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José Miguel Edwards

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JOYFUL ECONOMISTS

REMARKS ON THE HISTORY OF ECONOMICS AND PSYCHOLOGY FROM THE HAPPINESS STUDIES PERSPECTIVE

Thèse pour l'obtention du doctorat en sciences économiques
(Arrêté du 30 mars 1992)
Présentée et soutenue publiquement le 14 décembre 2009 par

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L'Université Paris I Panthéon-Sorbonne n'entend donner aucune approbation ni improbation aux opinions émises dans cette thèse. Ces opinions doivent être considérées comme propres à leur auteur.

PREFACE

The following is a revised version of the dissertation examined December 14, 2009 at the *Maison des Sciences Économiques* of the University of Paris I. I am deeply thankful of professor Annie L. Cot, the supervisor of this project, and of the Jury composed by professors Annie L. Cot, Richard Arena, Philippe Fontaine, Jérôme Gautié, D. Wade Hands and Harro Maas for supporting it, as well as for their valuable remarks and comments. Professors Richard Arena and D. Wade Hands provided written reports prior to the final examination, of which professor Jérôme Gautié was the president.

This version contains minor revisions throughout the script, some minor changes in the transitions between the chapters, and substantial changes in the third section of the introductory chapter and in the general conclusion. I must thank Richard Arena for his valuable comments on these parts of the dissertation. The main aim of modifying these parts was to further insist on the change of perspective between Part I of the dissertation on one hand, and Parts II and III on the other. I also added the five-minute-pause page of Gustav Mahler's Second Symphony between Parts I and II (and a brief explanation of this inclusion) in order to remind the reader of the important shift at that point in the dissertation. I must thank Harro Maas for suggesting the unexpected (though welcomed) analogy between the structure of Mahler's great piece of music and that of this modest work on the history of economics and psychology. I must also thank Kendra Walker for reading the modified parts, as well as Parts I and II of the previous version of the script, for this substantially improved the quality of the writing (which can be verified by comparing them to Part III). I am naturally the only one responsible of the numerous errors remaining.

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This dissertation is the product of four years of research work and discussion in the subfield of the history of economics. The whole dissertation project has been repeatedly presented and discussed in the Albert O. Hirschman (AOH) seminar organized in Paris by professors Annie L. Cot and Jérôme Lallement. I am very thankful to them and to all of the other participants of the AOH seminar. Early versions of chapters 1, 3, and 4 were presented at the 34th, 35th and 36th meetings of the History of Economics Society (HES) and supported by the young scholar program of the HES. I benefited from useful reports and comments by, or discussions with, Mary S. Morgan, D. Wade Hands, André Van Hoorn, Floris Heukelom, Harro Maas, Neil Nimand, and Pedro Duarte. Chapters 3, 5 and 6 are now the result of extensive discussions on papers presented at the 2nd and 3rd conferences on the History of Recent Economics (HISRECO). I am also very thankful to the participants of these conferences: Philippe Fontaine, Roger Backhouse, Tiago Mata and Guido Erreygers (the organizers), and D. Wade Hands, Dan Hammond, Evelyn Forget, Marcel Boumans, Ross Emmett, Edward Nik-Khah and Steven Medema for their comments, which positively influenced this dissertation. First versions of papers that became chapters 3, 5 and 6 were presented in the 10th and 11th Summer Institutes on Economic History, Philosophy, and History of Economic Thought in France. An early version of chapter 3 was also presented at a session of the *Atelier de Philosophie et Économie* (PHARE). Financial and institutional support was granted by the University of Paris I (*Allocation de Recherche, Monitorat, ATER*), GRESE, PHARE, and the *École Doctorale d'Économie Panthéon-Sorbonne*.

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RESUME : ETUDE DES RELATIONS HISTORIQUES ENTRE ANALYSE ECONOMIQUE ET ANALYSE PSYCHOLOGIQUE DU POINT DE VUE DE « L'ECONOMIE DU BONHEUR » I

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GENERAL INTRODUCTION

A History of Economics and Psychology from the Happiness Studies Perspective

0.1. The Economics of Happiness as a “viewpoint”

This dissertation is based on the recent emergence of “The Economics of Happiness” and begins by the presentation of its main traits and historical development (1). As we shall see, this development can be considered within the context of two different histories: the history of the relationship between economics and experimental psychology (2), and the history of the economic use of subjective data (3). The dissertation takes the economics of happiness as a viewpoint to revisit these events. In doing so, it reconstructs part of the history of economics and psychology and offers an alternative account of the economics of happiness than the ones currently advanced within the subfield.

(1) The economics of happiness is characterized by the analysis of subjective well-being data by economists, which has its origins in Richard Easterlin’s 1974 paper “Does Economic Growth Improve the Human Lot? Some Empirical Evidence”. During the twenty-five years following Easterlin’s paper, the economic analysis of happiness emerged slowly and joined established criticisms of economic theories of consumer behavior and welfare. It shared the scene with the analysis of consumer behavior in affluent contexts, of which John Kenneth Galbraith’s work is the most conspicuous example. It was in this context that Tibor Scitovsky wrote *The Joyless*

Economy (1976), an inquiry exploring the welfare effects of consumption, and Fred Hirsch introduced the concept of positional goods in the *Social Limits to Growth* (1977). Richard Layard's study on "Human Satisfaction and Public Policy" (1980) and Robert Frank's "Frames of Reference and the Quality of Life" (1989) explored the role of contexts in consumer behavior and proposed policy measures to correct the effects of social comparisons on social welfare. This literature was based on the idea that the standard versions of consumer choice theory and welfare economics were inadequate for the analysis of consumer behavior in affluent contexts.

The empirical analysis of happiness data was modest during the early development of the subfield, with the exception of a few attempts, namely in David Morawetz *et al.*'s analysis of "Income Distribution and Self-Rated Happiness" (1977), and in Richard B. Freeman's use of "Job Satisfaction as an Economic Variable" (1978). The economic analysis of happiness shares its origins with the analysis of "job satisfaction" by labor economists. David Blanchflower and Andrew Oswald's study of "Entrepreneurship, Happiness and Supernormal Returns" (1992) and Andrew Clark and Andrew Oswald's analysis of "Satisfaction and Comparison Income" (1996) are examples of the use of job satisfaction as a variable for happiness research. Another important trend in the early development of the economics of happiness was present in the work of Bernard van Praag, Paul Frijters, and their associates at Leyden University in the Netherlands. Started during the 1970s, the Leyden approach was focused on subjective evaluations of incomes.

Unlike the early literature, recent developments in the economics of happiness are marked by intensive studies of subjective well-being data on life satisfaction. These data are general. They are not restricted to particular domains such as job satisfaction or income satisfaction, but consist in answers to questionnaires asking "How satisfied are you with your life as a whole these days?", usually scaled from 1 ("completely dissatisfied"), to 10 ("completely satisfied"). Another trait of recent developments in the economics of happiness is the production of historical analyses justifying the subfield. These reconstructions are normative, for their main objective is to expand happiness studies in economics.

An EconLit search for references containing either "happiness", "life satisfaction" or "well-being" in the title gives over 400 items from 1974 to 2009. This literature is the object of Part I of the dissertation, which maintains

that the history of the economics of happiness has been greatly influenced by the foundation of “Hedonic Psychology” in 1999. Since that episode, the economists’ subfield has been developed with consideration for the psychologists’ analysis of happiness rather than in opposition to consumer choice theory or welfare economics. The history of the economics of happiness is thus read in two periods: the first, from Easterlin’s publication in 1974 to the establishment of hedonic psychology (1999); and the second, from the turn of the century onwards (1999-2009).

Parts II and III of the dissertation take the history of the economics of happiness as a viewpoint to explore the history of the relationship between economics on one hand, and both experimental and social psychology on the other. This choice is based on the idea that the passage from early studies of happiness by economists (1974-1999) to the recent development of the subfield (1999-2009) can be explained by two important shifts: in the psychological assumptions supporting the program, and in the intensiveness of the use of happiness data.

(2) While the early economics of happiness analyzed consumer behavior in affluent contexts and was directed against consumer choice theory, the recent approach not only promotes the use of subjective well-being data as proxies for utility, but also models consumer behavior in terms of utility functions.

Tibor Scitovsky’s *The Joyless Economy* (1976) is a good example of the early literature. Rather than taking preferences as a given, it explored the formation of the wants and needs of affluent consumers. These wants and needs were considered the outcome of external factors rather than the expression of internal states as revealed by choice behavior. The criticism of mainstream economics made by Scitovsky and the other early economists of happiness was mainly directed against the principle of consumer sovereignty and based on views of consumer behavior which relied on experimental psychology. According to these views, the behavior of an organism was the outcome of its interactions with the environment, and was controlled by the history of these interactions. Based on these “behavior control” accounts, early economists of happiness were interested in external factors involved in the formation of consumer preferences. The economic policies formulated by these economists, Frank’s (1989) for instance, involved institutional changes

designed to shape preferences in order to counter the negative welfare effects of positional (social) forms of consumption.

Unlike the early economic studies of happiness, recent developments in the subfield are congruent with the psychological foundations of mainstream economics. Along with the recent development of behavioral economics as a mainstream subfield, the economics of happiness positions itself as a complement to choice theory rather than as an alternative to it. In Clark, Frijters and Shields' (2008) words, the economic analysis of happiness data is a "valuable alternative, but complementary, approach to the revealed-preference framework that dominates the discipline of economics" (Clark *et al.*, 2008, p. 136).

(3) The other important distinction to make between the early and the recent economics of happiness lies in the use of subjective well-being data. While the early literature (1974-1999) evolved from theoretical discussions supported by only a few empirical studies, the current subfield is characterized by the intensive use of life satisfaction data. This difference is less related to the history of economic theory than to the history of the economic use of survey data in general and subjective data in particular. The history of the economics of happiness is then equally a viewpoint from which the dissertation explores the history of the use of such data, which is another important part of the history of the relationship between economics and psychology.

In what regards the subfield of the history of economics, the dissertation places itself among three approaches: the history of happiness in economics, the history of psychology in economics, and the history of the economic use of subjective data. Section 0.2 presents these three approaches and Sections 0.3 and 0.4 introduce the main theses and the outline of the dissertation.

0.2. Two normative and one standard reconstruction

During the last years, the economics of happiness has started to fill the pages of economic journals and volumes supported by leading publishers. "Happiness is back" (Bruni, 2004a), or *Happiness: A Revolution in*

Economics (Frey, 2008), are common labels used by economists of happiness to qualify the evolution of their subfield. Two different types of historical reconstructions are being written. On one hand (0.2.1), they picture the development of the subfield as a recovery of the analysis of happiness in economics, and deal with what the economists' happiness should be. These accounts are closely related to standard reconstructions of the history of economics and psychology (0.2.2). On the other (0.2.3), they deal with the history of the economic use of subjective data and present the emergence of the subfield as a challenge to economists' historic skepticism of such an approach.

0.2.1. Happiness: in, out, back

The history of happiness *in* economics is the specialty of Italian historians of economics of which Luigino Bruni and Pier Luigi Porta are the main figures. The first striking feature of this work is that it inserts the short history of the economics of happiness (1974-2009) in long reconstructions of the study of happiness in (and out of) economics. Bruni's publications (2004a, 2004b, 2006) are a clear example of this kind of analysis. They explore the meaning of the concept of happiness in the history of economics and trace its origins back to the ancient and classical traditions. Bruni's reconstructions present different histories of happiness in economics, and place the new subfield in relation to them (1 and 2). Bruni and Porta's *Handbook on the Economics of Happiness* (2007) introduces the subfield according to this same kind of approach (3).

(1) The first history Bruni presents is that of the classical approach of Adam Smith [1723-1790] and the British tradition on one hand, and the French and Italian classical traditions on the other. Like "Smith the philosopher", the author of the *Theory of Moral Sentiments*, the continental tradition would have produced moral philosophies promoting the development of interpersonal relations and social virtues¹. Consequently, in regards to the

¹ Bruni presents the continental tradition by means of the writings of the Napolitan priest Antonio Genovesi [1713-1769].

happiness concept, these two approaches would be in line with the ancient (Aristotelian) notion of “happiness as *eudaimonia*” (Bruni, 2004a, p. 23). However, unlike “Smith the economist”, the continental tradition would have taken the pleasures of social relations as the main benefits of living in society. Instead of specialization, or gains in productivity, Italian classical writers, such as Antonio Genovesi, would have written about public happiness (*felicità pubblica*) (*ibid.*, pp. 25-28).

Unlike Genovesi, British economists would have abandoned the analysis of public happiness for that of the wealth of nations. This shift is presented as a radical change in regards to the object of political economy: from happiness, to wealth.

“Smith’s political economy (and the whole classical paradigm) could have become something completely different: instead of being defined as the science of wealth, political economy could have been defined as the study of how and under which conditions riches could be transformed into happiness [...]. The tradition of economics after Smith [...] forgot the very complex and slippery relationship between wealth and happiness, as the enthusiasm for the ‘novelty’ of contemporary paradox of happiness signals.” (*ibid.*, p. 32)

(2) Bruni’s history of happiness in economics continues with the analysis of “happiness-pleasure” promoted by Jeremy Bentham [1748-1832] and his followers. These authors are held responsible for neglecting the “technology of happiness” (*ibid.*, p. 35) and giving a methodological turn “to the concept of happiness in economics” (*ibid.*)²:

“If we look carefully at Bentham’s idea of happiness we immediately see that in his system happiness is equal to ‘pleasure’ [...]. With Bentham the distinction between end (happiness) and means (wealth) disappeared, happiness also became the direct end of economic actions, and meant pleasure. Bentham’s approach to happiness, therefore, is far

² Bruni’s argument is based on the concept of “technology of happiness” by which he means the analysis of the relation between wealth and happiness. According to Bruni, the analysis of the happiness of nations was not completely absent in Britain. It was explored by economists of the Cambridge tradition, namely, Malthus [1766-1834] and Marshall [1842-1924]. In Bruni’s terms: “Malthus belongs to those economists (Sismondi, Genovesi, and many Italians) who thought that the ‘happiness of nations’” was “another inquiry, however, perhaps still more interesting than that of wealth” (Bruni, 2004a, p. 24). Malthus’ approach was continued by Marshall and Pigou (*ibid.*, p. 33), and the analysis of the “Happiness Transformation Problem” (Bruni, 2004b) would make the “bridge between the classical reflection on happiness in the eighteen century and the recent debates on the ‘paradoxes of happiness’” (Bruni, 2004b, pp.450-451).

from *both* Aristotle's (and Genovesi's) *eudaimonia*, and from Smith and the classics who kept the distinction between happiness (the final end) and wealth. Bentham's methodological project, as is well known, nurtured economics thanks mainly to the works of Jevons and Edgeworth." (*ibid.*)

Following Bentham's ideas, Jevons [1835-1882] and Edgeworth [1845-1926] would have transplanted utilitarian hedonism to economics turning the discipline into a "science of happiness-pleasure" instead of a science of wealth. With Jevons, "happiness-pleasure" became "central in neoclassical economics" (*ibid.*, p. 36):

"The reductionism of happiness/eudaimonia to utility/pleasure is the real break-point in this history of happiness in economics." (*ibid.*, original emphasis)

Shortly after this break-point, the "ordinalist turn" in economics would have led to the abandonment of psychology, and, in turn, of the notion of happiness-pleasure. Even if the ordinalists retained the theoretical structure of the marginalism of Jevons and Edgeworth (*ibid.*, p. 38), Bruni claims that they abandoned psychological introspection. This supposedly marks a turning point in both the history of the interrelationship of economics and psychology, and the history of happiness in economics:

"Up to Pareto's shift to rational choice theory and the birth of the ordinal and not hedonistic utility theory, economists did revert to psychology; Edgeworth, as well as Jevons, Pantaleoni and all the hedonist economists of the first generation of marginalism did not abstain from psychological introspection in order to understand the reality behind curves and formulas [...]. From the behavioral economists' point of view, the 'Paretian turn' in rational choice theory was where economics went wrong." (*ibid.*, p. 38)

Whereas Jevons and Edgeworth relied on psychology, Pareto's "followers" (Hicks, Allen, and Samuelson) would have taken the wrong path. According to Bruni's reconstruction, the economics of happiness represents a return to the marginalism of Jevons and Edgeworth. It is supported by "hedonism", "interrogates" people and uses "psychological data" (*ibid.*, p. 39). For Bruni, however, this happiness "is not the 'classical' happiness but the utilitarian 'pleasure'" (*ibid.*). Contemporary analysis of happiness would be "closer to Bentham and Jevons than to Genovesi or Malthus" (*ibid.*):

"there are more histories of happiness potentially available in economics than usually recognized. The three main lines are the classical (from Aristotle to Genovesi), the classical English, and the hedonist-Utilitarian [...]. Historical analyses show that the

contemporary debate on happiness is a return to the classic line (happiness is not wealth), but happiness remains individualistic and hedonistic: *eudaimonia* and *publica felicità* are the great absences here. This is a shame, because it is my conviction that economic (and social) theory can be enriched by a recovery of these elements of classical happiness.” (*ibid.*, pp. 39-40)

(3) Bruni and Porta’s *Handbook on the Economics of Happiness* (2007) starts with a section entitled: “Economics and happiness: a new field with a long history” presenting the subfield as the “process of rediscovery of happiness in economics” (Bruni and Porta, 2007, p. xiv)³. It claims that “the wealth-happiness nexus was central in the classical tradition” (*ibid.*), and suggests that, with Richard Easterlin (1974) and Tibor Scitovsky (1976), the paradox of happiness returned to economics “re-echoing economic science from its classical origins” (*ibid.*).

It is important to point out that despite the acknowledged fact that economists of happiness are not really interested in philosophical or even technical definitions of happiness, Bruni and Porta’s reconstruction remains focused on the analysis of the happiness concept and built around the distinction between *eudaimonic* and *hedonic* happiness (*ibid.*, p. xiii)⁴. The economists’ happiness concept is compared to Amartya Sen’s capabilities approach, as well as the psychologists’ happiness⁵. Furthermore, the economic analysis of subjective well-being data is placed far from the “*eudaimonistic* tradition”:

³ Bruni and Porta’s introduction to the *Handbook* is composed by the following sections: “1. Economics and happiness: a new field with a long history”, “2. The paradox of happiness”, “3. What is happiness”, “4. Explanations for the Easterlin paradox”, and “5. The present volume”. The *Handbook* itself is less a handbook than a collective volume based on the contributions to a “Conference on The Paradoxes of Happiness in Economics” held in Milan (2003). The twenty-four chapters of the volume are divided in the following four parts of six chapters each: “I. Lessons from the Past”, “II. Understanding the Paradox of Happiness”, “III. Relational Goods”, and “IV. Data and Policies”. Something similar happens with Bruni and Porta’s (2005) *Economics and Happiness*, the introduction of which starts by a section on “The Return of Happiness in Economics” (Bruni and Porta, 2005, p. 1).

⁴ “Economists do not even like the question: ‘what is happiness?’. To them happiness is not a concept clearly distinct from pleasure, satisfaction, or welfare [...]. Happiness, by economists, is not generally *defined* but empirically *measured*, on the basis of the answers to questionnaires that ask people: ‘how happy are you?’.” (*ibid.*, p. xvii)

⁵ Psychologists, they write, “use the expression ‘happiness’ with more precision than economists [...], happiness is considered to be a *narrower* concept than SWB [subjective well-being], and different from life satisfaction [...]: SWB is a synonym of ‘being happy’, a concept close to the Aristotelian approach to happiness as *eudaimonia*, whereas concepts

“We want to stress right from the beginning that the economists working today on the ‘happiness paradox’ are generally far from the *eudaimonistic* tradition. [...] happiness, in order to be a proxy of a good life, must be translatable into human flourishing (*eudaimonia*), in terms of capabilities and functioning [sic], human rights and freedom.” (*ibid.*, p. xx)

After reviewing “the most common explanations of the Easterlin paradox” (*ibid.*), Bruni and Porta show that the economics of happiness does not “refer to sociality as a source of happiness *per se*” (*ibid.*, p. xxvi). They declare that the happiness they are interested in is “*relational* happiness” letting their readers know that their reconstruction is normative, as it is focused on what they think the economists’ happiness should be. They emphasize the “importance of Aristotle’s theory of *eudaimonia*”, which they consider “worthwhile examining in greater depth” (*ibid.*, p. xix)⁶.

0.2.2. Psychology: in, out, back

As noted above, there are important elements in the history of the relationship between economics and psychology which intervene in the development of the economics of happiness. This seems to explain why the history of happiness in economics (as presented above) draws on standard accounts of the history of the relationship between the two disciplines⁷. These accounts tend to explain this history in three stages: a common origin in moral

such as ‘satisfaction’ and ‘happiness’ belong to ‘feeling’ happy.” (*ibid.*, p. xvii, original emphasis).

