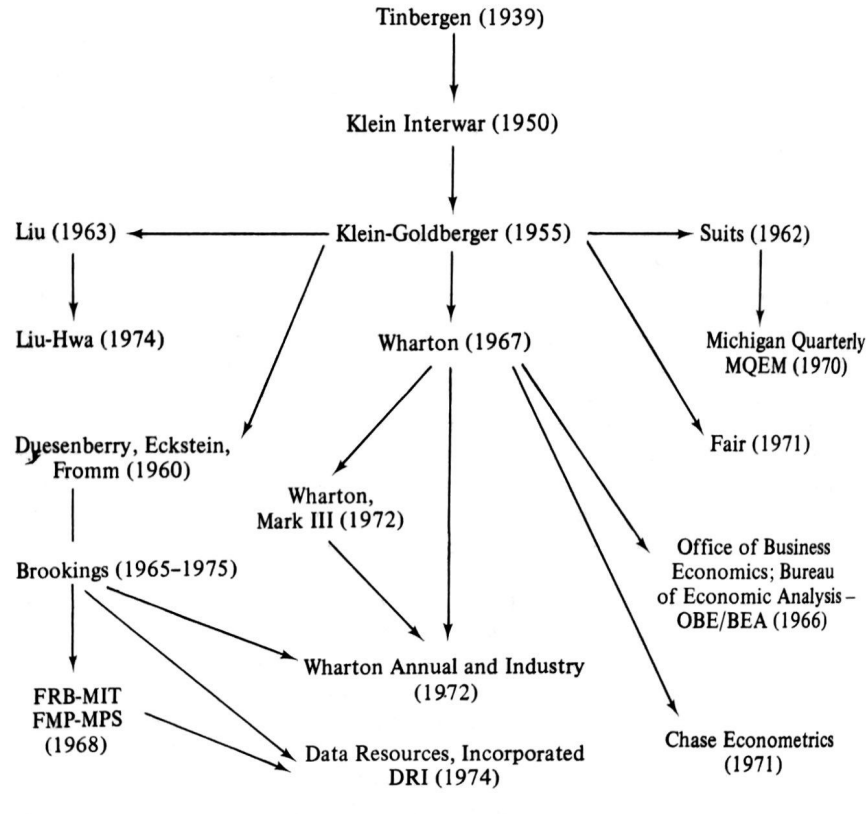
### 8.2 What comes next?

During the 1960s, Friedman continued to be a fierce opponent to Klein's large-scale macroeconomic modeling, and even more so, since the discipline experienced an explosion in the number of models built in the United States, and in other parts of the world. As Intriligator's (1978, 452) figure 12. 3 shows (here reproduced as figure 12), Klein's (and the Wharton's) models became the central axis for the development of the discipline.

Contrarily to what I do in this dissertation, Intriligator's scheme focuses on models *qua* models and not on the practice that accompanies those models. Without pretending that this kind of schemes could possibly illustrate a more intricate and complex history of macroeconomics that focuses on practices and institutions, Intriligator's figure is illuminating as a visual idea of Klein's centrality within this proliferation of models of the 1960s and 1970s, in the United States. A history of macroeconomic modeling of the subsequent decades that

considers the building of all these models is now necessary to understand how the practice evolved in its diversity and adaptability, and how elements such as the institutional organization, the division of tasks, the level of computerization, or the objectives pursued were adopted at different rates in different teams to build the following models.

Figure 12: Family Tree of macroeconomic models



Source: Intriligator (1978, 452)