⁶ The first part of the *Handbook* consists indeed in “six chapters of a historical nature, highlighting the contributions that old philosophies made to the modern concept of economic happiness” (Bruni and Porta, 2007, p. xxv).

⁷ This is the case of Graham’s entry on the economics of happiness in the *New Palgrave’s* 2008 edition: “Early economists and philosophers, ranging from Aristotle to Bentham, Mill, and Smith, incorporated the pursuit of happiness in their work. Yet, as economics grew more rigorous and quantitative, more parsimonious definitions of welfare took hold. Utility was taken to depend only on income as mediated by individual choices or preferences within a rational individual’s monetary budget constraint [...]. The study of happiness or subjective well-being is part of a more general move in economics that challenges these narrow assumptions. The introduction of bounded rationality and the establishment of behavioral economics, for example, have opened new lines of research. Happiness economics – which represents one new direction – relies on more expansive notions of utility and welfare, including interdependent utility functions, procedural utility, and the interaction between rational and non-rational influences in determining economic behavior” (Graham, 2008, pp. 1-2).

philosophy, a separation with the ordinalist turn, and a reunion with the development of behavioral economics during the late twentieth century. The focus is on “psychology” (not “psychologies”) as being “in” or “out” of economics (Hands, 2009, p. 2).

It is worth noting that much of the history of economics and psychology has been written as a by-product of research on economic subjects. It appears mainly in introductory surveys to studies about the recent development of behavioral economics (Rabin, 1998; Sent, 2004), experimental economics (Bruni and Sugden, 2007), the economics of happiness (Bruni, 2004a), or game theory (Giocoli, 2003). It offers summary reconstructions of the history of the two disciplines (especially short concerning psychology) and tends to provide only “thin” accounts of entities such as “behaviorism”, or “introspection”⁸.

A clear example of this kind of account are Bruni and Sugden’s (2007) introductory sections to a paper on “Pareto’s reformulation of choice theory”, which also deals with “current controversies about the status of behavioural economics” (Bruni and Sugden, 2007, p. 146). Wilhelm Wundt’s work and the marginal revolution in economics are used to explain the relationship between economics and psychology during the late 19th century:

“Wilhelm Wundt [...] is generally regarded as the first experimental psychologist. Wundt’s most influential book, *Grundzüge [sic] der physiologischen Psychologie* (1874) – published at the moment of the marginal revolution in economics – proposed the scientific investigation of people’s introspections about their experiences of consciousness.” (*ibid.*, p. 151)

This account leads the authors to the following statement:

“In understanding the relationship between psychology and economics at this time [the 1870s], it is important to recognize that, in both disciplines, introspection was treated as a legitimate source of data. The behaviorist movement in psychology, which denied the status of introspection, dates only from the second decade of the twentieth century.” (*ibid.*)

As will be shown further in the dissertation, this oversimplified view of the role of introspection in psychology and economics leads to a poor account of the impact of behaviorism in the two disciplines. Of course Bruni and Sugden’s (2007) paper does not intend to provide detailed accounts of

⁸ Sent (2004), for instance, presents an admittedly “crude overview of the historical connections between economics and psychology” (Sent, 2004, p. 740).

“introspection” or “behaviorism”. The problem is that such detailed accounts tend to be absent from the history of economics despite their importance to the understanding of the subject.

According to the standard reconstruction of the history of economics and psychology, economics was able to escape psychology by ridding itself of mentalism and focusing instead on the observation of economic choices. This move is often labeled “behaviorist” not only by economists but also by historians of economics⁹. This is essentially wrong. Lewin’s (1996) “Historical Synopsis” introducing a paper entitled “Economics and Psychology: Lessons for Our Own Day from the Early Twentieth Century” presents “behaviorist mainstream economics” in the following terms:

“A behaviorist movement arose in economics, as theorists attempted to free economics of all psychological elements. This movement contributed to the replacement of the older theory of cardinal utility, with the new notion of ordinal preferences.” (Lewin, 1996, p. 1295)

The idea economists have of behaviorism seems to be a direct consequence of the way in which the history of economics and psychology has been written. Presented in introductory surveys, it devotes too much attention to economics and does not allow for the integration of a psychological perspective¹⁰. It

⁹ This is the case of Lewin (1996), Asso and Fiorito (2003, 2004b), Easterlin (2004), Kahneman and Sugden (2005), Layard (2005), Angner and Loewenstein (forthcoming) and many other economists and historians of economics. Asso and Fiorito (2004b), for instance, write about “the emergence in the mid-1930s of a ‘behaviorist mainstream economics’ (Lewin, 1996)” out of the work of Eugene Slutsky (1915), John Hicks and Roy Allen (1934), and Paul Samuelson (1938). They show that “such a shift in attitude among mainstream economists” is strongly indicated in “Frank Knight’s famous critique of the ‘Slutsky School’ in demand theory (Knight, 1944)” (Asso and Fiorito, 2004b, p. 465). Richard Layard’s *Happiness: Lessons from a New Science* (2005) presents the concept of gross national product (GNP) showing that economics was “captured” by behaviorism in the 1930s: “The [GNP] concept was developed in the 1930s for a very good purpose – to help think about fluctuations and unemployment, and it has been crucial in the effort to control boom and bust. But very quickly it got hijacked to become a measure of national welfare [...]. This hijacking was inevitable once economics had been captured by behaviourism in the 1930s. It is actually a rather sorry tale. In the late nineteenth century most English economists thought that economics was about happiness [...]. Their system was not fully operational, but it was a forward-looking agenda. It was also in tune with late nineteenth century psychology like that of William James, who was actively studying the strength of human feelings. Then psychology turned behaviourist. [...] behaviourism became the intellectual climate, and in the 1930s it took over economics. This led to a much narrower concept of happiness” (Layard, 2005, p. 133).

¹⁰ It is worth noting that Lewin’s “synopsis” is often quoted in the literature despite the fact that the core of the paper argues *against* the idea of a “behaviorist” mainstream economics: “the theoretical practice of ‘behaviorists’ such as Samuelson contradicted their own professed methodological views. [...] as they reformulated preference theory so as to make its

equally advances “thin” analyses which overlook the importance of contexts. Chapters 3 and 4 of the dissertation are an attempt to revert this tendency.

0.2.3. Preconceptions against the use of subjective data

A second normative reconstruction of the history of the economics of happiness is being written by Richard Easterlin (2002, 2004). Instead of embedding the economic analysis of happiness data in a long history of happiness in economics (Section 0.2.1), Easterlin treats the development of the subfield in the context of the economic analysis of subjective data (1). In this historical reconstruction, he joins the discourse of other economists who promote their subfields as a challenge to the historic and unfounded skepticism of economists toward the use of these data (2).

(1) Easterlin’s (2004) reconstruction integrates the development of the economics of happiness into the history of the economic use of subjective data. He begins by showing that, unlike other social scientists, economists have been historically unwilling to use subjective data as economic facts:

“There is a long and respected history in the social sciences of survey research that elicits subjective testimony (i.e., self-reports) on feelings, beliefs, values, expectations, plans, attitudes and behavior, including intensive inquiry into possible shortcomings of such data. This extensive body of evidence is unfortunately largely excluded from economic analysis even though economic theory almost invariably includes reference to motivations, expectations, well-being, and the like.” (Easterlin, 2004, pp. 21)

For Easterlin, the exclusion of the analysis of subjective data is explained by economists’ “belief” that “what people *do* is more relevant than what they say” (*ibid.*). He thinks this “disciplinary stricture” has its origins in economists’ graduate training indoctrination to the “*disciplinary paradigm of behaviorism*” (*ibid.*, p. 31, emphasis added). Free from this stricture, scholars

behavioral implications more explicit, these mainstream economists nevertheless ignored the work of behaviorist psychologists. They continued to obtain their assumptions from introspection or a priori deduction, rather than looking to rigorous experimental results as their own behaviorist methodology indicated that they should.” (Lewin, 1996, p. 1318)

from other social sciences would have devoted over half a century to the analysis of subjective data.

In regards to quality issues of subjective data, Easterlin shows that there are equally shortcomings in objective outcomes (*ibid.*, p. 23). Research by psychologists shows that subjective well-being data are “meaningful and reasonably comparable among sizable groups in the population” (*ibid.*). Concerning the analysis of subjective data on attitudes and expectations, Easterlin claims that economists are likely to dismiss these “squishy” data in favor of “hard” numbers such as “unemployment” or “inflation rates” (*ibid.*, p. 28). According to Easterlin, economists’ historic skepticism towards the use of subjective data is problematic. For in the meantime, specialists in other disciplines have been studying the “data problems and their solutions” (*ibid.*, p. 30). He warns his colleagues against their attitude justifying this skepticism rather than taking into account the new evidence:

“A recent article by two economists on subjective survey data [...] states as its ‘primary objective’ to turn economists’ ‘vague implicit distrust’ of subjective testimony ‘into an explicit position grounded in facts’. [...], this strikes me as a rearguard action. The use of subjective testimony in economics is growing. Its use outside economics has expanded to the point that serious challenges are being made to the efficacy of economic policy. When asked by nonspecialists about such challenges, economists will not be able to hide for long behind the defense that subjective testimony is no good.” (*ibid.*, references omitted)¹¹

Easterlin’s account is normative, as are the previously presented reconstructions (Section 0.2.1). Influenced by his training as a quantitative analyst (Kuznets was his mentor, *ibid.*, p. 3), his objective is to promote the use of subjective data in economics by claiming that no “large body of data, hard or soft”, “should be dismissed out of hand” (*ibid.*, p. 30):

“It is the task of specialists in a given area, such as labor economics or economic history, to study and evaluate each piece of data potentially relevant to the problem at hand. The

¹¹ Easterlin’s reference is Bertrand and Mullainathan’s (2001) six-page paper. In this paper, the authors claim that, despite easy availability, subjective questionnaires are a “data source that economists rarely use” (Bertrand and Mullainathan, 2001, p. 67). They show that “the unwillingness to rely on such questions marks an important divide between economists and other social scientists” (*ibid.*). The economists’ doubts, they argue, are “based on a priori skepticism rather than on evidence” (*ibid.*). They would ignore a large body of experimental and empirical work investigating the meaningfulness of answers to these questions: “Our primary objective in this paper is to summarize this literature [two papers] for an audience of economists, thereby turning a vague implicit distrust into an explicit position grounded in facts” (*ibid.*).

parent disciplines responsible for subjective testimony – demography, sociology, psychology – have produced a substantial methodological literature on data problems and their solutions. This literature can be either read with a view to finding excuses to dismiss these data or for helpful guidance on how the data may be intelligently used. I hope the more positive attitude will, in time, prevail.” (*ibid.*)

(2) Charles Manski (2000, 2004) is another scholar involved in the analysis of subjective data. As most of his colleagues, he disagrees with the economists’ predisposition to “believe only what people do, not what they say” (Manski, 2000, p. 131):

“Rather than try to infer preferences and expectations from observations of chosen actions, why not elicit them directly? Pose this question to an economist, and chances are that one will receive an instant hostile response. Economists tend to be deeply skeptical of subjective statements [...]. As a result, the profession has enforced something of a prohibition on the collection of subjective data.” (Manski, 2000, p. 131)

Interestingly, Manski’s claims are supported by a venture into the history of the economic analysis of subjective data on expectations, intended to check “whether the conventional economic wisdom on collection of subjective data is well grounded” (Manski, 2004, p. 1337). Manski finds a “meager” “scientific basis underlying economists’ hostility to measurement of expectations” (*ibid.*). He mentions, however, two events that might have something to say about the history of the economic use of these kinds of facts:

“One influential event appears to be the Machlup (1946) criticism of the ongoing efforts by economists to interview businessmen about their cost and revenue expectations. Another important part of the story occurred in the 1950s and early 1960s, when economists reported negative evidence on the usefulness in predicting consumer purchase behavior of verbal assessments of expected household finances. These specific events appear to have predisposed academic economists to the *broad but unsubstantiated* conclusion that all data on expectations are suspect.” (*ibid.*, emphasis added)

Donald McCloskey’s (1983) opinions about the use of surveys in economics are reproduced by Easterlin, Manski and most of the other authors exploring the history of the use of subjective data¹². A passage referring to the full-cost

¹² The literature focused on the historical development of the subject is limited. To my knowledge, the available accounts are Hanemann (1994), Boulier and Goldfarb (1998), Bertrand and Mullainathan (2001), Hamermesh (2004), Easterlin (2004), and Manski (2004). While Bertrand and Mullainathan (2001) and Hamermesh (2004) are cautious about the use

controversy is often quoted in this brief literature showing that unlike other social scientists, “economists are extremely hostile towards questionnaires and other self-descriptions” (McCloskey, 1983, p. 514):

“Second-hand knowledge of a famous debate among economists in the late 1930s is part of an economist’s formal education. The debate concerned the case of asking businessmen if they equalized marginal cost to marginal revenue. It is revealing that the failure of such a study – never mind whether that was indeed the study – is supposed to convince economists to abandon all self-testimony.” (*ibid.*)

The economists’ preconception against the use of subjective data is also an element in McCloskey’s account, which argues that economists are “unthinkingly” averse to use subjective data as scientific facts. Surprisingly, this is the main element supporting the current accounts of this history¹³:

“One can literally get an audience of economists to laugh out loud by proposing ironically to send out a questionnaire on some disputed economic point. Economists are so impressed by the confusions that might possibly result from questionnaires that they abandon them entirely, in favor of the confusion resulting from external observation. They are *unthinkingly committed* to the notion that only the externally observable behavior of economic actors is admissible evidence in arguments concerning economics.” (*ibid.*, emphasis added)

0.3. An alternative reconstruction

Although this dissertation is divided in three main parts, there is an important distinction to make between Part I on one hand, and Parts II and III on the

of these data, Hanemann (1994), Easterlin (2004) and Manski (2004) promote the use of subjective data in their respective subfields. Hanemann’s section on the “objections to surveys” (1994, pp. 26-32) reproduces the standard account of this history in the context of the use of contingent valuations by environmental economists. He shows that the economists’ willingness to observe “what people *do*” prevails over listening to “what they *say*”: “for many economists the ultimate argument against contingent valuation is that it violates the habitual commitment of the profession to revealed preference [...]. Fathoming human behavior is not easy; one should utilize every possible source of information” (Hanemann, 1994, p. 37). McCloskey’s (1983) passages on the controversy are reproduced by Hanemann (1994), Boulier and Goldfarb (1998), Easterlin (2004), and Manski (2004).

¹³ Unlike the rest of the literature, Boulier and Goldfarb (1998) claim that the “majority position among economists and economic methodologists is that our unwillingness to use surveys is well founded” (Boulier and Goldfarb, 1998, p. 1).

other. Part I reconstructs the history of the economics of happiness and is focused on the context of the emergence of first the early and then the recent programs. Such an approach suggests that rather than rediscovering classical elements of the analysis of happiness (and therefore bringing happiness “back”, Bruni, 2004a), early economists of happiness were involved in a more general movement, which consisted in analyzing the welfare effects of affluence. This program, however, involved a psychological conception of consumer behavior that was at odds with the psychological underpinnings of mainstream economics (i.e. choice theory). Unlike the early economics of happiness, recent developments in the subfield are advanced as complements rather than alternatives to mainstream economic analysis (0.3.1).

Parts II and III of the dissertation take the history of the economics of happiness (as presented in Part I) as a viewpoint to explore the history of the relationship between economics and psychology. Part II (0.3.2) challenges the standard account of the history of economics and experimental psychology. Instead of reading this history as one of psychology being “in” or “out” of economics (Hands, 2009), it maintains that there are always psychological assumptions involved in the economic accounts of behavior. Part III (0.3.3) deals with the history of the economic use of subjective data, its aim being to move one step beyond the accounts mentioned above (Section 0.2.3). It explores the events cited in the little available literature and develops a historical reconstruction based on the distinction between the use of subjective data as theoretical elements (scientific tools) on one hand, and their use as a support for economic policies (tools for government) on the other.

0.3.1. The economics of happiness: early and recent

As noted above, Bruni and Porta’s reconstructions present the economics of happiness as a rediscovery of the wealth-happiness nexus studied by classical political economists. Such an approach, however, tends to disregard at least two important issues: the fact that the followers of Easterlin (1974) and Scitovsky (1976) are, in general, “not aware of such an old tradition” (Bruni

and Porta, 2007, p. xxxiv)¹⁴, and the fact that the historical development of the subfield is far from being uniform.

Instead of exploring the long history of the concept of happiness as being in or out of economics (and of analyzing concepts that economists are not aware of), Part I of the dissertation focuses on the emergence of the subfield itself. It deals with the context of its emergence, as well as with the literature that has had a direct influence in its development. Chapter 1 presents the early development of the subfield and shows that it joined already established criticisms of the standard (i.e. textbook) accounts of consumer behavior and welfare. These criticisms were mainly based on the idea that the analysis of the allocation of scarce resources was inadequate for an analysis of affluent consumers (1). Chapter 2 deals with recent developments in the economics of happiness, and claims that they are being advanced as complements rather than as alternatives to mainstream economic theories (2). Parts II and III of the dissertation use this history of the economics of happiness as a viewpoint to analyze the history of the relationship between economics and psychology (3).

(1) Chapter 1 starts by presenting Richard Easterlin's paper published in 1974 and presents the main features of the "early" economics of happiness (1974-1999). It also introduces the work of Tibor Scitovsky, Fred Hirsch, Richard Layard, Robert Frank and other early economists of happiness, and shows that their writings were mainly concerned with theoretical issues regarding the formation of preferences of affluent, and hence satisfied, consumers. This literature emerged in the context of strong criticisms to public policies in affluent countries, not only at the economic level, but also with regards to cold-war strategies, over-consumption, pollution of the environment, and the irresponsible use of natural resources. Reviewing this literature, the chapter introduces the concept of "economics of affluence", which it presents as closely related to critical studies by both experimental and social psychologists during the thirty years following the end of World War II.

¹⁴ This is acknowledged in an endnote by Bruni and Porta (2007): "these economists, and those who have followed them, are not aware of such an old tradition. Their reference points were far more recent: apart from psychology's influence, Duesenberry's (1949) social theories of consumption, or the American Institutional tradition (from Thorstein Veblen to John Kenneth Galbraith)" (Bruni and Porta, 2007, p. xxxiv.).

The early economics of happiness was supported by only a few empirical studies showing that affluent consumers did not become happier as their countries became richer. This was “the happiness paradox”, or “the affluence paradox” as presented by Easterlin (1974) or Hirsch (1977). Scitovsky’s (1976) book is a good example of this literature. It proposed an alternative to consumer choice theory by suggesting that American affluent consumers failed to allocate their resources between comfort and stimulation in order to maximize their well-being. Scitovsky’s analysis was based on a psychology of behavior control, meaning that he thought of consumer behavior as influenced by the environment. According to this view, choice theory was seriously limited for it relied on the assumption of given (i.e. exogenous) preferences. Scitovsky and his early followers were focused instead on the analysis of the formation of preferences, as were most of the economists involved in the analysis of affluence.

(2) Chapter 2 maintains that the strategy followed by economists of happiness from the turn of the century onwards has been markedly different. Their rather intensive empirical studies have emerged as alternatives to the psychologists’ studies of happiness, and have been promoted as a challenge to the economists’ skepticism of the use of survey data (rather than as a challenge to choice theory). The chapter shows that “recent” economists of happiness are interested in life satisfaction data, which are different from subjective well-being data produced by psychologists. Moreover, instead of using the findings to dispute choice theory (as the economists of affluence did), the data are used as proxies for utility. This is interesting, for these economists acknowledge positive correlations between happiness and income, which contradicts much of the earlier literature.

As the recent literature is quite extensive (over 400 references between 1999 and 2009), the chapter proceeds by quantifying the publications in economic journals and books, which are represented in several different charts (Section 2.5: Joyful economists in numbers) and listed in the appendix at the end of the dissertation (p. 253). Such an analysis reveals that most of the recent studies are devoted to the analysis of the correlates of happiness, which is mainly empirically guided research. They study happiness data from different countries which are correlated with different variables, not necessarily incomes or employment, but also “religiosity”, “family life”, “age”, and other

non economic variables. Besides this kind of analysis (which is also performed by psychologists and sociologists), the economic literature studies the happiness paradox, the history of happiness in economics, the analysis of happiness and public policy, and the “Life Satisfaction Approach” for valuing environmental goods.

Chapter 2 concludes by showing that most of the recent literature presents the analysis of happiness as a complement (rather than an alternative) to economic theory and policy. This literature does not intend to replace mainstream accounts, but rather uses happiness data as a measure of utility. The last section of Chapter 2 concludes by comparing Layard’s (2005) analysis of happiness and public policy with his earlier views, which are presented in Chapter 1 (Layard, 1980). This case study shows that Layard’s “new” account relies on an analysis of revealed preferences, which he completes with an analysis of happiness data. Another example of this approach is the “Report by the Commission on the Measurement of Economic Performance and Social Progress” requested by the French president (Stiglitz *et al.*, 2009), which suggests the inclusion of subjective well-being data as only one of among the many different variables to be looked at by policy makers. These views of the analysis of happiness are markedly different from those advanced in the early literature, which presented Easterlin’s paradox as justification for a discourse on the shaping of preferences.

One of the main conclusions of Part I of the dissertation is that the history of the economics of happiness is not uniform. It consists of at least two clearly distinct periods marked by changes in both the psychological underpinnings of the program, and the intensiveness of the use of “life satisfaction” data. Methodologically speaking, Part I has the character of a historical reconstruction. It is less focused on the history of the concept of happiness, than on the practices of the economists of happiness. However, even if Part I highlights the contexts in which the main literature has been produced, it is based on published material and downplays (without neglecting) the role of the institutions and personal backgrounds which have influenced the authors involved in the subfield. It emphasizes, instead, the differences between the early and the recent programs advanced by (sometimes the same) economists involved in happiness studies.

(3) The break in the dissertation between Part I and Parts II and III can be understood in different ways. While Part I deals with the writings of economists of happiness (and those of a few economists of affluence), Parts II and III deal with the history of economics and psychology. They are the result of an analysis of the development of the economics of happiness as an economics subfield. According to this analysis, events happening in core parts of economics are seen as beneficial to the emergence of the subfield. Such a perspective allows the history of the economics of happiness (as presented in Part I) to be viewed as a “symptom” for changes in mainstream economic analysis. Parts II and III focus on two of these changes: the development of (the “new”) behavioral economics as a mainstream subfield, and the rise in the use of survey data by mainstream economists, both of which occurred during the last decade of the 20th century. Parts II and III of the dissertation thus define the development of the economics of happiness as a viewpoint from which to rewrite two histories: the history of economics and experimental psychology (Part II), and the history of the economic use of survey data (Part III)¹⁵.

Part II of the dissertation opposes the standard history of economics and psychology (Section 0.2.2). Instead of developing a history focused on the relationship between different kinds of economics (the classical, the marginalist, the ordinalist), with a singular concept of psychology as being “in” or “out” of them (Hands, 2009), Part II allows for the diversity in the field of psychology, exploring the different kinds of approaches that have influenced the development of the economics of happiness. This does not mean that Part II is an exhaustive account of the “polymorph” development of psychology, itself a varied discipline (for that the reader may turn to the references in the history of psychology given in Part II). It rather extends the narrative to the origins of the psychological subfields involved in the history that is the object of this dissertation. It is in this sense that the history of the economics of happiness is taken as a viewpoint to explain the history of the relationship between economics and experimental psychology.

¹⁵ This methodology is somehow comparable (though less focused on contexts) to that of Philip Mirowski’s history of economics for the “cyborg” viewpoint in *Machine Dreams* (2002). It is also comparable to Nicola Giocoli’s approach in *Modeling Rational Agents* (2003) which relates the development of game theory as a subfield, to that of core parts of economics (such as general equilibrium theory). These two volumes were sources of inspiration for Part II of the dissertation, and are briefly discussed in Chapter 4 (section 4.5).

Part III of the dissertation focuses on the main elements put forward in the literature about the history of the economic use of survey data. As noted above (Section 0.2.3), this literature claims that economists have been unfoundedly skeptical towards the use of such data and lists the measurement without theory, the full-cost, and the measurement of expectations controversies as the main elements of this history. These controversies (at least the two first) are well-known events for historians of economics who have read them as elements of the history of econometrics, or of that of the theory of the firm. Part III gives a new reading of these events by considering them from a new perspective. The interest of taking the economics of happiness as a viewpoint (which also applies to Part II), is that it allows new ways of reading past events, as the future emergence of subfields will surely provide new elements to be taken as viewpoints to rewrite the history of economics.

The passage from Part I, on one hand, to Parts II and III, on the other, is thus marked by a quite radical change of viewpoint.

0.3.2. Remarks on the history of economics and experimental psychology

Part II of the dissertation explores the history of the relationship between economics and experimental psychology. Chapter 3 deals with the history of psychological accounts of behavior control (1) and Chapter 4 with the history of the psychological underpinnings of consumer choice theory (2). These two chapters trace the history to the late 19th century in order to explore the roots of these two accounts. In doing so, they investigate several elements which have been only slightly studied by historians of economics, such as the influence of psychophysics, behaviorism, and adaptation-level theory in consumer choice theory.

(1) Chapter 3 maintains that both economists of affluence and early economists of happiness had a psychological understanding of consumer behavior that was at odds with the psychological underpinnings of consumer choice theory, and that the quarrel between mainstream economists and these other economists was a matter of conflicting psychologies. This quarrel, the

chapter maintains, was nothing but a new episode in a long dispute opposing “purposive” views of behavior held by mainstream economists to “behavior control” approaches originating from experimental psychology.

According to the standard (i.e. textbook) economic accounts of consumer behavior, there are preferences to be satisfied which have their origins “within” sovereign consumers, that are transmitted to producers by means of the price system of markets. Unlike mainstream economists of their time, both economists of affluence and early economists of happiness were willing to explore the formation of preferences rather than relying on the principle of consumer sovereignty. They proposed alternative views of consumer behavior by pointing out that the origins of preferences did not lay within sovereign consumers, but could be explained by “external” factors such as the consumption of other agents (interdependence of preferences), or the activities of production and advertising (dependence effects in J. K. Galbraith’s terms).

The first sections of the chapter deal with the history these “behavior control” approaches, from their origins in experimental psychology to behaviorism as developed by B. F. Skinner from the 1930s to the 1970s, as well as their influence in economic analysis. They show that behaviorism was, above all, a product of the American conception of science. Unlike the standard reconstructions of the history of economics and psychology (which tend to consider behaviorism as the denial of introspection only: Section 0.2.2), they claim that behaviorism was much more than a denial of introspection. Its main ambition was not methodological, but functional (technological), its aims being the prediction and the control of behavior.

Behaviorism emerged out of a combination of German experimentalism on one hand, and the evolutionary naturalist approach of British psychologists on the other. This mixture was imported to America by instinct psychologists, such as William James, and by functional psychologists, such as William McDougall. Experimental psychology developed into behaviorism which was charged with progressive values and the desire to control behavior in order to improve American society. From this perspective, mainstream economics was all but behaviorist, as it systematically avoided behavior control issues. Behavior control approaches were discarded by economists all throughout the 20th century, despite the fact that behaviorists were successful in avoiding mentalism, and that avoiding mentalism was precisely what (mainstream)

economists were saying they were attempting to achieve. As we shall see, the history of behavior control is an interesting (though overlooked) part of the history of economics and psychology. Both functional psychology and the early behaviorism of Watson had their respective counterparts in economics: first, the early institutionalism promoted by Thorstein Veblen and Wesley C. Mitchell (whose references to functional psychology were explicit), and after WWI the program of “behaviorist institutionalism” promoted by Mitchell himself, Lawrence K. Frank and Morris A. Copeland.

After World War II, neobehaviorism emerged as a dominant school in American psychology with B. F. Skinner as its most prominent figure. Chapter 3 shows that there was also a counterpart to this movement in economics. This was the economics of affluence, which was far from what the so called “behaviorist mainstream economists” (E. Slutsky, J. Hicks, R. Allen, P. Samuelson) were doing, but close to B. F. Skinner’s views on American society. While economists of affluence, such as J. K. Galbraith, thought of consumer behavior as a matter of behavior control (controlled by “dependence effects” and “reversed sequences”), the economists invested in the development of choice theory assumed consumer sovereignty, and dismissed the behaviorist way of understanding behavior. The main features of behaviorism were kept distant from mainstream economics and rejected on the grounds of a “freedom and dignity” discourse opposing the progressive aim of using science to control economic behavior.

Chapter 3 concludes that the early economics of happiness was to remain excluded from mainstream economics, as it was based on the “wrong” psychology of consumer behavior. It ends by briefly presenting the reaction of P. Samuelson, M. Friedman and their followers to J. K. Galbraith’s approach.

(2) Chapter 4 is the counterpart of the history presented in Chapter 3. It is based on the idea that there are always psychological assumptions supporting economic accounts of behavior, which are systematically, though ambiguously, related to different psychological subfields. The first sections of the chapter show that the “economic psychologies” of Jevons and Edgeworth were radically different from what experimental psychologists (especially the German) were doing during the late 19th century.

The early development of German experimental psychology was strongly marked by the distinction advanced by Wilhelm Dilthey [1833-1911] between the sciences of the mind (*Geisteswissenschaften*) and the natural sciences (*Naturwissenschaften*). This distinction was integrated by Wilhelm Wundt [1832-1920], who characterized his psychological studies as “physiological” (*Physiologischen Psychologie*) and “social” (*Völkerpsychologie*)¹⁶. Wundt treated physiological psychology as a natural science that was restricted to the experimental analysis of sensations. *Völkerpsychologie* dealt with mental phenomena (such as purposive rationality) that he considered too complex to be studied through experimentation. As he considered human thought to be the result of social and historical elements, Wundt created “an experimental psychology that was not social and a social psychology that was not experimental” (Mandler, 2007, p. 59)¹⁷. Wundt’s *Principles of Physiological Psychology* (1874) advanced the new discipline as a natural science which was able to intermediate associationism and physiology¹⁸.

However, even if Wundt’s experimental psychology was influenced by associationism, Wundt’s laws of association were limited to the analysis of elementary sensations. “Fusions”, “assimilations” and “complications” were

¹⁶ According to Edwin G. Boring’s *History of Experimental Psychology* (1950), in order to understand experimental psychology one must “go back into philosophical psychology in order to see what it was that, married to physiology, gave birth to physiological, experimental psychology” (Boring, 1950, p. 158). Starting with Descartes [1596-1650], and passing through Leibnitz [1646-1716], Locke [1632-1704], Berkeley [1685-1753], Hume [1711-1776] and Hartley [1705-1757], Boring’s history takes (British) associationism as the stage preceding the emergence of German experimental psychology in the late 19th century: “The nineteenth century saw the culmination of associationism in James Mill [1773-1836] and its modification from a mental mechanics to a mental chemistry by John Stuart Mill [1806-1873]. It saw associationism made over by Bain [1818-1903] into the system that was to become the substructure for the new physiological psychology.” (Boring, 1950, p. 219)

¹⁷ Wundt’s experimental psychology, Mandler writes, “was to a large extent sensory psychology. It was strictly scientific, followed rigid rules of experimentation, and did not allow any ‘softer’ concerns. In the same vein and in part due to the influence of Fechner, Wundt adopted statistical error theory in experimental psychology, though he rejected statistical laws for the historical phenomena treated in *Völkerpsychologie*” (Mandler, 2007, p. 57).

¹⁸ Wundt explored this connection through psychophysical experimentation. In Wundt’s words, the essence of experimentation was “to modify the conditions of a fact and to adjust these conditions in a quantitatively determinable way, the goal being to recognize constant relations between causes and effects” (Wundt, 1874, p. 2). As only the “external physical conditions of internal phenomena” were accessible to direct measurement and could “be freely modified”, he thought “the application of the experimental method” could only be applied through the “contiguous field of psychophysics” (*ibid.*).

laws that interconnected sensations rather than thoughts or ideas, as did British (philosopher) psychologists¹⁹. In Boring's (1950) words:

“it is quite clear that [Wundt] thought of the elements as being experienced as such, and in fact his conception of introspection almost demands such a view. If introspection is the mere having of experience immediately, then it cannot be an inferential process of abstraction, and the elements must stand up under attention immediately in the given.”
(Boring, 1950, p. 334)

Wundt's introspection, or rather self-observation (*Selbstbeobachtung*), consisted in laboratory practices restricted to immediate experiencing. It was analytical, and “meant the resolution of experience into compounds of sensations and other elements like them” (*ibid.*, p. 329). The first sections of Chapter 4 claim that, unlike the German approach, British psychology was based on armchair introspection, and that it was this kind of psychology – which was not restricted to laboratory practices, but rather applied to the analysis of consciousness – that influenced the economic-psychological approaches of British marginalists. The chapter then presents the strategy followed by W. S. Jevons and F. Y. Edgeworth as well as the rejection of their program by Max Weber (1908) and the American institutionalists (Mitchell, 1910).

An important part of Chapter 4 (the middle-sections) is devoted to the analysis of Harry Helson's *Adaptation-Level Theory* (1964), which is used as a vehicle to explore the contrast between the theories of behavior developed by psychologists, as well as by economists (i.e. choice theorists). It maintains that the economic reaction to Helson's theory of behavior is comparable to the mainstream economists' reception of the “cyborg” approach of J. von Neumann and H. Simon, on one hand (Mirowski, 2002; Sent, 2004), and their reception of von Neumann and Morgenstern's theory of games, on the other (Mirowski, 2002; Giocoli, 2003). It claims, however, that the economic

¹⁹ Associationism consisted in the establishment of laws relating sensations and ideas. These laws (*similarity*, *contiguity* and *intensity* in J. S. Mill's case (Boring, 1950, p. 229)), included both thoughts and feelings which were studied by means of armchair introspection. In J. S. Mill's words: “It is obvious that the complex laws of thought and feeling not only may, but must, be generated from these simple laws [of association] [...]. These are cases of mental chemistry: in which it is possible to say that the simple ideas generate, rather than that they compose, the complex ones” (J. S. Mill's *Logic* (1843) quoted in Boring, 1950, p. 230). See Daston (1978), Maas (2005, pp. 155-162) and their references for further accounts on the history of Victorian psychology and economics and their relation to the emergence of naturalist approaches in the human sciences.

reaction to adaptation-level theory has been overlooked not only by historians of economics, but also (and mainly) by behavioral economists and economists of happiness who are directly involved in adaptation-level analysis (the usual terms for this kind of studies are respectively “reference dependence” and “hedonic adaptation”).

Helson’s theory had its origins in the psychophysical analysis of vision. Starting from the analysis of photopic adaptation, he extended the analysis of adaptation from sensory outcomes to that of the more complex phenomena of affectivity and motivation. For Helson, adaptation-levels controlled behavior. These levels were explained by the past and present stimulations experienced by an organism (i.e. external factors). According to Helson’s theory, all forms of behavior were influenced by the effects of stimulations on adaptation levels:

“an individual’s attitudes, values, ways of structuring his experiences, judgments of physical, aesthetic, and symbolic objects, intellectual and emotional behavior, learning, and interpersonal relations all represent modes of adaptation to environmental and organismic forces [...]. Stimuli impinge upon organisms already adapted to what has gone before, and internal states depend upon previously existing internal conditions as well as external inciters to action [...]. The pooled effect of [...] stimuli determines the adjustment or adaptation level underlying all forms of behavior.” (Helson, 1964, p. 37)

Adaptation-level theory was developed as an “experimental-quantitative approach to problems of behavior” (Helson, 1964, p. 57). It was the result of extending psychophysical analysis in the field of vision, to broader subjects, including affectivity and motivation. Psychologists involved in the development of adaptation-level theory showed that complex behaviors such as cognitive acts or the learning of skills, which were also affected by stimuli, were a function of adaptation-levels “no less than perception and judgment” (*ibid.*, p. 63).

Helson’s comprehensive account of behavior was a reference, not only for Scitovsky (1976) (who was both an early economist of happiness and an “old” behavioral economist), but also for Kahneman and Tversky (1979). The last section of Chapter 4 claims that Scitovsky (1976) and Kahneman and Tversky (1979) used of Helson’s theory in different ways which eventually marked the historical development of (the “new”) behavioral economics and the economics of happiness.

Helson's theory was a reference for early economists of happiness (such as Scitovsky, 1976, and Frank, 1989). Scitovsky's (1976) book proposed an alternative to consumer choice theory by considering consumer well-being as the outcome of two different types of consumption: need satisfaction and stimulation. The book showed that in affluent contexts, consumer well-being depended on stimulation consumption rather than need satisfaction. This kind of consumption was assumed to be subject to adaptation-level principles rather than those governing consumer choice theory. Chapter 4 presents Scitovsky's attempt to reformulate consumer choice theory with insights from experimental psychology. It shows that this theory failed to succeed, and is absent from both the "new" behavioral economics and recent developments in the economics of happiness. Unlike Scitovsky, Kahneman and Tversky (1979) were less radical towards choice theory. Instead of taking their psychological theory from the adaptation-level *theory of motivation*, these behavioral economists borrowed just a few elements from Helson's *theory of perception*, and a few others from cognitive psychology and behavioral decision research. They advanced a program that was not radically different from standard choice theory, which consisted in using "the rationality assumption of mainstream economics as a benchmark from which to consider deviations" (Sent, 2004, p. 750). This "new" behavioral economics has since then been presented as a complement rather than an alternative to consumer choice theory (*ibid.*).

While early economists of happiness opposed the mainstream economic accounts of consumer behavior, Kahneman, Tversky, and their followers have developed an approach that is consistent with the fundamental assumptions of choice theory, as, for instance, the exogeneity of consumer preferences.

Concerning the history of the economics of happiness, Part II of the dissertation claims that recent developments in the economics of happiness are based on a strategy that is comparable to that of the "new" behavioral economists. As noted above, Clark *et al.* (2008) present the economics of happiness as a "valuable alternative, but complementary, approach to the revealed-preference framework that dominates the discipline of economics" (Clark *et al.*, 2008, p. 136).

0.3.3. The economic analysis of subjective outcomes

Part III of the dissertation takes the history of the economics of happiness as a viewpoint to analyze the history of the economic use of subjective data. It is, however, important to point out from the beginning of this introductory section that not all survey data are subjective. Boulier and Goldfarb's (1998) paper on "the use and nonuse of surveys in economics", gives a useful distinction of "the categories of information that questionnaire surveys might be used to collect" (Boulier and Goldfarb, 1998, p. 4). They list seven different categories, each one with an explanatory example.

Figure 0.1 (Boulier and Goldfarb, 1998, pp. 4-5)

- 1 Facts of past or current economic life:
'Were you employed in the survey week?'
- 2 Predictions of future facts of economic life:
'Do you intend to buy a new car next year?'
- 3 Measurement of hard-to-gauge variables, or the value of goods and services not traded in the market:
'What is the most that your household would agree to pay each year in higher prices [for petroleum products] for wire net covers to prevent about 20,000 migratory wildfowl from dying each year in waste-oil holding ponds in the Central Flyway?' (see Desvousges *et al.* 1993: 120)
- 4 The interviewee's explanation of his/her past and current behavior:
'Why did you decide to hire more people last period?'
- 5 Predictions of future behavior or behavior under counter-factual conditions:
'If you found an envelope with \$100 in it, and the owner's name on the front, what is the probability that you would return it?' (see Frank *et al.* 1993)
- 6 Participant or observer opinions and ideas:
'Were you satisfied with the training program you participated in? Do you have any suggestions for improving it?'
- 7 Gauging the interviewee's information set: for example, how 'well-informed' is the interviewee?
'What interest rate is (was) your bank paying on your savings account?'

The first category in Figure 0.1 is not subjective for us, as it makes reference to "simple descriptions of economic activity" (*ibid.*, p. 5). The information gathered by this kind of question requires the "recounting of facts which are presumed known by the respondent" (*ibid.*, p. 6). This kind of survey data is "accorded relatively high 'fact status' by economists", and has been widely used (*ibid.*)²⁰. The six other categories of survey data are subjective. They

²⁰ For Stiglitz *et al.* (2009), all survey data are subjective and subjective measures "have always been part of the traditional tool-kit of economists and statisticians" (Stiglitz *et al.*, 2009, p. 43). They state that unemployment "is typically measured based on people's answers as to whether they worked at all in a specific reference week, whether they actively looked for a job, and whether they would be available to start working in the near future" (*ibid.*). They

correspond to questions intended to explore inner aspects of the respondents' lives. Following the classification in Figure 0.1, these categories explore: (2) intentions, (3-4) preferences, (5) motives, (6) satisfactions and (7) knowledge²¹. As this information is related to subjects traditionally studied by psychologists, the history of the economic use of these data is also connected to the history of the relationship between the two disciplines.