In this continued narrative, models will not only become impressive systems of hundreds of equations with larger and more professionalized teams, but the whole practice will become also part of the educational curricula of various universities, and the project will reach global spread. The fact that, contrarily to the 1940s and the 1950s, more complete archival material exists for the 1960s, 1970s, and 1980s, is also an encouraging indication that the continuation of this project through these decades will yield important results to enrich our understanding of twentieth century macroeconomics. Yet, here again, the examination should be made considering national and even institutional idiosyncrasies, since the proliferation of

macroeconomic models that happened in the United States, for instance, was very different from that occurred in Europe, Latin America, or Asia. In fact, the US macroeconomic industry was characterized by a considerable amount of competition between models that were built not only by various public authorities (the Federal Reserve Board, the Council of Economic Advisors, the Department of the Treasury) but also by private organizations (Wharton, Brookings) and commercial institutes which aimed at producing forecasts for the commercial sector (Den Butter and Morgan 1998, 450; 2000). On the contrary, in countries like The Netherlands or Norway, macroeconomic modeling was more closely related to authorities at the central level, although important differences exist there as well. While macroeconomic models contained projections only to inform policy makers in The Netherlands, in Norway these models formed the basis of the implementation of economic policy through the elaboration of comprehensive macroeconomic plans (449-450). There is an additional branch of exploration that is not reflected in Intriligator's figure, however. This branch consists in the study of the international dissemination of macroeconomic modeling, not as a set of multiple initiatives at the national level, but at the regional and global scales. Here, Project LINK is the logical place to look at. In fact, this project represents the continuation of Klein's ambitious idea that a macroeconomic model should be built for policy purposes, taking the whole economic system into account through the integration of smaller sub-systems or blocks. Also, the institutional configuration of the consortium integrated by more than 60 countries and coordinated by the United Nations, makes an important case for continuing the exploration of the role that institutions play in the production of economic knowledge, and how the policy sphere enters the modeling practice.

### 8.3 Towards a history of macroeconomic modeling

I consider my dissertation to be a contribution to the present historiography understood in two different senses. In its first sense, historiography constitutes the body of knowledge of the history

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of economics, and, in the particular case of my dissertation, the body of knowledge of the history of macroeconomics and econometrics. By focusing on a *history of macroeconometrics*, my dissertation overcomes the artificial boundaries set by some historians between econometrics and macroeconomics, and provides a richer understanding of the history of both sub-disciplines that gets closer to the actual practices of contemporary macroeconomists and econometricians. By focusing on Klein and on the development of macroeconomic modeling as central objects of study, my dissertation fills up a gap of important events that have not been thoroughly studied yet and that shed light into the way macroeconomics was actually done in the postwar period.

In the second sense, historiography constitutes the methods used by historians of sciences to write the history of sciences. In this sense, my dissertation pretends to contribute an additional piece to a field like Science and Technology Studies (STS), focusing on the interrelations between knowledge, sciences, technology, and society. Indeed, in this dissertation, I attempted at providing a material history of the conditions and personae that made possible the emergence of macroeconomic modeling characterized as a locally and institutionally created practice, situated in specific political, historical, and geographical contexts, in which technological objects such as models that “talk” became the new scientific standard to produce macroeconomic knowledge. I hope that this particular case might be illustrative to other studies of this kind.

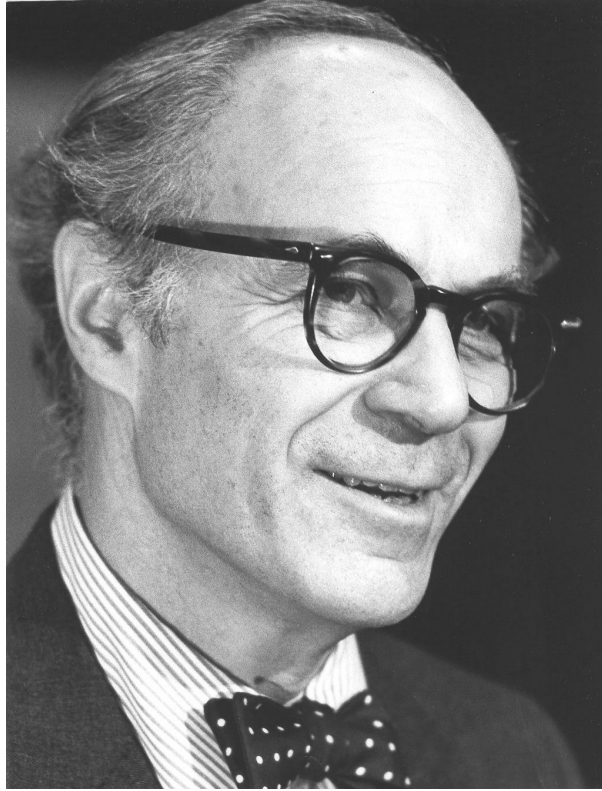
The results of my dissertation allow me to make three bold claims about the historiography of macroeconomics and econometrics: (1) a new history of macroeconomics should embrace a broader approach that emphasizes both the empirical side of macroeconomics and the practices of econometricians considering methodological, historical, theoretical, and epistemological matters in order to provide a history of the *discipline*, rather than a history of *theories* or *ideas*. In this sense, (2) a history of econometrics should focus on the construction, adaptation and application of methods and theories, and on the scientific, social, intellectual,