Chapters 5 and 6 of the dissertation venture into the history of the economic use of subjective data, their aim being to provide an introductory rather than a final account of the subject. As noted above (Section 0.2.3), the available reconstructions do not go very far. They present the economists' skepticism towards the use of subjective data as a result of unsubstantiated preconceptions, the origins of which are supposed to be second-hand-knowledge of both the full-cost controversy and the economic expectations debates of the 1950s. Chapter 5 reads these controversies as part of a history of subjective quantification and economic *theory*. It explores George Katona's attempt to develop a program of behavioral economics out of the analysis of subjective data, and relates Katona's studies to first the full-cost and the measurement without theory controversies, and then the collection and analysis of subjective data on attitudes and expectations (1). Chapter 6 deals with the history of subjective quantification and economic *policy*. It shows that unlike Katona's use of subjective data, the current approaches are the outcome of applied research rather than theoretical programs. It presents both the current analysis of economic expectations and the development of contingent valuations, and shows that even economists of happiness are attempting a move in this direction with the Life Satisfaction Approach for valuing environmental goods (2).

(1) Chapter 5 begins by showing that the available literature on the measurement without theory and the full-cost controversies tends to overlook Katona's role in these two events. It shows that the literature about the full-cost debates neglect the fact that half of F. Machlup's (1946) paper was

are forced, however, to make the distinction between these data and "subjective measures of quality of life" which have "no obvious objective counterpart" (*ibid.*).

²¹ Subjective well-being data are similar to category 6 in Figure 1.1. They correspond to general satisfaction questions (life satisfaction) or in some cases to questions restricted to particular domains (as "job satisfaction" or "income satisfaction").

focused on the analysis of the validity of R. Lester's (1946) questionnaires, and that his claims were based on Katona's analysis of economic behavior. This rather unexplored feature of the controversy shows that Machlup's (1946) defense of marginalism was based on the analysis of "real" businessmen behavior rather than on the instrumentalist (*as if*) arguments often advanced in the literature on the controversy²². With regards to the measurement without theory debates, Chapter 5 also shows that standard accounts of the conflict between the Cowles Commission and the NBER tend to overlook Katona's project of analyzing subjective data of business behavior. Katona's *Price Control and Business* (1945) was indeed conducted under the auspices of both the Cowles Commission and the NBER, and seems to have influenced Koopmans' argumentation in the measurement without theory controversy²³.

The rest of the chapter deals with the history of Katona's program of "economic psychology". During the 1940s, Katona shifted from the psychological analysis of learning and teaching he developed in *Organizing and Memorizing* (1940), to the analysis of business behavior. He attempted to build a psychology of economic behavior out of the collection of subjective data. In *Price Control and Business* (1945), Katona analyzed the attitudes and motives underlying the economic behavior of businessmen by means of detailed interviews rather than mail or telephone surveys. His study was intended to promote the use of interviews for economic research. From 1947 onwards, Katona worked on the development of the Survey of Consumer Finances conducted by the Survey Research Center of the University of Michigan, where he attempted to develop an "economic psychology" out of the analysis of consumer behavior. However, in 1954, a committee appointed by the Federal Reserve Board evaluated the Survey of Consumer Finances in terms of its predictive power, and gave a negative report on the data on consumer attitudes. A controversy ensued between Katona and some of the members of the committee, resulting in a conference about the quality of the

²² This literature tends to read the event as a theoretical conflict between the Oxford Group project and the "Marshallian supply and demand framework" (Lee, 1981, p. 339) leading to the instrumentalist interpretation of the theory as defended by M. Friedman (1953) and F. Machlup (1955) (Mongin, 1992, 1997, 2000).

²³ Like Koopmans, Katona was critical of the NBER-type analysis exploring aggregate rather than individual behavior. This was indeed the reason why he proceeded by means of "detailed interviews with a small sample of businessmen", rather than "compiling quantitative data on prices, sales, costs, and profits" as the NBER did (Katona, 1945, p. 5).

subjective data on expectations (NBER, 1960). These debates led to new programs of subjective quantification.

Between the 1960s and 1970s, different surveys on consumer “attitudes”, “buying intentions”, and “expectations”, were carried out by the Survey Research Center (interviews), the NBER (mail surveys), the U.S. National Industrial Conference Board (telephone surveys), and the U.S. Bureau of Census (Juster, 1960, p. 604). During the 1960s, members of the U.S. Bureau of Census and of the NBER proposed a shift from the analysis of “attitudes”, to “buying intentions”, and then to that of “subjective purchase probabilities”:

“a survey of buying intentions is simply a less efficient way of getting an estimate of purchase probabilities than a survey of explicit probabilities. Intentions seem to have no informational content that a probability survey does not also have, and the probability survey is able to extract information that is not obtainable from intentions surveys.”

(Juster, 1966, pp. 660-661)

Unlike Katona’s project, the aim of Juster’s approach and that of his followers at the NBER was to predict purchase rates. This approach was based on probability surveys and supposed to breach the step from the qualitative study of attitudes, to the quantitative analysis of purchase expectations. Juster’s approach was developed in order to meet the requirements of government agencies and was designed as a forecasting device rather than as a theoretical account of consumption. However, the analysis of subjective data on purchase probabilities failed to succeed, and most of the Federal survey programs were discontinued during the 1970s. Chapter 5 concludes that Katona’s program failed partly because it was not understood by economists, who were interested in the predictive power of the data rather than their potential to develop new theories of aggregate consumption.

(2) Chapter 6 shows that much of the current analysis of subjective data (which has become quite intensive) is the result of applied research programs. This is the case, for instance, of the Index of Consumer Sentiment produced by the Survey Research Center since the 1950s. Originally designed by Katona as part of his theoretical program of behavioral economics, the Index of Consumer Sentiment is nowadays used by businessmen and government agencies as a forecasting device rather than as a theoretical tool, and has been

replicated in many countries (Curtin, 2004). The Survey of Economic Expectations conducted since 1993 by the University of Wisconsin Survey Center and studied by C. Manski, J. Dominitz and their associates, is an attempt to renew with the Federal programs presented in Chapter 5 (supported by Juster (1960, 1966) and his associates). It consists of a nationwide telephonic survey examining how the American labor force perceives its near-term economic future. Both the Index of Consumer Sentiment and the Survey of Economic Expectations are intended as means to produce useful data rather than evidence for testing existing theories or developing new ones. The Survey of Economic Expectations, for instance, produces data in the form of probabilistic elicitation in patterns that are consistent with the prevailing theories of economic expectations (Dominitz and Manski 1997a-b, Manski and Straub 2000)²⁴.

Chapter 6 also analyzes the raise of “contingent valuations” by ecological and environmental economists, which measure “existence values” of natural resources. It shows that “the use of sample surveys (questionnaires) to elicit the willingness of respondents to pay for (generally) hypothetical projects or programs” (Portney, 1994, p. 3) responds to governmental requirements rather than to theoretical problems. It was indeed the inclusion of existence values in edicts such as the Oil Pollution Act (1990) that got economists involved in the analysis of this kind of facts.

The last two sections of Chapter 6 analyze the economics of happiness in terms of its usefulness. They show that unlike much of the economic analysis of subjective data, there is no evident use for the happiness data as “tools for government” (Desrosières, 2008a, p. 7). Rather than producing data to deal with economic problems (such as the Index of Consumer Sentiment or contingent valuations), economists of happiness analyze life satisfaction data that are produced by third parties in general surveys. The “Life Satisfaction Approach” for valuing environmental goods, which is one of the most recent developments in the subfield, uses life satisfaction data as proxies for utility in an attempt to find an economic use for these kinds of facts. Such an approach,

²⁴ Dominitz and Manski (1997a) think their approach has become attractive for economic research precisely because “probabilistic expectations provide empirical evidence in the form sought by modern economic theory” (Dominitz and Manski, 1997a, p. 857). Economists analyzing decision making under uncertainty, they write, “generally assume that individuals assign coherent subjective probabilities to future events. So economists can readily utilize probabilistic expectations data, whereas qualitative expectations data are difficult to utilize” (*ibid.*).

however, does not go without contradicting much of the previous developments in the subfield.

0.4. Summing up

In summary, the dissertation begins by marking the distinction between early and recent developments in the economics of happiness (I. The Economics of Happiness). It then takes this history of the subfield as a viewpoint with which to explore the history not only of the relationship between economics and experimental psychology (II. Bypassing Behavior Control), but also of the economic use of subjective data (III. Subjective Quantification in Economics).

Part I of the dissertation is developed in two chapters. Chapter 1 deals with the early economics of happiness (1974-1999) and maintains that, rather than an echo of classical analyses, this program was the result of discussions about the effects of affluence in both consumer behavior and welfare. It was supported by just a few empirical studies and was largely directed against economics theories. Chapter 2 presents the recent development of the subfield (1999-2009), which is characterized by an intensive analysis of life satisfaction data. It shows that the recent economics of happiness has been positioned with consideration to hedonic psychology, and that the main empirical findings are presented as complements, rather than opposed to mainstream economic accounts.

Part II (Chapters 3 and 4) takes the history of the economics of happiness as a viewpoint to explore the history of the relationship between economics and experimental psychology. It is based on the idea that the shift from the early to the recent economics of happiness is marked by a change in the psychological assumptions supporting these two programs. Chapter 3 deals with the history of the psychology supporting the early economics of happiness, which was a psychology of behavior control. Chapter 4 explores the history of the psychological assumptions supporting both the recent economics of happiness and the “new” behavioral economics, which are principles underlying choice theory.

Finally, Part III (Chapters 5 and 6) is based on the intensification of the economics analysis of happiness data which it takes as a viewpoint to observe

the history of the use of subjective data in economics. Chapter 5 explores the history of subjective quantification and economic *theory* and shows that the use of subjective data has systematically failed as a means of testing or developing theoretical accounts. Chapter 6 deals with the history of subjective quantification and economic *policy* and claims that the recent upsurge in the economic analysis of subjective data can be explained by the use of such data as “tools for government” (Desrosières, 2008a, p. 7). It concludes by showing that the latest developments in the economics of happiness constitute a move in this direction as they consist in using life satisfaction data as tools for valuing nonmarket goods.

I. THE ECONOMICS OF HAPPINESS

Introduction

The following two chapters present the economics of happiness by introducing the main literature. They make the distinction between the early and the recent programs. Chapter 1 begins by presenting Easterlin's (1974) version of the happiness paradox. It shows that his argument concerning the relation between incomes and happiness was based on James Duesenberry's (1949) relative income thesis as well as Hadley Cantril's (1965) socio-psychological studies. The novelty of Easterlin's paper consisted in presenting new evidence showing that consumers' satisfaction (their satisfaction with their income, consumption, job, etc.) was relative to their expectations (*aspirations*), their past situation (*habituations*) and the welfare of other people (*social comparisons* or *interdependence of preferences*). He claimed that there were "good psychological reasons" explaining why people in rich countries felt less happy than they should.

Chapter 1 further maintains that rather than rediscovering classical studies about the happiness-wealth nexus (Bruni, 2004a), both Easterlin (1974) and Scitovsky (1976) were involved in a wider movement: the economics of affluence. This movement can be traced back to James Duesenberry's (1949) critical analysis of the Keynesian consumption function, as well as John Kenneth Galbraith's (1958) writings on economics and affluence. The economics of affluence, as presented by these authors, considered that economic theory was outdated and inappropriate for the economic analysis of affluent consumers.

Drawing on Easterlin (1974) and Scitovsky (1976), Fred Hirsch (1977) showed that an increasing portion of consumption took a social aspect in affluent contexts. This meant that consumer satisfaction depended not only on private consumption, but also on consumption by others, and that it was highly influenced by the relative positioning of consumers in society. Robert Frank (1985, 1989) extended Hirsch's concept of positional goods to the analysis of consumer welfare and maintained that economic theory should not only take into account absolute levels of consumption, but also a "frame of reference within which to evaluate them" (Frank, 1989, p. 80). Both Hirsch (1977), and Frank (1985, 1989) are nowadays considered as early contributors to the economics of happiness (Bruni and Porta, 2005, p. 4). They explored consumer behavior in affluent contexts which they explained by means of "external" factors such as social "norms" of consumption.

Based on the few available empirical studies of the period (mainly Easterlin, 1974), Richard Layard (1980) attempted to formulate recommendations of economic policy. As for other early economists of happiness (namely R. Frank), Layard's recommendations were advanced in theoretical terms and focused on the formation of consumer preferences. His advice consisted in applying taxes and changing institutions in order to "offset the individual drive for status" (Layard, 1980, p. 738).

Chapter 2 deals with the literature of the second period (1999-2009), which is marked by both the intensification of the analysis of "life satisfaction" data, and the establishment of the economics of happiness as a subfield. It claims that the foundation of hedonic psychology (1999) had a great impact in the development of the economists' subfield. It presents the main developments in hedonic psychology including D. Kahneman's concepts of "objective happiness" and "experienced utility". Rather than collecting retrospective evaluations of happiness, hedonic psychologists have developed new techniques of measurement which focus on immediate evaluations. These evaluations supposedly avoid important biases of "duration neglect" and "imperfect recall", contained in the survey data analyzed by economists.

Unlike hedonic psychologists, economists of happiness are focused on life satisfaction data gathered in general surveys. They reject the psychological theories of happiness according to which subjective well-being adjusts to "adaptation-levels" after major events such as winning a lottery or suffering crippling accidents. They explore alternative ways in which happiness may be

affected by public policies, and are not only interested in the effects of economic factors such as income variations or unemployment, but also on those of demographic factors such as “family relationships” or “community and friends” (Layard, 2005, p. 63). Since the recent literature is numerous in comparison to the references for the first period, the chapter proceeds by quantitatively analyzing the publications in economics books and journals. In what regards the content of the literature, it shows that the recent economics of happiness is empirically (rather than theoretically) focused, and less radical in its position on economic policy than the earlier literature.

Chapter 1. 1974-1999: Economics for the Affluent, Happiness for the Satisfied

1.1. Introduction

The following sections present the main features of the early analysis of happiness by economists. Section 1.2 begins by presenting Easterlin's (1974) seminal paper and showing that the interpretation of his empirical findings was based on James Duesenberry's (1949) relative income argument as well as Hadley Cantril's (1965) socio-psychological studies. Section 1.3 presents Scitovsky's analysis of consumer satisfaction developed in *The Joyless Economy* (1976). This analysis showed that choice theory was inadequate for the analysis of consumer behavior in affluent contexts. Based on insights coming from experimental psychology, Scitovsky explained that rather than keeping unsatisfied margins in all their needs, consumers arranged to have "intermittent complete satisfaction, with the moments or periods of full satiation suitably spaced over time" (Scitovsky, 1976, p. 67). He stated that consumption in affluent societies was consumption by satisfied consumers. As choice theory excluded complete satisfaction of needs, it was incapable of dealing with this kind of behavior. Scitovsky's theory was thus a theory of consumer behavior in affluent contexts.

Section 1.4 shows that rather than rediscovering classical studies about the happiness-wealth nexus (Bruni, 2004a), Easterlin (1974) and Scitovsky (1976) were involved in a wider movement: the economics of affluence. This movement emerged shortly after the end of World War II and defended ideas that became widespread during the 1960s and 1970s. During this time, economists became increasingly interested on the effects of corporate mass production on the stock of natural resources and the quality of the environment on one hand, as well as consumer satisfaction on the other. This was the case, for instance, of James Duesenberry's (1949) critical analysis of the Keynesian consumption function and J. K. Galbraith's (1958) writings about the affluent societies.

Galbraith's views were based on the idea that economic theory was outdated and inappropriate for the economic analysis of affluent countries. The views of economists such as Fred Hirsch (1977) and Robert Frank (1985, 1998) were close to Galbraith's. They were focused on the analysis of the wants and needs of affluent consumers, their criticisms were directed against both choice theory and welfare economics, and they are nowadays considered as early contributors to the economics of happiness (Bruni and Porta, 2005, p. 4).

Section 1.5 shows that instead of advocating for an increased productive efficiency in order to satisfy consumer needs, early economists of happiness focused on correcting the negative welfare effects that resulted from social comparisons and positional competitions. Their claims were based on theoretical principles rather than empirical findings, and explored the welfare effects of "the pursuit of status", "expected incomes", and "expected status". Such was the case with Richard Layard (1980). In order to regulate these effects, Layard proposed taxes designed to "offset the individual drive for status" (Layard, 1980, p. 738), and called for "institutional changes" and "changing human nature" as means to alter individuals' utility functions.

Section 1.6 concludes the chapter by presenting the main trends in the early development of the subfield. It does so by presenting the contributions of A. Oswald (1997), R. Frank (1997), and Y-K. Ng (1997) in a symposium for *The Economic Journal* entitled *Controversy: Economics and Happiness*. These contributions summarized the main features of the early development of happiness studies in economics. Oswald (1997) presented the main findings of the few empirical studies of the period. These findings supported Easterlin's paradox and showed that unemployed people were particularly unhappy. Frank's (1997) account focused on theoretical issues as the inclusion of social comparisons as externalities. Finally, Ng (1997) advanced the analysis of happiness in the language of a new welfare economist and advocated for a return to the analysis of cardinal measures of welfare and their comparisons.

1.2. Discovering happiness data and introducing the happiness paradox

As noted above, Richard Easterlin's paper "Does Economic Growth Improve the Human Lot?" (1974) was the first economic analysis of happiness data

ever made. It tackled one of the cornerstones of welfare economics by testing the relation between economic development and social welfare. “Is there evidence that economic growth is positively associated with social welfare, i.e., human happiness?” (Easterlin, 1974, p. 6) This was Easterlin’s main question. His answer was negative and was presented in three steps¹.

(1) Easterlin’s approach consisted in exploring happiness through quantitative analyses of subjective well-being data. The paper started by presenting the concept of happiness (i.e. subjective evaluations of the respondents) and the measuring methods². He acknowledged problems in the reliability of the data but denied that they produced biases strong enough to invalidate the findings:

“the possibility of differential bias in the replies by income level cannot be ruled out, [...]. My own feeling is that while such bias may exist, it is not significant enough to invalidate the association between income and happiness. Perhaps the most important basis for this judgment is the impressive consistency of the results in a variety of times and places with widely differing cultural and socioeconomic circumstances.” (Easterlin, 1974, p. 15)

Easterlin’s defense of the quality of the data was based on research results of psychologists whose studies began during the 1930s³.

(2) Easterlin’s results were presented as a paradox (recently labeled the Easterlin Paradox) contrasting two kinds of findings. The first were positive correlations for subjective well-being and income within countries:

“Does greater happiness go with higher income? [...]. In every single survey, those in the highest status group were happier, on the average, than those in the lowest status group

¹ Easterlin’s paper consisted in three parts: “1. The Concept and Measurement of Happiness” (pp. 6-15); “2. The Evidence” – divided in “Within-Country Comparisons” (pp. 15-20), “International Comparisons” (pp. 20-24), and “National Time Series” (pp. 24-27) – and “3. Interpretation”, divided in “Theory” (pp. 27-29), “Evidence for a “Relative Income” Interpretation” (pp. 29-32), and “An Analogy” (pp. 32-34).

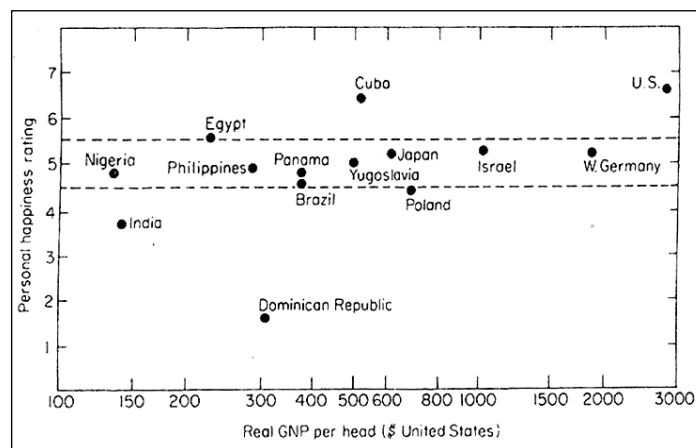
² Easterlin brought together results of surveys of human happiness conducted in fourteen countries. The data were of two types. First, responses to a Gallup-poll-type survey asking: “In general, how happy would you think say you are – *very* happy, *fairly* happy, or *not very* happy?”, and second, a survey *à la* Cantril (1965), yielding a “rating of each individual of his personal standing on a scale from 0 (the worst possible life) to 10 (the best possible life)” (Easterlin, 1974, p. 8). Despite the difference in these two measurement procedures Easterlin suggested that “the concept of happiness underlying them [was] essentially the same” as they both consisted in subjective evaluations of the respondents (*ibid.*).

³ Easterlin’s main references were Cantril (1965), and Wilson (1967). The work of these two authors is presented below in this section and in Chapter 2.