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economic, and historical conditions that made possible the construction and application of econometric methods. And finally, (3) these new kinds of histories of macroeconomics and econometrics must necessarily consider Lawrence R. Klein as one of the central figures in their analyses and narratives. Indeed, Klein was not only the person who created and forged macroeconometric modeling as a practice, but it was also his kind of macroeconometric modeling which dominated empirical practices in macroeconomics for more than thirty years, from the 1940s to the 1970s.

\* \* \*





Lawrence R. Klein

Source: Washington Post, [www.washingtonpost.com](http://www.washingtonpost.com)







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# Résumé de thèse

## *L'économie saisie par les outils : Lawrence R. Klein et la construction de la modélisation macro-économétrique, 1939-1959*

*Objectif de la thèse : Klein et la construction d'une nouvelle pratique scientifique*

Les modèles macro-économétriques ont joué un rôle décisif dans la transformation de la macro-économie aux Etats-Unis depuis les années 1940, tant dans la sphère politique qu'académique. D'une part, inscrits dans une époque de libéralisme progressif qui poursuivait la stabilité économique à travers l'intervention de l'État, les modèles macro-économétriques ont pourvu les économistes avec des outils de planification économique puissants. D'autre part, ces modèles ont aussi changé la manière dont la connaissance macro-économique était produite, en insistant sur l'aspect empirique ainsi que sur une sophistication technique dans le contexte d'une informatisation croissante et d'une transformation des pratiques scientifiques qui s'appuyaient de plus en plus sur un travail d'équipe. Cependant, ces modèles n'ont pas changé la manière de produire de la connaissance économique par eux-mêmes. Avec les modèles, une nouvelle pratique scientifique a dû être établie : la modélisation macro-économétrique.

Cette thèse, dont l'objectif est de faire prévaloir l'importance de la macro-économétrie dans l'histoire de la macro-économie, s'articule autour de deux questions centrales : (1) Quelles ont été les forces et les objectifs qui ont motivé le développement de la modélisation macro-économétrique, et quelle était la nature des outils et des institutions que les macro-économistes ont construit pour observer, comprendre et contrôler l'économie d'après-guerre



aux Etats-Unis ? Et (2) quels ont été les conséquences de la construction et de l'utilisation de tels outils dans la production du savoir macro-économique ? En considérant Lawrence R. Klein comme une figure centrale, je parcours la discipline économique des années 1940-1950 en me focalisant sur l'intersection entre l'histoire de la macro-économie et celle de l'économétrie, et ainsi, je propose une nouvelle vision de l'économie du vingtième siècle en tant que discipline « saisie par les outils », dans laquelle la théorie (économique et statistique), l'application, l'expertise et la politique s'incorporent dans un même outil scientifique : le model macro-économétrique. J'expose donc l'histoire de la macro-économie non pas comme le produit de questions idéologiques monolithiques ou purement théoriques, mais plutôt comme le produit de visions épistémologiques et de stratégies de modélisation divergentes qui remontent aux débats entre les approches empiriques de la macro-économie étatsunienne et les méthodologies Walrasienne et Marshallienne. Ainsi, je soutiens la thèse que Klein a été le personnage principal dans la création d'une nouvelle manière de produire le savoir macro-économique, qui à travers la construction et l'utilisation d'outils complexes (modèles macro-économétriques), mis en place au sein d'une configuration institutionnelle spécifique (laboratoires économétriques), poursuivait des objectifs explicites de politique économique, et par laquelle les rôles bien définis des experts (équipes scientifiques) étaient intégrés à une nouvelle pratique scientifique : la modélisation macro-économique.