[...]. I am inclined to interpret the data as primarily showing a causal connection running from income to happiness.” (Easterlin, 1974, pp. 15, 20)

The second were findings for aggregate data revealing weak correlations for subjective well-being and income between countries (averages) and for subjective well-being and income in American time series. As an initial interpretation, Easterlin maintained that although income and happiness went together, the relation was “not as obvious as in the within-country cross-sectional comparisons” (*ibid.*, p. 27). He presented the analysis of correlations by means of the following diagram:

Figure 1.1 (Easterlin, 1974, p. 22)



In the figure, the area comprised within the broken lines was meant to show the absence of correlation between subjective well-being and income (averages). Easterlin’s explanation was the following:

“The inference about a positive association relies heavily on the observations for India and the United States. [...] the personal happiness ratings for 10 of the 14 countries lie virtually within half a point of the mid-point rating of 5, as is brought out by the broken horizontal lines the diagram [...]. The happiness differences between rich and poor countries that one might expect on the basis of the within-country differences by economic status are not borne out by the international data.” (*ibid.*, pp. 21, 23)

(3) The third part of Easterlin’s paper was intended as an explanation of the findings. Drawing largely on James Duesenberry’s (1949) “relative income” thesis (Section 1.3), he explained that the satisfaction an individual derived

from his consumption depended on his relative spending rather than his spending in absolute terms:

“there is a ‘consumption norm’ which exists in a given society at a given time, and which enters into the reference standard of virtually everyone. This provides a common point of reference in self-appraisals of well-being, leading those below the norm to feel less happy and those above the norm, more happy.” (*ibid.*, pp. 28-29)

The author defended his claim with studies by social psychologists, notably Hadley Cantril’s *The Pattern of Human Concerns* (1965). In this well-known volume, Cantril had shown that consumption norms varied directly with the economic development of a country⁴. Like Cantril, Easterlin believed that comparison standards adjusted in time canceling the effects of income rises on subjective well-being (*ibid.*, p. 32). According to Easterlin, there were psychological factors which conditioned individuals’ assessment of their economic and social situation:

“there are good psychological reasons why people may not feel better off, even though they ‘should’. This is because the standard with reference to which evaluations of well-being are formed is itself a function of social conditions. As these conditions ‘improve,’ the norm tends to advance along with people’s actual experience. Economic analysis has been able, for a long time, to resist the uncomfortable implications of this mechanism, by assuming that tastes are given and/or immeasurable. [...] with the growth in concern about long-term economic growth, on the one hand, and in the evidence on people’s feelings and aspirations, and the factors governing them, on the other, one can only wonder whether this view will be much longer defensible.” (*ibid.*, pp. 33-34)

1.3. The joylessness of the satisfied

Two years later, Tibor Scitovsky published *The Joyless Economy* (1976), in which he compared psychological and economic theories of motivation and proposed an alternative to consumer choice theory. Scitovsky’s book was divided in two parts: an analytical part comparing psychological and

⁴ Easterlin’s paper was based on extensive reproductions of Cantril’s survey results. Based on the answers to Cantril’s interviews, he showed that the differences in material aspirations of American and Indian people depended on the level of development of the two countries.

economic approaches to behavior (1), and an applied part comparing the American and the European lifestyles (2)⁵.

(1) Scitovsky's theoretical analysis was opposed to the economic accounts of consumer behavior and showed that economics was "concerned almost exclusively with a world of scarcity" (Scitovsky, 1976, p. 64). He claimed that by understanding consumers as "having many needs and desires, but not enough money, time, or energy to satisfy all of them completely" (*ibid.*), economists dealt with consumption in contexts of scarcity. According to this view, all needs remained constantly unsatisfied in their absolute level:

"If the economist's 'ideal man' is not rich enough to fill all his needs to satiation, he will stop short of fully satisfying all of them, not just one or a few. And not only must he keep unsatisfied margins on all his needs and desires, but he also must see to it that any extra dollar he spends on one thing yields him as much satisfaction as that extra dollar would if he spent it on any other thing. If that were not so, a little rearrangement of his spending pattern could make him better off at no extra cost." (*ibid.*, p. 65)

Scitovsky's aim was to correct the economic theory of consumer choice. He showed that, unlike the economists' discourse, the analysis of psychologists was strongly based on experimental evidence showing that complete satisfaction was usually reached. According to these studies, the need satisfaction processes in men and animals were discontinuous and complete rather than continuous and partial:

"Men and animals have many needs, but, because they cannot attend to all of them at once, they fill most needs intermittently, one at a time [...]: people arrange to have intermittent complete satisfaction, with the moments or periods of full satiation suitably

⁵ Tibor Scitovsky [1910-2002] is largely recognized because of his contributions to the "new" welfare economics (Scitovsky, 1941, 1951, 1972, 1973). The *Joyless Economy* stands at the end of his career and was overlooked for several decades before drawing the attention of scholars involved in the recent development of behavioral economics and the economics of happiness. Scitovsky presented *The Joyless Economy* as an "Inquiry into Human Satisfaction and Consumer Dissatisfaction". It was divided in two parts. The first – "I. The Psychology and Economics of Motivation" – was composed by five chapters: "Between Strain and Boredom" (pp. 15-30), "The Pursuit of Novelty" (pp. 31-58), "Comfort Versus Pleasure" (pp. 59-79), "Enter Economics" (pp. 80-105), "Necessities and Comforts" (pp. 106-132), and "Income and Happiness" (pp. 133-145). The second part of the book – "II. The American Way of Life" – was divided in: "Is Our Life Too Good?" (pp. 149-181), "Is It too Dull?" (pp. 182-203), "Our Puritain Ghost" (pp. 204-223), "Our Disdain for Culture" (pp. 224-247), "What's Wrong with Mass Production?" (pp. 248-263), and "What's Wrong with Specialization?" (pp. 264-284).

spaced over time. Other forms of compromise, however, are also possible. For example a man who is too poor to heat his home to the most agreeable temperature is much more likely to heat a part of it to that temperature rather than heat the whole of it to a suboptimal temperature.” (*ibid.*, pp. 64, 67)

Scitovsky considered satiation as a point of complete satisfaction. He equated complete satisfaction with comfort and showed that full states of comfort implied the absence of pleasures and pains (“affect” in psychological terms). Continuous comfort was thus incompatible with pleasure feelings⁶:

“the satisfaction of a need gives both pleasures and comfort. But the continuous maintenance of comfort would eliminate pleasure, because, with arousal continuously at its optimum level, there can be no change in arousal towards the optimum. In other words, incomplete and intermittent comfort is accompanied by pleasure, while complete and continuous comfort is incompatible with pleasure.” (*ibid.*, p. 71)

Scitovsky showed that affluent consumers faced the “dilemma” of choosing between comfort and pleasure. As this dilemma was absent from consumer choice theory (which excluded complete satisfaction of any want), Scitovsky’s aim was to adjust the analytical framework of economics so as to offer an account of economic behavior rich enough to deal with this “new” kind of choice:

“Drives to relieve discomfort, stimulation to relieve boredom, and the pleasures that can accompany and reinforce both – those are the three motive forces of behavior distinguished by psychologists today [...]. I propose to simplify the psychologist’s threefold classification into a twofold one. We shall henceforth be concerned with choice, especially with choice in economically advanced and affluent societies, where the main scope for choosing between pleasure and comfort lies in the area of stimulation, because affluence crowds out for many people, the pleasures of want satisfaction. In such societies want satisfaction can be more or less equated with comfort. Most pleasures of most people come from stimulation in such a society [...].” (*ibid.* pp. 78-79)⁷

⁶ According to the psychology of motivation on which Scitovsky’s analysis was based, the “need satisfaction” processes depended on the environments in which the organisms evolved. In the United States, “a very affluent country”, the context in which consumer behavior took place was a context in which the standard of living took “the form of more occasions and larger areas of satiation” (*ibid.*, p. 71). Although this resulted in a “self-evident” increase in comfort, Scitovsky showed that “another likely consequence [was] a reduction in pleasure” (*ibid.*).

⁷ Scitovsky’s analysis of motivation was based on Harry Helson’s *Adaptation-Level Theory* (Scitovsky, 1976, p. 40). This aspect of *The Joyless Economy* is more deeply analyzed in Chapter 4.

This quote contains the main elements of Scitovsky's project. It was based on the work of psychologists and intended as an analysis of choice between pleasure and comfort in affluent contexts. In these contexts, consumers were "satisfied consumers" most of the time.

(2) Although Scitovsky had drawn on psychology to elaborate his theory, his purpose was also that of a welfare economist: to prescribe economic policies (Scitovsky, 1941). The principles advanced by Scitovsky were, however, not concerned with production nor the availability of resources (he wrote for affluent societies) but with the preferences of economic agents. The second part of *The Joyless Economy* explored the "American Way of Life", for in Scitovsky's view, the inclination of Americans for comfort goods largely exceeded their demand for stimulation. This misallocation of resources was supposed to be the source of the joylessness of American consumers.

Scitovsky's aim was to show that the American consumption pattern was shaped by "cultural, educational and economic influences" (Scitovsky, 1976, p. 283). These influences made the Americans' "consumption of stimulation, variety, and novelty" "lower than average" for an affluent country (*ibid.*)⁸. Among the main factors forming American tastes and swaying the American "consumption pattern in favor of comfort and against stimulus enjoyment" (*ibid.*, p. 224), Scitovsky counted the "Puritan Ghost", which was a cultural issue, and the "Disdain for Culture" which he counted as the effect of "a bias in [...] education" (*ibid.*). Besides these two factors, Scitovsky pointed to the effects of production in shaping consumers' tastes. Both "mass production" and "specialization" were supposed to limit consumers' sovereignty:

"Among the specialists to whom the consumer relinquishes some of his sovereignty is the producer. Increasing specialization inevitably deepens the gulf between the producer's specialist expertise and the consumer's generalist ignorance of the nature and design of manufactured products [...]. The producer gains from reducing the time and effort needed to produce a given output, because that adds to both his profit and the economy's growth. What is more natural than that he should try to please the consumer the same way? Most

⁸ This difference in the consumption pattern was supposed to reflect "national differences in tastes" which were supposed to be the outcome of individual differences: "Individual differences in tastes exist and are often great; it is only natural to expect there to be national differences in tastes as well." (*ibid.*, p. 199)

of the consumer goods America has given the world save time and effort – no wonder we overdo saving both.” (*ibid.*, p. 274)

1.4. From the economics of affluence to the economics of happiness

As noted above, the works of Easterlin (1974) and Scitovsky (1976) are nowadays considered as starting points of the economics of happiness that historians like Bruni (2004a) relate to old histories of happiness in economics. However, these works should not be taken as sudden rediscoveries of old traditions. They rather fit in a more general and critical movement of discussion of the welfare effects of affluence. Contextually speaking, the early economics of happiness (1974-1999) draws more on this literature than it does on the discourse of Aristotle, Genovesi or Bentham (Introduction). The aim of this section is to show that early economists of happiness were part of a more general enterprise: “the economics of affluence”⁹. It shows that this literature was directed against the inadequacy of economics in dealing with the post-war problems of the affluent countries (1). James Duesenberry’s (1949) analysis of the interdependence of preferences opposed the Keynesian consumption function (2). Fred Hirsch (1977) (3), and Robert Frank (1985, 1989) (4), applied Duesenberry’s theses to microeconomic accounts of consumer behavior.

(1) The main concepts used by economists of affluence from the 1950s to the 1970s, and from the mid-1970s onwards by early economists of happiness, were exposed by John Kenneth Galbraith in *The Affluent Society* (1958), furthered in *The New Industrial State* (1967), and popularized in *The Age of Uncertainty* (1977)¹⁰. Galbraith claimed that the framework of economics had

⁹ Writings about economics and affluence emerged short after the end of World War II in the more developed countries. John Kenneth Galbraith’s writings about *The Affluent Society* (1958) are the most conspicuous example of the discourse held by economists of affluence. Galbraith’s views were not new by the time he published his famous book but were rather widespread opinions held from the late 1940s to the 1970s not only by economists but also by other scholars who renewed with the early work of institutionalists such as Veblen (1899) and Mitchell (1912) (Chapter 3 deals with B. F. Skinner’s writings about welfare in affluent societies).

¹⁰ This Section is based on the first of these publications. *The New Industrial State* and *The Age of Uncertainty* are briefly presented in Chapter 3.

become obsolete for the analysis of affluent societies (the American society in particular), and maintained that it was senseless for the American government to persist in economic policies that were intended to deal with problems of scarcity:

“The shortcomings of economics are not original error but uncorrected obsolescence [...]. The affluent country which conducts its affairs in accordance with rules of another and poorer age [...] forgoes opportunities. And in misunderstanding itself will, in any time of difficulty, implacably prescribe for itself the wrong remedies.” (Galbraith, 1958, pp. 3-4)

After the introductory chapters, *The Affluent Society* showed that unlike the traditional (classic) “preoccupations of economic life” (*ibid.*, p. 93), the economic discourse no longer focused on the “equality” and “security” of man, but had “narrowed down to a preoccupation with productivity” (*ibid.*). Despite the affluent character of the American economy, the analysis of production and efficiency still remained among the economists’ main concerns:

“In the world of Ricardo goods were scarce. They were also closely related, if not to the survival, at least to the elemental comforts of man [...]. Now goods are abundant. More die in the United States of too much food than of too little [...]. Yet production remains central to our thoughts.” (*ibid.*, pp. 96-97)

Galbraith thought that economic analysis was based on two misleading principles: scarcity, and consumer sovereignty¹¹. As Scitovsky would claim in *The Joyless Economy* (1976), Galbraith thought economic analysis excluded the concept of satiation, and was focused instead on the analysis of the satisfaction of given wants. He advanced the two propositions in the following terms:

“The first is that the urgency of wants does not diminish appreciably as more of them are satisfied [...]. The concept of satiation has very little standing in economics [...]. The

¹¹ In Galbraith’s view, there was also an asymmetry in the way the private and public productions were considered. He thought there were strong general views regarding “privately produced production” as the only important output (*ibid.*, p. 104). Private production was supposed to add to “national well-being” and increase “national wealth”, whereas public services were considered as a necessary burden to be “carried by the private production” (*ibid.*). Galbraith encouraged the affluent countries to invest in public services in order to catch up the production of the private sector: “we must find a way to remedy the poverty which afflicts us in public services and which is in such increasingly bizarre contrast with our affluence in private goods [...]. The community is affluent in privately produced goods. It is poor in public services.” (*ibid.*, pp. 240, 246). A similar concern about private vs. public production was addressed by G. Myrdal in his *Challenge to Affluence* (1963).

second proposition is that wants originate in the personality of the consumer or, in any case, that they are given data for the economist. The latter's task is merely to seek their satisfaction. He has no need to inquire how these wants are formed. His function is sufficiently fulfilled by maximizing the goods that supply the wants." (*ibid.*, p. 112)

Unlike the "want satisfaction" framework of traditional economic analysis, Galbraith thought consumption was guided by "dependence effect" mechanisms. These mechanisms suggested that preferences were influenced by at least two external factors: the aggregate production level and the advertising practices of producers:

"The urge to consume is fathered by the value system which emphasizes the ability of the society to produce. The more that is produced the more must be owned in order to maintain the appropriate prestige. The latter is an important point, for, without going as far as Duesenberry in reducing goods to the role of symbols of prestige in the affluent society, it is plain that his argument fully implies that the production of goods creates the wants that the goods are presumed to satisfy [...]. The even more direct link between production and wants is provided by the institutions of modern advertising and salesmanship." (*ibid.*, p. 121)

(2) As the preceding quote shows, Galbraith's analysis referred to James Duesenberry's writings about the interdependence of preferences¹². Duesenberry's *Income, Saving and the Theory of Consumer Behavior* (1949) showed that even if the interdependent character of consumption had been recognized and discussed "since the earliest days of economics" by economists such as Veblen, Pigou, or Knight (Duesenberry, 1949, p. 14), it had "failed to influence the development of economic theory" (*ibid.*, p. 15). He showed that this failure was due both to "the failure of the critics to prove empirically the importance of interdependence" (*ibid.*), and to their failure to produce a "substitute for the analytic scheme provided by the preference system concept" (*ibid.*).

Duesenberry's book was a criticism to the Keynesian consumption function. It was focused on the analysis of aggregate behavior rather than individual

¹² Galbraith's passages on the dependence effect were also supported by references to Keynes' *Essays in Persuasion* (1932). Galbraith's views about the effects of advertising and salesmanship on consumer behavior are presented in Chapter 3.

decision making and avoided the analysis of the psychological origins of individual preference functions¹³.

“If we wish to explain in detail every purchase by every individual we are in a hopeless position. We certainly cannot create a useful analytical scheme on the basis of detailed individual psychology [...]. Instead of explaining every detail of consumption behavior, we try to explain the average behavior of a large group. [...] we are compensated by being able to deal with the interdependence problem and by getting more definite results than can be obtained from the preference analysis.” (*ibid.*, pp. 18-19)

The first chapters of Duesenberry’s book were based on empirical studies of incomes, savings and consumption for the American economy. Starting from the evidence that “the average urban family with a \$1500 income” saved “8 per cent” during the 1920s (*ibid.*, p. 26), Duesenberry’s objective was to explain why “a similarly placed family saved nothing” in 1941 (*ibid.*). In other words, his objective was to explain why “people with a given income increase[d] their consumption” (*ibid.*). It was in this context that he introduced the relative judgment argument. Spending, he claimed, was pushed by the dissatisfaction felt by an individual when comparing his living standard to that of others:

“The analysis of the forces causing impulses to consume shows that these arise when an individual makes an unfavorable comparison of his living standard with that of someone else [...]. Consequently the dissatisfaction with his consumption standard which an individual must undergo is a function of the ratio of his expenditures to those of people with whom he associates.” (*ibid.*, p. 32)

After testing his own hypothesis against data on savings (*ibid.*, pp. 47-92), Duesenberry extended his criticism to “the general theory of consumer behavior” (*ibid.*, p. 93)¹⁴. It is important to realize that this criticism was directed against the independence postulate in Keynes’ “general theory” and not against consumer choice theory. Duesenberry argued that “the propensity

¹³ Duesenberry’s book was divided in five chapters besides the introduction and conclusions: “The Empirical Basis of the Theory of Consumers’ Choice” (pp. 6-16), “A Reformulation of the Theory of Saving” (pp. 17-46), “A Theory Versus the Facts” (pp. 47-68), “Short Run Fluctuations in Saving” (pp. 69-92), and “The Implication of Interdependent Preferences” (pp. 93-110).

¹⁴ Duesenberry’s hypothesis was the following: “Cet. par. the propensity to save of an individual can be regarded as a rising function of his percentile position in the income distribution. The parameters of that function will change with changes in the shape of the income distribution” (*ibid.*, p. 45)

to save” should be regarded as a rising function of the individuals’ “percentile position in the income distribution” rather than as a function of absolute income (*ibid.*, p. 45).

(3) As noted above, Duesenberry’s relative income thesis was furthered and applied to the analysis of “new” subjects by Galbraith and other authors from the 1950s to the 1970s, and from the mid-1970s onward by Easterlin and his followers. In 1977, Fred Hirsch presented a detailed account of the relative income/consumption problem and brought the analysis down from Duesenberry’s aggregate discourse to the individual preferences of consumers¹⁵. Hirsch’s *Social Limits to Growth* (1977) had its origins in both the economics of affluence and the economics of happiness. Like Galbraith, Hirsch was focused in the “outmoded perspective” of economic analysis. He dealt with the “paradox of affluence”, and explored “the complexity and partial ambiguity of the concept of economic growth once the mass of the population [had] satisfied its main biological needs” (Hirsch, 1977, p. 1). He claimed that consumption had a social component in the affluent societies:

“as the level of average consumption rises, an increasing portion of consumption takes on a social as well as an individual aspect. That is to say, the satisfaction that individuals derive from goods and services depends in increasing measure not only on their own consumption but on consumption by others as well.” (*ibid.*, p. 2)

In order to deal with the interdependence of preferences, Hirsch developed the concepts of “social scarcity” and “positional consumption”. He argued that in affluent contexts, consumption was gradually extended to goods satisfying “social” rather than “private” needs, a phenomenon that seriously undermined the capacity of economic growth to solve want satisfactions. Hirsch’s *Social Limits to Growth* were the direct consequence of the social character of consumption:

¹⁵ Besides Duesenberry (1949), Hirsch’s main references were Harrod (1958): “The Possibility of Economic Satiety” and Linder (1970): *The Harried Leisure Class*. He showed that the concept of social scarcity explained “the full significance of relative incomes in economic welfare” (Hirsch, 1977, p. 111). He showed that relative incomes had “been observed by a long line of economists and philosophers”, and that a “relative income hypothesis” had been “formulated by James Duesenberry after World War II in stronger form, contending that relative income was the dominant influence on the proportion of income spent rather than saved” (*ibid.*)

“social limits exist in the sense that an increase in physical availability of these [social] goods or facilities, [...], changes their characteristics in such a way that a given amount of use yields less satisfaction. This is equivalent to a limitation on absolute supply of a product or facility of given ‘quality,’ and it is in this sense that it is regarded here as a social limitation [...]. Satisfaction is derived from relative position alone, of being in front, or from others being behind [...]. Where the sole or main source of satisfaction derives from the symbol rather than the substance, this can be regarded as pure social scarcity.” (*ibid.*, pp. 20-21)

Besides this thesis, Hirsch is to be acknowledged for incorporating positional competition in the framework of microeconomic theory by considering it as an externality. He viewed symbolic (positional) competition as a market failure: a “disjunction between the terms of individual and social choice offered by market opportunities” (*ibid.*, p. 52). As for the more traditional externalities, Hirsch thought of incorporating external costs “imposed on others” in the “market situation confronting the individual” (*ibid.*):

“By positional competition is meant competition that is fundamentally for a higher place within some explicit or implicit hierarchy and that thereby yields gains for some only by dint of losses to others [...]. The existence of the positional sector in the context of growth in the material sector can thus be seen as a kind of ‘system externality’.” (*ibid.*, pp. 52-53)

Following Galbraith, Hirsch focused on the problem of the “transmission” of information “from the preferences of the individual consumer to the delivery mechanism of the market and governmental suppliers” (*ibid.*, p. 17). He was aware of Galbraith’s “dependence effects” and explained that Galbraith’s argument could be presented “in the terms of his critics’ models by viewing it as a case of market failure” (*ibid.*, p. 108)¹⁶. The “Hole in the Affluent Society” (*ibid.* p. 102), swallowing the welfare of consumers, could be corrected by taking into account the “public context” of “private marketing” (*ibid.*, p. 108). What went wrong with the industrial system, he concluded, was “not the delivery but the order: to meet consumers’ *individual* wants” (*ibid.*, pp. 108-109).

A final important point about Hirsch’s work is that it was “guided, stimulated and criticized” by Tibor Scitovsky and supported by Easterlin’s (1974) analysis of happiness data¹⁷. Based on the argument that external costs

¹⁶ Chapter 3 deals with the mainstream economists’ reaction to the economics of affluence.

¹⁷ “Perhaps the most striking indication of the significance of relative income has been presented by Richard Easterlin in a review of existing empirical evidence on the connection

imposed to individual agents by general economic improvements were “not revealed in the national accounts” (*ibid.*, p. 55), he was favorable to the development of new social indicators evaluating the “key outputs of the system in the form of proxies for happiness or well-being” (*ibid.*).

(4) During the 1980s, Robert Frank borrowed Hirsch’s (1977) concept of positional goods and extended Hirsch’s framework to the analysis of well-being. He applied the relative income argument to the analysis of consumer welfare and advanced the “frame of reference concept” which is nowadays a core element in both the economics of happiness and behavioral economics. In *Choosing the Right Pond* (1985), Frank showed that, for any given consumption level, an individual was better off with a high position in a modest economy than with a low position in an affluent context¹⁸. Frank’s book title reflected the following picture.

Figure 1.2 (Frank, 1985, p. i)



between income and expressed satisfaction with life, in the sense of happiness [...]. Easterlin’s explanation is essentially in the dominance of relative standards and reference points as the basis of individuals’ expectations of what they should have” (Hirsch, 1977, p. 111).

¹⁸ Frank presented Hirsch’s concepts by explaining what happened with prize winnings: “Many of the prizes in life are what Fred Hirsch has called ‘positional goods’ – goods that are thought after less because of any absolute property they possess than because they compare favorably with others in their own class. [...] If the quality of instruction in all universities were suddenly cut by half tomorrow, students admitted to Harvard would still have the same obvious reasons for celebrating their good fortune as before.” (Frank, 1985, p. 7)

In a subsequent article entitled “Frames of Reference and the Quality of Life” (1989), Frank backed up his argument by referencing the importance of contexts in the determination of the utility of consumption:

“The neoclassical economic model of choice abstracts from context, saying that utility depends only on the level of consumption [...] its narrow focus misses something important. To predict people’s behavior, to draw inferences about their well-being, or to make intelligent policy decisions, we must not only know the relevant levels of consumption, but also have an appropriate frame of reference within which to evaluate them.” (Frank, 1989, p. 80)

He based his argument on Harry Helson’s *Adaptation-Level Theory* (1964) and proposed an analysis of well-being relying on the psychologist’s account of perception:

“The pioneering reference in the psychological literature on the role of context in perception is Harry Helson’s 1964 book, *Adaptation-Level Theory*. Helson explains that, within broad limits, the human nervous system responds less to the absolute level of any stimulus than to deviations between it and the relevant norms or reference levels encountered in local environments.” (*ibid.*)¹⁹

According to Frank’s account, the perception of consumers depended on their environment of reference. As a consequence, their behavior was ruled less by the maximization of utility functions based on “consumption levels” (*ibid.*) than by their reaction to the experiences of the pleasure of “doing well relative to local norms” (*ibid.*, p. 81). These local norms were determined by various modes of comparison: consumption by others (interdependence), their own past consumption (adaptation), or expected consumption (aspirations) (*ibid.*). The economic policies proposed by Frank were also characteristic of the early economics of happiness. Like Hirsch’s, they were advanced within the framework of welfare economics:

“The first theorem of welfare economics tells us that competitive equilibria will be Pareto optimal under certain conditions. One of these conditions is the absence of significant externalities. Yet if relative consumption is important, it follows logically that this

¹⁹ Harry Helson’s *Theory* is presented in Chapter 4. Frank’s claims about adaptation made also reference to *The Joyless Economy*: “The psychological reward mechanism – [...] – is activated not by good conditions in any absolute sense, but by improving conditions that are favorable in relative terms (For an illuminating discussion, see Tibor Scitovsky, 1976)” (Frank, 1989, p. 80).

condition cannot be satisfied, because each person's consumption imposes negative externalities on others." (*ibid.*, p. 82)

Frank proposed consumption taxes as means to correct what he saw as a market failure:

"If the problem is that positional consumption goods are misleadingly attractive to individuals, the simplest solution is to make them less attractive by taxing them. A tax on positional consumption should be attractive to economists for the same reasons that a tax on pollution is attractive. Such taxes make the economy function more efficiently, not less." (*ibid.*, p. 83)

More interestingly (as we shall see in Chapter 3), Frank thought about manipulating contexts of consumption. According to his viewpoint, consumer behavior was not sovereign but conditioned by the environment:

"Because a given level of consumption provides more satisfaction in some contexts than in others, people can increase their satisfaction by strategic manipulation of the contexts in which consumption occurs [...]. In general, a given level of consumption will provide greater satisfaction the more favorably it compares with the consumption levels of other in the reference group. The apparent implication is that everyone will do best to work with the least able coworkers and to live in the poorest neighborhoods." (*ibid.*, p. 84)

1.5. Happiness and economic policy: Layard (1980)

Instead of calling for an increased efficiency to meet the satisfaction of consumer wants, early economists of happiness showed that there was not always a direct relation between resource availability and want satisfaction in affluent contexts. This position was reflected in their policy recommendations. Richard Layard's (1980) recommendations are worth presenting in detail for it is interesting to compare his early ideas to those he has been advancing more recently (Chapter 2). His paper on "Human Satisfaction and Public Policy" (1980) dealt with two main subjects: (1) "The Pursuit of Status", and (2) "The Role of Expected Income and Expected Status". These subjects were approached by means of analytical arguments about individual economic behavior.

(1) Layard's first incursion to the analysis of happiness was based on the argument that "despite economic growth" there was "much causal evidence that people in the West [were] not becoming happier" (Layard, 1980, p. 737). If growth did not bring happiness, Layard thought the important question to ask was "what policy conclusions follow[ed]" (*ibid.*). The first part of the paper was focused on the negative welfare effects of social comparisons. To correct these effects, Layard thought the government should apply taxes designed to "offset the individual drive for status" (*ibid.*, p. 738):

"If it could identify the actions that improve status, the government could tax them. For example, if income confers status, it can and should be taxed on efficiency grounds. Thus it may be that the income tax is a lot less inefficient than is sometimes supposed." (*ibid.*)²⁰

But more interestingly, he explored the welfare effects of institutions designed to enhance productivity by promoting competitive motives. He thought these institutions had a positive effect on individual motivation as well as an impact on economic behavior. They subtracted, however, from human welfare:

"The scope for status competition is also of course affected by institutions. It may be true that human nature is intrinsically competitive. But the extent to which the competitive motive dominates his behaviour depends upon the number of competitions that are open to him. Every organizer knows this, and when he worries about insufficient motivation among those for whom he is responsible he is tempted to invent a competition [...]. There is no doubt that these devices do motivate. The question is whether they add to or subtract from human welfare." (*ibid.*)

In order to counter the welfare effects of these kinds of institutions, Layard moved somewhat beyond the borders of his discipline by proposing not only "institutional changes" but also "changing human nature". These unconventional measures were intended to change the structure of consumers' preferences. These measures, he wrote, seemed to be "a much surer approach" than the mere application of income taxes (*ibid.*, p. 744). By changing the individuals' utility functions, it was possible to make people get "more pleasure from the welfare of others and less from the feeling of being better than others [we]re" (*ibid.*):

²⁰ "if status and expectations are as important determinants of human satisfaction as they seem to be, it will not do for economists to tell policy-makers to ignore them. There is little virtue in tidy solutions that ignore major elements in a situation." (Layard, 1980, p. 748)

“the problem is that every stage of life is littered with institutions that reinforce the competitive value system [...]. If we spend so much time putting people in order, can we really expect ourselves to work for motives unconnected with rank-order? Yet if we cannot, it is not going to be easy to improve human welfare (at any rate once a modicum of physical comfort has been achieved). If personality is largely constructed in the first six years of life, perhaps the best hope lies in a moral code which forbids all comparisons between children until they are, say, six.” (*ibid.*)

(2) When he came to analyze the welfare effects of expected incomes and expected status, Layard’s (1980) proposals proved quite curious too. Regarding the happiness paradox, he showed that the rise of “expected income” was an “obvious reason” that explained why higher income had “not brought more happiness” (*ibid.*, p. 745). In order to prevent incomes falling below their expected levels, Layard proposed policies designed to maintain low expected incomes. He supported this argument with Scitovsky’s theses about the effects of novelty on the well being of affluent consumers (Chapter 4):

“The point I have been making about expectations bears some relation to Scitovsky’s argument about novelty. We need to be surprised, and this is a difficult thing for the individual to contrive for himself. Clearly he will be better off if his income is always running ahead of his expectations. This argues in favour of public policies to encourage low expectations and, perhaps, low juvenile wages.” (*ibid.*, pp. 747-748)

Finally, in regards to “expected status”, Layard thought the welfare effects of losing status were stronger than those of increasing status²¹. He was thus favorable to maintaining status rankings and opposed income redistributions unless there was “some good reason to the contrary” (*ibid.*, p. 748). Any changes in relative positions would deteriorate subjective well-being for the community as a whole:

“we may suppose that increases in status above the level expected produce smaller gains in happiness than the loss of happiness caused by a shortfall in status below the level expected [...]. The importance of expected status is not of course an argument against the

²¹ This part of Layard’s argument was based on the analysis of “loss aversions” made by behavioral economists (Chapter 4): “People seem to fight against cuts in their living standards much more energetically than they fight for increases. This suggests that the marginal utility of income for decreases in income below what is expected is much greater than the marginal utility for increases in income” (Layard, 1980, p. 747).

redistribution of income. The fact that the poor do not resent the riches of the wealthy does not mean that their happiness could not be increased by a bigger cut. But it does again argue in favour of preserving relative rankings, unless there is some good reason to the contrary.” (*ibid.*, p. 748)

1.6. An overview of the literature and the main subjects of the period: a conclusion

Discussion about the economic analysis of happiness arose in *The Economic Journal* in 1997, with a symposium entitled *Controversy: Economics and Happiness*. The three contributions to the controversy, by Oswald (1997), Frank (1997), and Ng (1997), reviewed the main features of the early developments in the subfield: the empirical findings (1), the main implications in terms of economic policy (2), and its position towards economic theory (3).

(1) Andrew Oswald’s “Happiness and Economic Performance” analyzed subjective well-being data and resumed the main empirical findings of the period. He recommended the incorporation of subjective well-being data to national accounting by arguing that economic “things” mattered “only in so far as they ma[de] people happier” (Oswald, 1997, p. 1815). He partially anticipated recent developments in the subfield:

“Unlike gross domestic product and inflation, happiness is not something that governments try to record from year to year. This essay will show that they could and, for the issues of Economic Trends in the next century, possibly should.” (*ibid.*)

Oswald summarized the most significant results of the empirical studies in seven findings (listed below). With the exception of the analysis of “suicidal behavior” (Findings 5 and 6), Oswald captured the main features of the economic analysis of happiness data²²:

²² An alternative method to measure well-being was attempted by Bernard van Praag in 1971 and further developed as “the Leyden approach” by B. van Praag, P. Frijters and their associates. The approach consisted in measuring utilities via “verbal qualifiers”. The following is a short description by van Praag and Frijters (1999): “The Leyden approach focuses primarily on the evaluation of *income* [...]. [It] is based on two assumptions. The first is that the individuals are able to evaluate income levels in general and their own income in particular, in terms of “good,” “bad,” “sufficient,” and so on. We call these terms *verbal*

“FINDING 1. Happiness with life appears to be increasing in the United States. The rise is so small, however, that it seems extra income is not contributing dramatically to the quality of people’s life.” (*ibid.*, p. 1818)

“FINDING 2. Since the early 1970s, reported levels of satisfaction with life in the European countries have on average risen very slightly.” (*ibid.*, p. 1820)

“FINDING 3. Unemployed people are very unhappy.” (*ibid.*, p. 1822)

“FINDING 4. Reported happiness is high among those who are married, on high income, women, whites, the well-educated, the self-employed, the retired, and those looking after the home. Happiness is apparently U-shaped in age (minimizing around the 30s).” (*ibid.*, p. 1823)

“FINDING 5. Consistent with the patterns in happiness data, suicidal behaviour is more prevalent among men, the unemployed, and those with marital problems. Over the long run, as Britain has got richer, the suicide rate has declined (though this is not true for men since the 1970s). Rich countries apparently have more suicides.” (*ibid.*, p. 1825)

“FINDING 6. High unemployment may swell the number of people taking their own lives. Suicide data suggest that joblessness is a major source of distress.” (*ibid.*, p. 1825)

“FINDING 7. In Britain and America the level of job satisfaction is not rising over time.” (*ibid.*, p. 1827)

Findings 1 and 2 were related to early research on the happiness paradox. The main studies of the period were made by Easterlin (1995) and Kenny (1999). Easterlin’s (1995) aim was to update the results of his first study (1974)²³, and Kenny’s (1999)²⁴, to explore the causality between economic growth and happiness. They both found weak correlations between subjective well-being and incomes.

The early economists of happiness also attempted a few studies of the correlates of happiness (Findings 3 and 4). This approach was close to the psychologists’ practices (Chapter 2), as it consisted in analyzing different variables that were supposed to be correlated to subjective well-being data.

qualifiers. The second assumption is that verbal labels can be translated in a meaningful way into a numerical evaluation on a bounded scale, for example [0,1]” (van Praag and Frijters, 1999, p. 417). “The work originating from Leyden School has tried to operationalize the concepts of welfare, well-being, and so on, which are considered immeasurable and esoteric by most of the economic profession. With rather simple and inexpensive questions in large-scale surveys, considerable information has been found on feelings. At least the feeling of welfare and well-being may be “explained” by objectively measurable variables and by partial satisfaction measures with respect to aspects of life” (*ibid.*, p. 430).

²³ “Will raising the incomes of all increase the happiness of all? The answer to this question can now be given with somewhat greater assurance than twenty years ago (Easterlin, 1973; Easterlin, 1974). It is ‘no’ [...]. Put generally, happiness, or subjective well-being, varies directly with one’s own income and inversely with the incomes of others. Raising the incomes of all does not increase the happiness of all because the positive effect of higher income on subjective well-being is offset by the negative effect of higher living level norms brought about by the growth in incomes generally.” (Easterlin, 1995, p. 36)

²⁴ “Using poll data from the OECD countries over the last forty years, this paper argues that, if there is a link between growth and happiness, it does indeed appear to run from happiness to growth, not vice-versa.” (Kenny, 1999, p. 4)

Unlike psychologists, however, economists of happiness gave particular emphasis to the negative correlation between unemployment and happiness. Studies by Clark and Oswald (1994)²⁵, and Winkelman and Winkelman (1998)²⁶ were among the main ventures into this subject. Finding 7 shows that the early economics of happiness was also closely related to the analysis of job satisfaction made by labor economists. Interestingly, the analysis of job satisfaction data was as intensive as the analysis of subjective well-being during the early development of the subfield²⁷.

(2) Robert Frank's (1997) contribution to the symposium was focused on the implications of the happiness paradox for economic policy. It showed that general increases in "material consumption" had "little discernible" effects on subjective well being and claimed that the same resources could be used in alternative ways to "give rise to lasting increases in subjective well-being" (Frank, 1997, p. 1833)²⁸. Agreeing with Layard (1980), Frank proposed to correct the failures induced by "status concerns" by means of taxes²⁹:

²⁵ Clark and Oswald (1994) acknowledged that their paper was "in the psychologists' tradition" (Clark and Oswald, 1994, p. 648). It consisted in testing the principle of voluntary unemployment by exploring whether individuals were "effectively choosing to be unemployed" (*ibid.*) They used "data from the first sweep of the new British Household Panel Study to try to test whether, in the 1990s, unemployed people [we]re relatively happy or unhappy" (*ibid.*). Their main conclusion was that unemployed people in Great Britain in 1991 had "much lower levels of mental well-being than those in work" (Clark and Oswald, 1994, p. 658).

²⁶ Winkelman and Winkelman (1998) used panel data for Germany (German Socio-Economic Panel (GSOEP)) in order to test the direction of causality between unemployment and unhappiness: "Our main result is that the detrimental effect of unemployment persists after individual specific fixed effects are accounted for. We also shed some light on the causality issue. While panel data do not always solve the problem of causation, we present various types of evidence suggesting that unemployment in fact causes dissatisfaction." (Winkelman and Winkelman, 1998, p. 2)

²⁷ The main early studies of job satisfaction were made by Borjas (1977), Hamermesh (1977), Freeman (1978), Blanchflower and Oswald (1992), Clark and Oswald (1996), and Clark (1997). According to Freeman (1978), while job satisfaction was "the subject of popular attention" in both "sociology", "industrial psychology", and "theories of alienation" (Freeman, 1978, p. 135), it had "been studied by relatively few economists" (*ibid.*). It is worth noting that some of the economists involved in this research (like Clark and Oswald, 1996) were also involved in the early development of the economics of happiness. Clark and Oswald's (1996) analysis of job satisfaction data was intended as a contribution to the economics of happiness: "The purpose of this paper is to provide a test of the theory that happiness depends upon a comparison level of income. It does so by using new data on a random sample of workers who are asked how content they feel with their jobs. The data set thus provides self-reported levels of satisfaction" (Clark and Oswald, 1996, pp. 360, 375).

²⁸ Frank's (1997) paper consisted in the following eight short sections: "The Determinants of Subjective Well-Being", "Behavioural Evidence From the Economics Literature", "Does

“If our problem is that certain forms of private consumption currently seem more attractive to individuals than to society as a whole, the simplest solution is to make those forms less attractive by taxing them. Without raising our overall tax bill at all, a progressive consumption tax would change our incentives in precisely the desired ways.” (*ibid.*, pp. 1841-1842)

Another important feature of Frank’s contribution, which is also a common feature in the early economics of happiness, is that it presented the problem within the analytical framework of economics by considering the effects of positional consumption as negative externalities³⁰:

“The dependence of utility on relative consumption gives rise to what I have elsewhere called positional externalities. Analytically, these externalities are no different from ordinary environmental pollutants [...]. Most economists accept the existence of positional externalities as a purely descriptive matter. Yet many of these same economists may question whether such externalities are proper targets for public policy intervention. On the face of it, this is a curious position for the profession that has always insisted that ‘a taste for poetry is no better than a taste for pushpins’.” (*ibid.*, p. 1843)

Like Layard (1980), Frank moved beyond the boundaries of the economic profession by advocating for policies intended to shape the individuals’ preference structures. In Frank’s case, this shaping was to be attempted not only by taxes, but also by “less formal means” such as adopting new “social norms”:

“The progressive consumption tax is a simple policy measure that can *help mould the frame of reference* in mutually beneficial ways [...]. Even with such a tax, it will still

Anything Matter”, “If Spending Different Would Make Us Happier Why Don’t we Do It”, “One Solution: A Progressive Consumption Tax”, “Effects on Savings and Growth”, “Are Positional Externalities A Legitimate Concern of Tax Policy?”, and “Cash on the Table”.