L'histoire que je présente dans cette thèse peut être lue à partir de trois points de vue différents. Premièrement, cette thèse peut être lue comme étant l'histoire d'une discipline proposant une réponse à une nécessité politique de contrôler l'économie des Etats-Unis dans le contexte précis de la période d'après-guerre. Cette réponse a consisté en l'adoption, l'adaptation, l'invention et la construction de nouveaux outils et techniques qui ont engendrés la création d'une nouvelle expertise scientifique, et ont, par conséquent, fait naître une nouvelle façon de comprendre le monde. Dans les grandes lignes, la réponse des économistes

aux difficultés de la période de l’après-guerre a conduit au développement d’outils spécifiques incorporés à l’intérieur d’institutions spécifiques qui leur ont permis de se rapprocher des pouvoirs politiques à travers une nouvelle pratique scientifique. En ce sens, cette thèse étudie les questions suivantes : Quel a été l’objectif que les modélisateurs macro-économétriques ont voulu atteindre avec ces nouveaux outils ? Une fois construits, qu’ont-ils réellement pu atteindre avec ces nouveaux outils pendant ces années enthousiastes ?

Deuxièmement, ma thèse peut être lu comme une histoire des conditions qui ont permis l’ascension de la modélisation macro-économétrique. En effet, cette thèse analyse l’état de la discipline économique et des connaissances statistiques et mathématiques de l’époque, ainsi que les configurations institutionnelles scientifiques, les sources de financement, la demande politique et le type de chercheurs qui ont rendu possible la construction et l’adoption de cette nouvelle pratique.

Enfin, cette thèse peut aussi être perçue comme une histoire intellectuelle de la modélisation macro-économétrique centrée sur Klein en tant que protagoniste de la construction et de la consolidation d’une pratique scientifique révolutionnaire. Outre la création d’une nouvelle pratique scientifique, Klein, grâce à sa détermination et sa force de persuasion, s’est continuellement battu pour la continuation de son programme économétrique qu’il considérait utile, puissant et nécessaire, non seulement pour contrôler l’économie, mais aussi pour la mise en place de réformes sociales.

*Structure de l’argument : La production de connaissances en macro-économétrie saisie par les outils*

Les objectifs principaux de la thèse sont de montrer que Klein était la figure centrale dans le développement de la modélisation macro-économétrique, ainsi que de présenter les effets de cette nouvelle pratique sur la production du savoir macro-économique. En ce sens, le titre de la thèse « l’économie saisie par les outils » fait référence au fait que la science économique de

la deuxième moitié du vingtième siècle a construit des institutions productrices de savoir qui s'articulaient autour d'outils scientifiques tels que les modèles macro-économétriques. En plus d'avoir incorporé des objectifs de politiques publiques et scientifiques dès leur création, ces institutions ont été configurées de telle sorte qu'elles puissent accueillir des équipes de chercheurs et experts, éléments essentiels pour cette pratique scientifique.

Cette thèse est divisée en deux parties, chacune desquelles étant composée de trois chapitres. Dans la première partie « Les années formatives de Klein », j'explore le parcours académique de Klein entre 1938 et 1959. Dans la deuxième partie de la thèse « la consolidation de la modélisation macro-économétrique », j'analyse l'opposition de longue date entre le programme d'économétrie structurelle, inspiré par Klein, et le programme d'économie statistique inspiré par Milton Friedman.

### *Les années formatives de Klein*

Dans le premier chapitre de la première partie, chapitre 2, j'analyse en particulier, la façon dont Klein a construit son identité en tant que macro-économètre, ainsi que la manière dont il a contribué à former la nouvelle pratique de la modélisation macro-économétrique. A travers de l'étude de la vie académique de Klein, j'examine le contexte intellectuel de la discipline économique pendant les années 1940 et 1950, et les relations que Klein a établi avec différents personnages et différentes institutions qui ont marqué le développement de sa propre identité en tant que macro-économètre.

Dans le chapitre 3, je considère le projet de Klein de « refaire » le travail macro-économétrique de Jan Tinbergen comme étant une réaction raisonnée et informée, découlant de l'analyse de la critique que John Maynard Keynes (1938 ;1939) avait fait des travaux de l'économiste hollandais. En 1944, seulement cinq ans après que la controverse Keynes-Tinbergen eût eu lieu, Klein se trouvait dans une position doublement fascinante : d'une part,

Klein était considéré comme l'un des experts sur la théorie keynésienne, puisqu'il venait de finir sa thèse sous la direction de Paul A. Samuelson sur la « Révolution Keynésienne ». Mais d'autre part, il venait d'être recruté par Jacob Marschak à la Cowles Commission avec l'objectif clair de refaire le modèle de l'économie étatsunienne que Tinbergen avait préparé à la fin des années 1930 pour la Ligue des Nations. De par cette position paradoxale, Klein a dû surmonter la tâche difficile de réconcilier le monde Tinbergenien qui cherchait à implémenter des outils techniques et rigoureux pour en tirer des inférences, avec le monde Keynésien qui montrait une aversion marquée pour ce genre d'outils techniques, mais qui défendait la nécessité du travail empirique.

Le chapitre 4 se concentre sur la manière distinctive que Klein avait de faire de l'économétrie. En me focalisant sur la période de Klein en tant que chercheur à la Cowles Commission (1944-47), ce chapitre analyse une série de publications fondamentales et d'événements qui ont été décisives dans la formation de son image sur l'économétrie. En particulier, le chapitre montre que l'adoption d'une méthodologie suffisamment flexible et orientée vers la pratique, ainsi que son soutien du pluralisme dans l'utilisation des théories, ont été le résultat de sa participation dans la construction de modèles empiriques à proprement parler. De plus, cette approche plus flexible de Klein qui découle de ses travaux appliqués et orientés vers la pratique, contraste avec la méthodologie prescriptive de ses collègues de la Cowles Commission qui découle de leurs travaux théoriques où l'abstraction joue un rôle très important. Ce chapitre conclue que son image de l'économétrie aurait permis à Klein, non seulement d'enrichir le processus de spécification des modèles, mais aussi de poursuivre son programme macro-économétrique au-delà des années 1940s, et durant toute sa vie, de conserver son optimisme par rapport à l'utilisation de ces outils macro-économétriques pour atteindre des objectifs de contrôle de l'économie et de réforme sociale.

*La consolidation de la modélisation macro-économétrique*

Dans la deuxième partie de la thèse « la consolidation de la modélisation macro-économétrique », j'analyse l'opposition de longue date entre les programmes en économétrie structurelle inspiré par Klein et en économie statistique inspiré par Milton Friedman. Suivant l'idée de Theodore Porter (1994) qui décrit l'histoire moderne de la discipline économique étatsunienne comme étant fondamentalement une histoire d'idées concurrentes de quantification, plutôt qu'une histoire de théories ou d'idées économiques opposées, ce chapitre se concentre sur l'opposition entre les approches empiriques en macroéconomie représentées par le National Bureau of Economic Research et la Cowles Commission.

Le chapitre suivant, en prenant la division entre Marshall et Walras comme point de départ des débats méthodologiques entre les deux approches empiriques en macroéconomie, tente de démontrer que l'introduction de l'économétrie et la transformation de l'économie vers une discipline saisie par les outils à partir des années 1940-1950, a changé les relations entre la théorie économique, l'économie appliquée et la politique économique. Dans ce chapitre, j'insiste sur le fait que plutôt d'être un moyen de combler les écarts entre théorie et données, la macro-économétrie aurait transformé radicalement la prééminence de la théorie sur l'application, les données, et les questions de politique économique. En opposition à la pratique économiste, la pratique macro-économétrique de la deuxième partie du vingtième siècle (qui implique l'adhésion à l'outil économétrique) ne dissocie pas la théorie, l'application et les données, mais au contraire, combine et fusionne ces éléments en un seul system de raisonnement ou pratique scientifique : la modélisation macro-économétrique.