²⁹ “Our spending patterns are in part a result of incomplete information about the extent to which we will adapt to different goods and experiences; and in part they are a result of the fact that many forms of consumption appear much more attractive to individuals than they are to society as a whole.” (Frank, 1997, p. 1839)

³⁰ The same interpretation was already present in his 1989 article (Section 1.4). On this subject, his ideas fit in with Layard’s (1980). Frank (1989) believes that the pursuit of social status must be regarded as a negative externality and redressed by taxing: “top policy advisers still insist that taxes must be avoided on efficiency grounds. Once we recognize the importance of context in utility analysis, however, it quickly becomes apparent that these concerns are misplaced [...]. Because individual consumers do not take positional externalities into account in their choices, the result is that such commodities appear much more attractive to individuals than to society as a whole. If the problem is that positional consumption goods are misleadingly attractive to individuals, the simplest solution is to make them less attractive by taxing them.” (Frank, 1989, p. 83)

prove useful to ameliorate consumption externalities through a variety of less formal means – adoption of social norms, choice of personal reference groups, introspection, and so on.” (*ibid.*, p. 1844, emphasis added)

(3) Finally, Yew-Kwang Ng’s “Case for Happiness, Cardinalism, and Interpersonal Comparability” (1997) approached the controversy from the viewpoint of a traditional welfare economist³¹. Like the other early economists of happiness, Ng wrote for affluent societies:

“for economically advanced countries (the number of which is increasing) there is evidence that for the whole society and in the long run (in real purchasing power terms) money does not buy happiness, or at least not much.” (Ng, 1997, p. 1849, references omitted)

Ng criticized the economists’ aversion towards the use of subjective concepts as well as their reluctance to make interpersonal comparisons of cardinal utilities (*ibid.*, p. 1848). Even if he avowed his preference for using “the more objective concepts when sufficient” (*ibid.*), he thought “the more subjective concepts and even their interpersonal comparison” should be used when needed (*ibid.*). Ng’s argument was developed on theoretical grounds and challenged the ordinalist turn in economics:

“The majority of economists are not that heavily brain-washed. They believe that cardinal utility is not meaningless [...]. The impossibility results of Arrow (1951/63), Sen (1970), Kemp and Ng (1976) and Parks (1976) show the impossibility of reasonable social decisions without interpersonal comparison of cardinal utility [...]. Thus, a paradox is created: we need interpersonal cardinal utilities but have difficulties measuring them.” (Ng, 1997, pp. 1852, 1855)

³¹ Ng’s paper was divided in: “I. Economists’ Preference for Preference and Ordinalism”, “II. Why is Happiness More Important”, “III. Cardinal Utility”, “IV. The Measurement and Interpersonal Comparison of Utility and Happiness”, and “V. The Acceptability of Monetary Measures of Welfare Changes Used by Economists”. This was not the first time Ng wrote about happiness and welfare economics. In 1978 he had modeled the effects of adaptations and aspirations in order to clarify “the Harrod-Hirsh’s concept of positional goods” (Ng, 1978, p. 575). Based on Easterlin’s findings (1974) he was among the first voices encouraging economists to turn to the analysis of subjective well-being data (Ng, 1978, pp. 584-585).

Chapter 2. 1999-2009: Making Happiness an Economic Subject

“With scarcely a shot fired, economics is in danger of surrendering the empirical study of subjective-well being to other disciplines. Since the 1940s, surveys reporting personal happiness or life satisfaction have been cumulating steadily. These data have been a gold mine to scholars in disciplines other than economics, and a recent research bibliography reports over 3000 contributions. Economic research on subjective welfare, however, has been stifled by the heavy hand of a disciplinary paradigm stipulating that what people say is irrelevant to understanding their feelings or behavior. Meantime, a number of distinguished psychologists – who do listen to what they say – have, in an imposing and encyclopedic volume, now laid claim to a new field they term hedonic psychology, which virtually writes off the relevance of economics to individual well-being. Among their tentative conclusions is that ‘happiness...is a personality trait with large heritable component’. ‘External factors’ such as economic circumstances are, at best, second or lower order determinants of subjective well-being, and, because of the dominance in determining happiness of genetics and personality, economic policy can have only a small long-term effect on well-being.” (Easterlin, 2002, p. ix, references omitted)

2.1. Introduction

The economics of happiness has changed with the turn of the century. It is no longer opposed to consumer choice theory or welfare economics but has been renewed with consideration to the psychological analysis of happiness data (*Hedonic Psychology*). In 2002, Richard Easterlin published *Happiness in Economics* (2002), a collection of the main contributions to the economic analysis of happiness since its beginnings in 1974. The introduction to this volume (quoted above) presents the main elements supporting the recent development of the economics of happiness. Economic studies of happiness data are presented as a challenge to both hedonic psychology, and the economists’ skepticism towards the analysis of subjective outcomes.

Chapter 2 presents the recent development of the economics of happiness. However, as this development is closely related to that of hedonic psychology, the chapter begins with an overview of the history of the psychologists' subfield (Section 2.2). Unlike economists of happiness, hedonic psychologists have been involved in the production, analysis, and improvement of subjective well-being measures for over three decades. This work has led them to develop new data based on immediate evaluations of hedonic experiences. Kahneman's concept of "objective happiness" (Section 2.3) resumes much of the psychologists' position towards the analysis of subjective well-being. Rather than using "top-down" measures of life satisfaction obtained from general surveys, psychologists prefer the "bottom-up" approach based on immediate evaluations of particular hedonic experiences.

The rest of the chapter deals with the current state of the economics of happiness. Section 2.4 shows that economists are interested in general measures of subjective well-being, preferring the analysis of life satisfaction data to measures of "objective happiness". Much of the recent development in the economics of happiness is indeed the result of the analysis of life satisfaction data available in panel studies such as the "British Household Panel Survey (BHPS)", the "German Socio-Economic Panel Study (GSOEP)", and the "Russian Longitudinal Monitoring Survey (RLMS)" (Clark *et al.*, 2008, p. 99). One of its main objectives is to fill the concept of utility with empirical content.

Section 2.5 proceeds by analyzing the economics of happiness in numbers. It presents an EconLit search for references containing either "happiness", "life satisfaction" or "well-being" in the title, and tests Clark *et al.*'s (2008) claim that the analysis of happiness data has become a "hot topic" among economists. It divides the recent literature into the following four groups of decreasing importance in terms of the number of publications: the analysis of the correlates of happiness, the analysis of the happiness paradox, the analysis of the history and the policy implications of the economics of happiness, and the Life Satisfaction Approach for valuing environmental goods.

The last two sections of the chapter deal with the analysis of the happiness paradox and the policy recommendations advanced in the recent literature. They show that unlike the early economics of happiness (1974-1999), the recent literature is being advanced as a complement rather than as an

alternative to standard (i.e. textbook) economic accounts. Section 2.6 presents part of the controversy on the happiness paradox running between Easterlin and other scholars of happiness during the past two decades. It shows that, unlike Easterlin's paradox (1974, 1995), the recent accounts tend to acknowledge a positive correlation between incomes and happiness, and to propose the use of subjective well-being data as proxies for utility. Section 2.7 concludes by presenting Richard Layard's (2005) interpretations of the analysis of happiness and public policy. It uses Layard's book to present the main traits of the recent economics of happiness and to mark the contrast with Layard's earlier arguments (Layard, 1980).

2.2. Hedonic Psychology: an overview

Since the turn of the century, the establishment of hedonic psychology has influenced the economics of happiness. The psychologists' subfield is now the outcome of more than seven decades of production and analysis of subjective well-being data, and its historical development can be divided in three main periods: from 1930 to 1967 (1), from 1967 to 1984 (2), and from 1984 to 1999 (3)¹.

(1) In his 1967 survey entitled "Correlates of Avowed Happiness", Warner Wilson examined more than 30 years of research by psychologists (1930-1967)². He showed that most of this research was devoted to the analysis of

¹ This division is admittedly crude. It is based on the publication dates of the main surveys made by hedonic psychologists themselves rather than on events marking the development of the subfield. It is useful, however, to present the main traits of its history which is the object of this section. A detailed analysis of the early development of subjective well-being studies appears in Erik Angner's dissertation: *Subjective Measures of Well-Being: A Philosophical Examination* (2005).

² Wilson's paper was divided in the following four sections: "Measurement, Reliability, and Validity" (pp. 294-295), "Dimensions of Hedonic Tone" (pp. 295-297), "Correlates of Avowed Happiness" (pp. 297-302), and "Discussion of General Aspects of the Findings" (pp. 302-305). It was based on Wilson's dissertation (1960) which was supervised by Donald T. Campbell to who we come back in Chapter 4. The first paper presented by Wilson (1967) was G. Watson's "Happiness among Adult Students of Education" (1930) published in the *Journal of Educational Psychology*. Watson studied self-estimates of happiness produced by 388 students. According to Wilson's account, Watson (1930) concluded that "wealth and education of parents, intelligence, and school success were not related to happiness, and that good health, high job satisfaction, a happy home, and good relationships with other people,

the reliability of subjective well-being data. This analysis consisted in evaluating subjective measures of well-being of the participants under study by correlating them with evaluations made by third parties (judges). According to Wilson, the results of these studies were satisfactory. They showed that “avowed happiness [could] be determined reliably” (Wilson, 1967, p. 294) and supported the subjectivist approach to the study of happiness:

“Data from these several studies suggest that judges agree poorly among themselves, that judges vary in the extent to which they agree with self-ratings, and that few judges agree closely with self-ratings. At the same time, the data show that most judges agree with self-ratings to some extent and that the pooling of judges’ estimates increases the agreement with self-ratings. These facts would seem, if anything, to support the validity of self-ratings.” (*ibid.*, p. 295)

Besides analyzing the concept of subjective well-being itself and proposing different methods for measuring it, the main studies of the period were focused on two types of studies: those focusing on the differences of “avowed happiness between individuals” (*ibid.*, p. 301), and those focusing on the “differences occurring within individuals across time” or “mood studies” (*ibid.*). Wilson’s survey explored the results of the first of these two kinds of studies which he summarized in the following terms:

“The happy person emerges as a young, healthy, well-educated, well-paid, extroverted, optimistic, worry-free, religious, married person with high self-esteem, high job morale, modest aspirations, of either sex and of a wide range of intelligence.” (*ibid.*, p. 294)

(2) According to Ed Diener’s (1984) survey of the discipline, “over 700 studies” were published since Wilson’s review and his own (Diener, 1984, p. 542)³. This shows quite clearly that the psychological analysis of subjective

including a spouse, were conducive to happiness” (Wilson, 1967, p. 297). The analysis of the effects of aspirations was also explored during the early days of hedonic psychology: “aspiration would be negatively correlated with happiness if success were held constant, and [...] success would be positively correlated with happiness if aspiration were held constant” (Wilson, 1967, p. 302).

³ Diener’s survey was divided in three long sections: “Defining and Measuring Subjective Well-Being” (pp. 543-552), “Influences on Subjective Well-Being” (pp. 552-562), and “Theory” (pp. 562-569). He resumed subjective well-being studies in the following terms: “The area of subjective well-being has three hallmarks First, it is subjective [...]. Second, subjective well-being includes positive measures. It is not just the the absence of negative factors, as is true of most measures of mental health [...]. Third, the subjective well-being measures typically include a global assessment of all aspects of a person’s life. Although

well-being data was already intensive decades before the turn of the century. In regards to the main findings by psychologists between 1967 and 1984, Diener showed that the conclusions drawn by Wilson (1967) remained valid, though subject to a few qualifications. Unlike Wilson, Diener showed that the analysis of subjective well-being data revealed weak correlations between subjective well-being (SWB) and demographic variables. Consequently, researchers had turned to the analysis of correlates such as personality and attitudes:

“A number of investigators have noted with dismay the small proportion of variance that can be accounted for with demographic variables. This has led some to look elsewhere for more potent variables, to fields such as personality or attitudes [...]. However, SWB is probably determined by a large number of factors that can be conceptualized at several levels of analysis and it is perhaps unrealistic to hope that a few variables will be of overwhelming importance.” (*ibid.*, p. 561)

It is worth noting that it was also during this period (1967-1984) that Harry Helson’s *Adaptation-Level Theory* was applied to the analysis of happiness (1971) and that the *Social Indicators Research* journal was launched (1974)⁴. However, according to Diener’s (1984) survey, the production and analysis of subjective well-being data was not sufficiently advanced to fully explain the phenomenon of adaptation. He showed that positive factors such as health or income did correlate with subjective well-being and that it seemed “unlikely that people [would] completely adapt to all conditions” (*ibid.*, p. 567). He thought it was possible that adaptation reduced but did not eliminate the effects of some changes, and that the “limits of the parameters” that influenced adaptation were not yet properly understood (*ibid.*)⁵.

affect or satisfaction within a certain domain may be assessed, the emphasis is usually placed on an integrated judgment of the person’s life.” (pp. 543-544)

⁴ Hedonic psychology is now the outcome of numerous publications in the *Social Indicators Research* journal (founded in 1974). Harry Helson’s *Adaptation-Level Theory* is extensively presented in Chapter 4.

⁵ Diener’s definition of adaptation was the following: “Adaptation to events means that when they first occur, events can produce either happiness or unhappiness, depending on whether they are good or bad. However, over time the events lose their power to evoke affect. The person adapts to good conditions so they no longer evoke happiness, and a similar adaptation process occurs for bad events [...]. In social comparison theory, one uses other people as a standard [...]. In adaptation [...], a person’s past life is used to set the standard.” (Diener, 1984, p. 567, references omitted)

(3) In 1999, the year of the foundation of hedonic psychology, E. Diener, E. Suh, R. Lucas and H. Smith published “Subjective Well-Being: Three Decades of Progress”⁶. This survey confirmed that demographic variables had “surprisingly small effects” on subjective well-being (Diener *et al.*, 1999, p. 276). Besides this, the survey presented the main advances in regards to the conception, production, and analysis of subjective well-being data. After “three decades of progress”, subjective well-being was no longer regarded by psychologists as a simple construct, but as an area of scientific research formed by the analysis of different kinds of evaluations:

“we define SWB as a general area of scientific research rather than a simple scientific construct [...]. Moods and emotions, which together are labeled affect, represent people’s on-line evaluations of the events that occur in their lives [...]. In addition to studying affective reactions, SWB researchers are interested in cognitive evaluations of life satisfaction.” (*ibid.*, pp. 276-277)

If many of the findings presented by Wilson (1967) remained valid after three decades, Diener *et al.* (1999) showed that psychologists had shifted their focus from the analysis of correlations between demographic variables and subjective well-being data obtained from surveys (life satisfaction data), to the production and analysis of new data. Consequently, the new program was supposed to be based on more sophisticated measurements “recognizing the multifaceted nature of emotions and SWB” (*ibid.*, p. 295). The development of even better methods for measuring subjective well-being was indeed among the main objectives proposed for furthering research in the subfield.

2.3. Hedonic psychologists and “objective happiness”

As shown in the previous section, the psychological analysis of subjective well-being data has been largely dedicated to improve the measurement of happiness. Hedonic psychologists have developed new techniques for measuring happiness based on evaluations of immediate hedonic experiences

⁶ Diener *et al.*’s survey was structured as follows: “The Field of Subjective Well-Being” (pp. 276-277), “The Components of Subjective Well-Being” (p. 277), “Research Method” (pp. 277-278), “Theory” (pp. 278-286), “Wilson’s Conclusions Reexamined” (pp. 286-294), and “Take-Home Message” (pp. 294-295).

rather than retrospective data collected in surveys. They even propose these new kinds of measures as targets for social policy:

“we propose that nations should begin monitoring pleasure and pain through on-line experience recording among samples of respondents to complement existing social indicators, and to provide a more direct assessment of the final outcome about which people are most concerned.” (Kahneman *et al.*, 1999, p. xii)⁷

Given the quality problems of life satisfaction data, hedonic psychologists have thought of new ways of quantifying happiness. In order to correct the retrospection biases of survey data, the new methods attempt to isolate the immediate components of the evaluations (1). Two new methods have been proposed: the Experience Sampling Method (2), and the Day Reconstruction Method (3).

(1) In “Objective Happiness” (1999), the first chapter of the *Foundations of Hedonic Psychology*, Kahneman presented a new concept of happiness based on the isolation of the affective component of a hedonic experience. This affective component is measured by means of immediate evaluations and is supposed to give an objective basis to the analysis of subjective well-being⁸. There is indeed a high contrast between the new methods of measurement (objective happiness) and retrospective evaluations of life satisfaction (subjective happiness):

“An assessment of a person’s objective happiness over a period of time can be derived from a dense record of the quality of experience at each point (instant utility) [...]. Subjective happiness is assessed by asking respondents to state how happy they are. Objective happiness is derived from a record of instant utility over the relevant period.” (Kahneman, 1999a, pp. 3, 5)

⁷ *Hedonic Psychology* was institutionalized in 1999 by Daniel Kahneman, Ed Diener and Norbert Schwarz (eds.) by means of a collective volume entitled *Well-Being: The Foundations of Hedonic Psychology* which defined the discipline as “the study of what makes experiences of life pleasant or unpleasant” (Kahneman, Diener and Schwarz, 1999, p. ix).

⁸ According to Kahneman (1999a), these measurements of happiness are objective so far as they can be established by an observer: “Objective happiness, of course, is ultimately based on subjective data [...]. It is labeled objective because the aggregation of instant utility is governed by a logical rule and could in principle be done by any observer with access to the temporal profile of instant utility [...]. More than a century ago, the economist Francis Edgeworth (1881) wrote of using an “hedonimeter” in just this way” (Kahneman, 1999a, p. 5).

Hedonic psychologists have developed this “hedonimetric” approach in order to calculate sums (*total utilities*) of “on-line” evaluations of hedonic experiences (*instant utilities*). These sums have been found to be systematically different from retrospective evaluations of the same experiences (*remembered utilities*). According to the new measures, retrospective evaluations of subjective well-being can be accurately predicted by means of simple estimates based on just two outcomes: the peak and the end of a hedonic experience⁹:

“several studies have shown that people’s intuitive evaluations of their own experiences and of the experiences of others deviate sharply from temporal integration [...]. The retrospective evaluations and choices observed in these studies generally conformed to a simple rule of Peak-End evaluation.” (*ibid.*, p. 19)

For these psychologists, retrospective evaluations disregard the duration of an experience. In view of these biases, these evaluations are considered poor indicators of subjective well-being:

“A significant corollary of Peak-End evaluation is duration neglect. Duration was a factor in all the studies [...], but this variable had little or no effect on retrospective global evaluations [...]. In the context of measurement of well-being, these results convey a discouraging message: retrospective evaluations of experiences are likely to provide erroneous estimates of the ‘true’ total utility of past experiences.” (*ibid.*, pp. 19-20)

(2) The Experience Sampling Method (ESM) is the main method by means of which Kahneman and other hedonic psychologists have developed the “objective happiness” approach to subjective well-being measurements (Kahneman, 1999a; Kahneman *et al.*, 2004a). Free from biases of

⁹ These results are also the outcome of the research program of behavioral economists, which is focused on decision making processes rather than the analysis of SWB itself (Chapter 4). Kahneman, Wakker and Sarin (1997) present a number of experiences showing that total utilities differ systematically from remembered utilities (these remembered utilities are also considered “decision utilities” as they are supposed to underlie decision making). Kahneman *et al.* (1997) studied the peak-end principle through the analysis of 4 distinct experiments: (1) exposing participants to 16 short videos (45 and 120 seconds), 8 pleasant and 8 unpleasant; (2) asking patients undergoing colonoscopies to assess their pain level every 60 seconds; (3) having participants immerse their hands in a basin of water at 14°C for 60 seconds, then 90 seconds (increasing the temperature of the water to 15°C during the last 30 seconds without letting the participants know); (4) exposing participants to various sounds: an unpleasant sound (78db.) for 10 seconds, then the same experience followed by 4 extra seconds of the same sound at a lower level (66db.) In each of these four experiments, participants were asked to give on-line evaluations and retrospective evaluations. The experiments showed that retrospective evaluations could be predicted by *peak-end* estimations.

retrospection, the ESM is supposed to be the most effective method for gathering subjective well-being data. It produces evaluations of the immediate experiences in real time and is described in the following terms by D. Kahneman, A. Krueger, D. Schkade, N. Schwarz and A. Stone (2004a):

“ESM is carried out by supplying subjects with a electronic diary (e.g., a specially programmed palm pilot) that beeps at random times during a day and asks respondents to describe what they were doing just before the prompt. The electronic diary also asks respondents to indicate the intensity of various feelings.” (Kahneman *et al.*, 2004a, p. 431)

The Experience Sampling Method is considered the reference method for obtaining subjective well-being data. It is intended to overcome problems of “imperfect recall and duration neglect” (*ibid.*) and is “the current gold standard for measurement of well-being in the Edgeworth tradition” (*ibid.*).