En d'autres termes, je propose une nouvelle perspective sur la division entre Walras et Marshall, dans laquelle j'insiste sur le fait que leur différence n'est pas fondamentalement basée sur une opposition entre la théorie de l'équilibre générale et la théorie de l'équilibre partiel, mais découle en fait d'une opposition méthodologique entre deux stratégies de

modélisation.

Dans le chapitre 6, j'expose de façon précise ces divergences méthodologiques au travers de l'étude d'un évènement historique que je considère clé, à savoir le débat entre Friedman et la Cowles Commission. Je soutiens la thèse que ces divergences méthodologiques ne sont pas seulement basées sur l'utilisation de différentes méthodes statistiques, théories économiques, ou bien d'idéologies politiques, mais sont le résultat d'une divergence de principes méthodologiques et de stratégies de modélisation utilisées par les macro-économètres pour comprendre et représenter le monde, ainsi que pour résoudre des problèmes concrets.

Malgré l'incapacité de l'économiste à observer et comprendre le système économique dans toute sa complexité, l'approche Walrasienne de la Cowles (suivie par Klein) considère systématiquement l'économie dans son intégralité. Par contre, l'approche Marshallienne du National Bureau, particulièrement celle de Friedman, tente de prendre en compte cette limitation et considère le modèle économique comme un outil visant à construire des systèmes de pensée tout en se basant sur l'observation de sous-parties réduites de l'économie.

Dans le chapitre 7, je présente la manière dont la controverse entre Milton Friedman, Gary Becker, et Lawrence R. Klein durant les années 1957-1958, a contribué à l'évaluation de la performance des modèles macro-économétriques. En effet, durant cette période, le discours de Friedman et Becker soulève d'importants points, particulièrement celui concernant l'inaptitude des modèles macro-économétriques à considérer la fonction de consommation et prédire le produit national, et dont la cause est l'implémentation d'un « faux » critère pour juger la performance des modèles.

Alors que les macro-économètres considéraient les extrapolations faites sur la bases des formes réduites pour évaluer la performance de leurs modèles, Friedman et Becker étaient convaincus que performer des simulations à échelle globale était la démarche à suivre pour sélectionner les modèles les plus appropriés. Mis à part le ton critique utilisé par Friedman et

Backer, leur argumentation peut aussi être vue comme une critique constructive et pionnière mettant en avant l'importance d'une sélection plus rigoureuse de critères d'évaluation de performance de modèles, qui par la suite donnera naissance à une pratique devenue courante et systématique chez les macro-économètres, et aujourd'hui connue comme « full- or dynamic model simulation ».

En conclusion, dans cette dissertation, je défends la thèse que la transformation de la macroéconomie de l'après-guerre a conduit au développement et à l'instauration de nouvelles pratiques scientifiques empiriques, et en particulier des pratiques de modélisation quantitative, qui ont conduit à l'adoption et à l'adaptation d'outils scientifiques ayant pour but de produire un savoir économique rigoureux sur la base à la fois du travail d'équipe et du jugement des experts, et dans laquelle les sphères de la théorie (économique et statistique), celle de l'application et celle de la politique économique ne sont pas séparables.

Etant donné que les observations macroéconomiques ne peuvent pas être menées dans un laboratoire, mais qu'elles découlent nécessairement du « monde réel », ce qui exclu la formulation d'une théorie exacte et explicite, l'objectif de l'introduction de méthodes mathématiques et statistiques n'était pas de fournir une 'boule de cristal' pour prévoir de futures évènements économiques, mais plutôt, de développer un processus de raisonnement systématique et une pratique scientifique, qui permettraient produire un savoir rigoureux au sein d'une institution et qui donneraient une place très importante au rôle des experts que ce soit au niveau individuel et collectif (au sein d'une équipe). De ce point de vue, la modélisation macro économétrique à grande échelle développée par Klein peut être considérée comme la pratique la plus importante introduite dans la période de l'après-guerre.

*L’apport historiographique*

Je considère ma thèse comme étant une contribution à l’historiographie dans deux sens différents. D’un côté, l’historiographie constitue le corpus de connaissances de l’histoire de la pensée économique et, dans le cas particulier de cette thèse, le corpus de connaissances de l’histoire de la macroéconomie et de l’économétrie. En me focalisant sur *l’histoire de la macro-économétrie*, ma thèse surmonte les frontières artificielles mises en place par certains historiens entre l’économétrie et la macroéconomie, et offre une compréhension plus riche de l’histoire de deux disciplines en rapprochant l’histoire de la pensée économique des pratiques de macro-économistes et des économètres de l’époque. En me focalisant sur Klein et sur le développement de la modélisation macro-économétrique en tant qu’objets principaux d’étude, ma thèse comble certains écarts de l’histoire qui n’avaient pas encore été étudiés systématiquement. Dans le deuxième sens de l’historiographie, l’autre contribution de ma thèse est la proposition d’une méthode par laquelle étudier l’économie des années 1940-1950, méthode informée philosophiquement, et basée sur une histoire concrète et matérielle dont des personnages réels et les objets scientifiques en sont les protagonistes, et qui, je le souhaite, servira d’inspiration pour d’autres études de cette nature.

Enfin, les résultats de ma thèse me permettent de faire trois affirmations claires sur l’historiographie de la macroéconomie et de l’économétrie : (1) une nouvelle histoire de la macroéconomie devrait adopter une approche qui mettrait l’accent sur la nature empirique de la macroéconomie et sur les pratiques des macro-économètres, en considérant des questions méthodologiques, historiques, théoriques et épistémologiques dans le but de proposer une histoire de la *discipline*, plutôt qu’une histoire des *théories* ou des *idées*. Dans ce sens, (2) une histoire de l’économétrie devrait se concentrer sur la construction, l’adaptation et l’application des méthodes et de théories, et sur les conditions scientifiques, sociales, intellectuelles, économiques et historiques qui ont rendu possible la construction et



l'application de méthodes économétriques. Finalement, (3) ces nouveaux types d'histoire de la macroéconomie et de l'économétrie devraient forcément considérer Lawrence R. Klein comme l'une des figures centrales dans leurs analyses et dans leurs narratives. En effet, le type de modélisation économétrique que Klein a créé et façonné a par la suite dominé les pratiques empiriques des macroéconomistes durant plus de trente ans, entre les années 1940-1970.





## Economics as a “tooled” discipline: Lawrence R. Klein and the making of macroeconometric modeling, 1939-1959

In this dissertation, I place macroeconometric modeling at the center of the history of twentieth century macroeconomics, i.e. as a history of *macroeconometrics*, and ask two central questions: (1) What exactly were the objectives and the forces driving the development of macroeconometric modeling, and what kind of tools and institutions did macroeconomists build to observe, understand, and control the US postwar economy? (2) What were the effects that the construction and use of these tools had on the production of macroeconomic knowledge? Taking Lawrence R. Klein as a vehicle, I travel across the economics discipline of the 1940s and 1950s, and study the intersection between the history of macroeconomics and the history of econometrics, providing a new understanding of twentieth century economics as a “tooled” discipline, in which theory (economic and statistical), application, expertise, and policy become embedded within one scientific tool: a macroeconometric model. Consequently, I present the history of macroeconomics not as the product of monolithic ideological and purely theoretical issues, but rather of divergent epistemological views and modeling strategies that go back to the debates between US-Walrasian and US-Marshallian approaches to empirical macroeconomics in which macroeconometric modeling forms the heart of macroeconomics. My thesis is that Klein was the most important figure in the creation of a new way to produce scientific knowledge that consisted in the construction and use of complex tools (macroeconometric models) within specific institutional configurations (econometric laboratories) and for explicit policy and scientific objectives, in which well-defined roles of experts (scientific teams) were embodied within a new scientific practice (macroeconometric modeling).

**Keywords:** Macroeconometric modeling, history of macroeconomics, history of econometrics, history of recent economics, economic methodology, Lawrence R. Klein.

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**Mots clés :** Modélisation macro-économétrique, histoire de la macroéconomie, histoire de l'économétrie, histoire de la pensée économique récente, méthodologie économique, Lawrence R. Klein.