(3) The Day Reconstruction Method (DRM) has been developed as a considerably less expensive way of minimizing the biases of life satisfaction data. According to Kahneman *et al.* (*Science*, 2004b), the Day Reconstruction Method proceeds by demanding detailed reconstructions of the respondents’ daily activities after which it requests them to evaluate their hedonic experiences. This method is intended to produce well-being data as accurate as the data obtained by means of the Experience Sampling Method. The new approach combines time allocation studies with the elicitation of hedonic experiences of the participants:

“Experience sampling is the gold standard to which DRM results must be compared; the DRM is intended to reproduce the information that would be collected by probing experiences in real time [...]. DRM respondents first revive memories of the previous day by constructing a diary consisting of a sequence of episodes. Then, they describe each episode by answering questions about the situation and the feelings that they experienced, as in experience sampling [...]. Evoking the context of the previous day is intended to elicit specific and recent memories, thereby reducing errors and biases of recall.” (Kahneman *et al.*, 2004b, pp. 1776-1777)¹⁰

¹⁰ Kahneman *et al.* (2004b) present the DRM in the following four points: “(i) the elicitation of a detailed description of the previous day in the respondent’s life; (ii) the goal of approximating the results of continuous, real-time experiences measurement; (iii) procedures designed to support accurate retrieval of specific episodes; and, (iv) multidimensional description of the affect experienced in each episode” (Kahneman *et al.*, 2004b, p. 1777).

In conclusion, instead of assessing social welfare using general measurements of well-being (top-down approach); psychologists prefer measurements based on real time evaluations (bottom-up approach):

“The main advantages of our bottom-up approach vis-à-vis top-down life satisfaction measures are: (i) it avoids some of the biases (e.g. duration neglect) of global retrospective evaluations; and (ii) it is connected to the allocation of time, which can be measured.” (Kahneman *et al.*, 2004a, p. 433)

2.4. Joyful economists and life satisfaction

The development of new techniques for measuring subjective well-being and the correction of biases of retrospection are not main concerns of economists of happiness. As noted above (Introduction), these economists tend to avoid philosophical or even technical definitions of the concept of happiness:

“Because happiness is such an elusive concept, it makes little sense to proceed by trying to define what happiness is. Fortunately, there is a useful way out. Instead of trying to determine what happiness is from the outside, one can ask the individuals how happy they feel themselves to be.” (Frey and Stutzer, 2002a, p. 4)

According to Frey and Stutzer (2002a), economists do not interpret subjective well-being data in the way psychologists do. These (joyful) economists have found their happiness in the form of retrospective evaluations of life satisfaction. Unlike psychologists, economists of happiness are interested in the cognitive processes involved in these evaluations. They admit that “subjectively oriented concepts are necessarily less precise” than Kahneman’s “objective happiness” (*ibid.*, p. 6). The impreciseness of life satisfaction data is, however, not a problem for these economists¹¹:

“precisely because cognitive factors enter into subjective happiness, these concepts are useful for issues connected with happiness, which have a bearing on social aspects. [...]

¹¹ In keeping with Frey and Stutzer’s claim, Helliwell (2006) thinks that it is more relevant to study life satisfaction than objective happiness: “Kahneman argues that primary research and policy attention should be given to experienced utility, with the apparent dominance of remembered utility as a driver of individual decisions to be treated as a mistake. I am rather more inclined to conclude that if remembered utility produces a consistent set of forward-looking decisions and backward-looking evaluations, then it should be given pride of place by analysts and policy makers.” (Helliwell, 2006, p. C35)

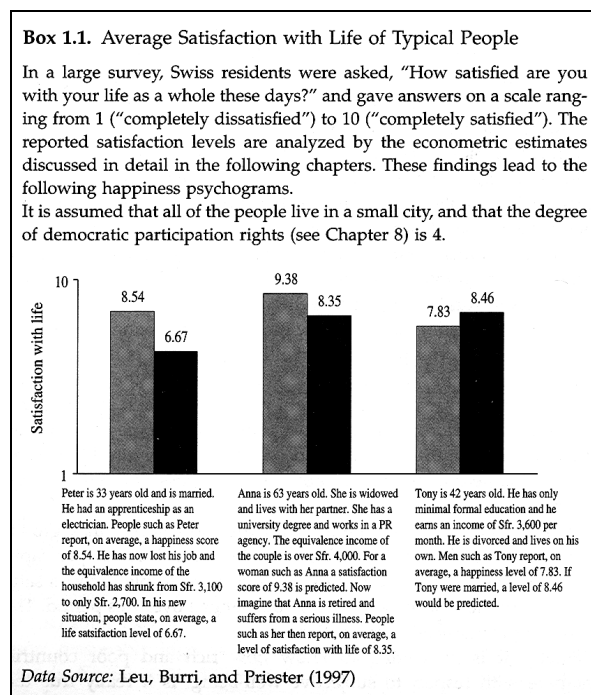
physiological and moment-based measures rely on strongly normative judgments in the sense that happiness is assessed according to fixed rules, although our attitude towards particular pleasures and pains is not a priori given. Individual well-being is not an isolated feeling, but strongly depends on the conditions in which the persons concerned live. Thus, social comparisons are of great importance and have to be taken into account.” (*ibid.*)

Unlike hedonic psychologists, economists of happiness are interested in the cognitive aspects of subjective well-being. These aspects are supposed to be reflected in the general data of life satisfaction¹². Consequently, the economists’ studies are based on subjective well-being data obtained from questionnaires asking individuals to assess their happiness on a general basis:

“They reside within the individual and do not claim or want to be objective [...]. They globally assess happiness over the whole life domain and are not restricted to particular areas, such as job satisfaction or health.” (Frey et Stutzer, 2002a, p. 30)

The following figure presents Frey and Stutzer’s summary of a typical economic approach.

Figure 2.1 (Frey and Stutzer, 2002a, p. 7)



¹² Drawing on the findings of hedonic psychologists, Frey and Stutzer present subjective well-being as “an attitude consisting of the two basic aspects of cognition and affect. ‘Affect’ is the label attached to moods and emotions. Affect represents people’s instant evaluation of the events that occur in their lives. The cognitive component refers to the rational or intellectual aspects of subjective well-being. It is usually assessed with measures of satisfaction.” (Frey et Stutzer, 2002b, p. 11)

It is worth noting that unlike psychologists, economists of happiness are not interested in the effects of every single variable affecting subjective well-being. They rather take into account the variables related to economic outcomes (*ibid.*, p. 50). Consequently, they consider personality traits (as “being optimistic”) and demographic factors (as “being married”) only as far as they depend on economic variables. Frey and Stutzer show that the impact of these variables can be adequately captured by a direct analysis of the impact of economic variables on happiness:

“The extent of a person’s optimism is likely to be higher when the economy is booming than when it is depressed [...]. Demographic factors, such as age and gender, do not depend on the state of the economy. But marital status to some extent does: Getting married (or getting a divorce) at times depends to some degree on economic conditions [...]. To the extent that personality and demographic factors depend on economic conditions, their influence on happiness is captured by directly analyzing the effect of economic factors on happiness.” (*ibid.*)

Like Kahneman, these economists are looking for a cardinal measure of utility. Unlike hedonic psychologists, however, they approach subjective well-being by means of life satisfaction data¹³:

“utility has been filled with content again: utility can and should be cardinally measured in the form of subjective well-being.” (Frey and Stutzer, 2002b, p. 43)

2.5. Joyful economists in numbers

As noted above (Introduction), economists of happiness tend to emphasize the growing number of publications devoted to the analysis of subjective well-being data. A. Clark, P. Frijters and M. Shields (2008), consider the growth of the literature to be exponential and the analysis of happiness to be a “hot topic” in economics:

¹³ Unlike hedonic psychologists, economists of happiness tend to compare “average happiness”. Di Tella and MacCulloch (2006) claim that this kind of comparisons are not subject to L. Robbins’ well-known criticism: “once the analyst moves beyond comparing just two individuals and instead starts focusing on groups, the problems of comparing happiness are much reduced [...]. This is important because a large fraction of the happiness literature in economics is based on comparing average happiness scores for large numbers of people” (Di Tella and MacCulloch, 2006, p. 29).

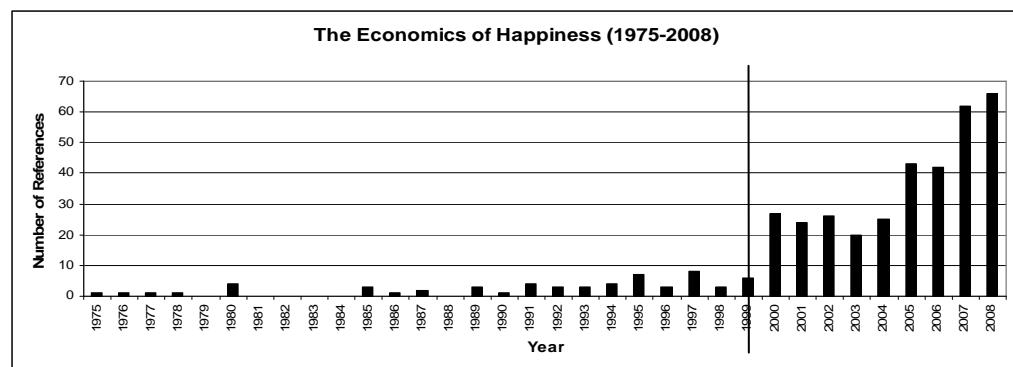
“Studying the causes and correlates of human happiness has become one of the hot topics in economics over the last decade, with both the size and depth of the literature increasing at an exponential rate.” (Clark *et al.*, 2008, p. 95, reference omitted)

It is clear that in contrast to the small number of publications of the first period (1974-1999), the recent development of the economics of happiness seems to be outstanding. According to Clark *et al.* (2008), this would testify that “an increasing number of economists believe that self-reported well-being data contain valuable information that can complement our understanding of individual behavior” (*ibid.*, p. 106). However, this statement should be carefully read for, as rightly acknowledged by Frey (2008), economists of happiness “are too engaged in the topic to be objective enough” (Frey, 2008, p. xii)¹⁴. Clark *et al.* (2008), justify their enthusiastic claims with the following search:

“A search of EconLit for journal articles with either ‘happiness’, ‘life satisfaction’ or ‘well-being’ in the title, identifies 465 published articles between 1960 and 2006. Of these, 363 (78 percent) have been published since 1995, 285 (61 percent) have been published since 2000, and one third of the literature (37 percent, or 173 articles) has appeared in print in just the last three years.” (Clark *et al.*, 2008, p. 106)

The rest of this section analyzes the recent development of the economics of happiness based on a search similar to Clark *et al.*’s (2008) for EconLit references between 1975 and 2008 (Fig. 2.2).

Figure 2.2 (source: EconLit)



¹⁴ This does not stop Frey (2008) from entitling his book *Happiness: A Revolution in Economics* which opens with the following statement: “Economics is undergoing a remarkable new development, which may even be called revolutionary. This development is likely to change economics substantially in the future [...]. Happiness research is, to some extent, an exception. Its findings are slowly being taken into account in standard economics. Indeed, research on happiness has already become a hot topic, particularly among young economics scholars.” (Frey, 2008, p. ix)

At first sight, this search seems to support Clark *et al.*'s (2008) data. However, in order to sense the magnitude of the economics of happiness as a subfield (rather than the enthusiasm of its practitioners) the analysis must be taken a step further. Figure 2.3 presents the data for the more recent publications (1999-2008) in comparison to all EconLit references for the same period.

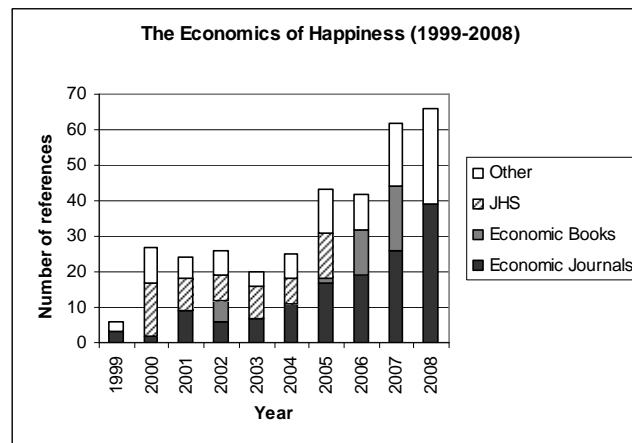
Figure 2.3 (source: EconLit)



The percentages of the happiness references for each year are respectively 0,36%, 1,49%, 1,50%, 1,67%, 1,25%, 1,67%, 2,81%, 2,73%, 4,16%, and 4,73%. Without discussing whether these percentages are “hot” or “revolutionary”, it is interesting to note that the happiness literature has been growing in relative terms. However, further analyses of the data show that not all references correspond to economic journals, suggesting that Clark *et al.*'s (2008) search overestimates the number of publications in the subfield. Figure 2.4 shows the results by type of reference. It takes into account the journals and books in which the papers were published and groups the references according to whether they appeared in “economics journals”, “economics books”, the *Journal of Happiness Studies* or other sources¹⁵.

¹⁵ The search presented here (June 2009) gave 411 EconLit references from 1975 to 2009. The category “other” contains references which do not correspond to economic analyses. Working Papers were also grouped in the “other” category in order to avoid double (or sometimes triple) counting of the same papers.

Figure 2.4 (source: EconLit)



As shown in the figure, several journal articles for years 2000 to 2005 were published in the *Journal of Happiness Studies* (JHS), which is not in the database from 2006 onwards¹⁶. Most of these references are not related to the analysis of economic subjects and should not be considered as part of the subfield. References in “economic journals” and “economic books”, give a more realistic picture of its development which is discrete enough to be presented extensively (references are listed in an Appendix at the end of the dissertation including the available references for 2009)¹⁷. References in economic books are substantial for years 2002, 2006 and 2007 (Fig. 2.4). They correspond respectively to chapters in Easterlin’s *Happiness in Economics*, Ng and Ho’s *Happiness and Public Policy*, and Bruni and Porta’s *Handbook on the Economics of Happiness*¹⁸.

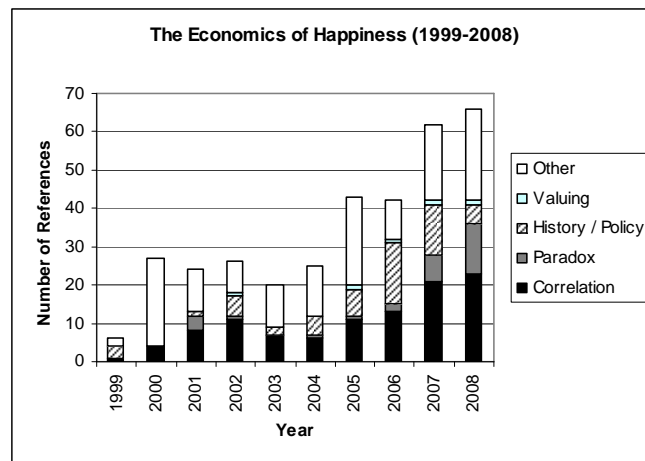
¹⁶ The *Journal of Happiness Studies* (JHS: Springer) was launched in 2000 and still exists. The fact that it is no longer part of the EconLit database seems to reflect the fact that it is not an economic journal, though some economists of happiness as Easterlin, Frey or Stutzer have papers published in the JHS. The analysis of the economics of happiness by subjects (Fig. 2.5) takes into account the literature published in the JHS which is directly related to economics.

¹⁷ It is worth looking to these references in order to have an idea of the main contributors to the economics of happiness as well as the publication sources. The list of references clearly shows that even this “filtered” search overestimates the work of economists of happiness. The fact of counting a few more references may, however, correct the omission of some pertinent (however few) other references which do not appear as they do not contain “happiness”, “life satisfaction” or “well-being” in the title (as Helliwell (2006), or Frank (2005)).

¹⁸ Easterlin’s (2002) volume reprints the main literature of the early development of the subfield (1974-1999). The chapters in the volume are: Easterlin (1974), Friedman and McCabe (1996), Scitovsky (1996), Ng (1978), Frank (1997), Layard (1980), van Praag and Frijters (1999), van de Stadt *et al.* (1985), Oswald (1997), Clark and Oswald (1994), Di Tella *et al.* (2001), Frey and Stutzer (2000), Easterlin (2001), and Sen (1996). Ng and Ho’s volume is the outcome of “An International Interdisciplinary Conference on “Progress, Happiness,

Does this new picture (Fig 2.4) tells us something about the recent development of the subfield? Yes, if complemented with the analysis of the contents of the books and papers considered. Figure 2.5 shows the references divided by subjects. It groups the literature in four categories: (i) the analysis of the correlates of happiness, (ii) the analysis of the happiness paradox (subjective well-being/income), (iii) the analysis of the history and policy implications of the economics of happiness, and (iv) the Life Satisfaction Approach for valuing environmental goods.

Figure 2.5 (source: EconLit)



The following two sections are devoted to the analysis of the happiness paradox (Section 2.5) as well as that of happiness and public policy (Section 2.6). Chapter 6 returns to the rest of the literature by exploring the analysis of the correlates of happiness and the Life Satisfaction Approach for valuing environmental goods.

2.6. Joyful economists and the happiness paradox

The analysis of Easterlin’s paradox has been central in the development of the economics of happiness and has been growing during the last years (Fig. 2.5). There has been a long and interesting controversy between Richard Easterlin

and Public Policy” held in Lingan University in 2005. It contains twelve chapters divided in four parts: (1) “A Historical Overview of the State of Well Being of Humanity”, (2) “Determinants and Origins of Happiness: The Conceptual Framework”, (3) “Area and Policy Case Studies”, and (4) “Implications and Conclusions”. Bruni and Porta’s (2007) book was presented in the introductory chapter.

and sociologist Ruut Veenhoven about the effects of income variations on subjective well-being (1). The recent literature on the paradox rejects Easterlin's findings suggesting that income does raise happiness. Rather than being built against economic theories of consumer behavior and welfare, the new literature brings the economics of happiness close to mainstream economics by adopting the framework of utility functions and using subjective well-being data as proxies for utility (2).

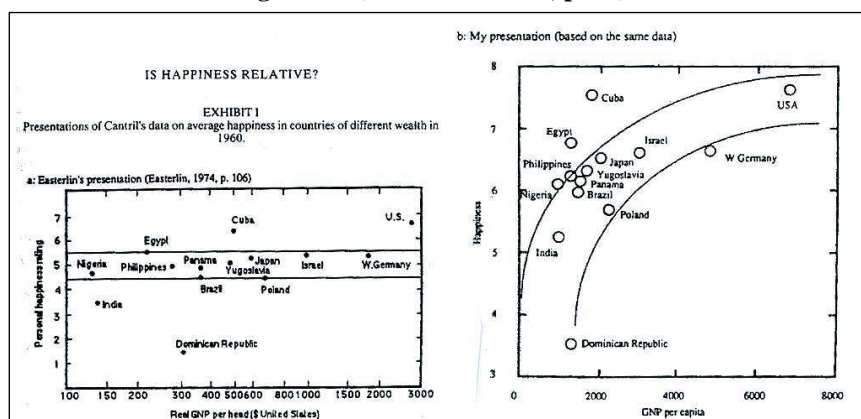
(1) In his paper entitled "Is happiness relative?" (1991), Veenhoven openly attacked both the "relative happiness" theories of psychologists, and Easterlin's paradox¹⁹:

"The theory that happiness is relative is based on three postulates: (1) happiness results from comparison, (2) standards of comparison adjust, (3) standards of comparison are arbitrary constructs [...]. Recent investigations on happiness (in the sense of life-satisfaction) claim support for this old theory. Happiness is reported to be as high in poor countries as it is in rich countries, no less among paralyzed accident victims than it is among lottery winners and unrelated to stable living conditions." (Veenhoven, 1991, p. 1, references omitted)

Veenhoven claimed that the relationship between incomes and happiness was positive rather than "uncertain" and that wealth was subject to a "law of diminishing happiness returns". He supported these claims by analyzing Easterlin's (1974) data and comparing the economist's representation with his own figure (Fig. 2.6).

¹⁹ R. Veenhoven has been a very important actor in the development of happiness studies. He is the director of the World Database of Happiness, founded the *Journal of Happiness Studies* (with E. Diener and A. Michalos), and wrote the first paper in the first issue of the JHS: "The Four Qualities of Life", in which he concluded that the most inclusive measure of happiness is "how long and happily people live" (Veenhoven, 2000, p. 1). This section is based on the controversy between Easterlin and Veenhoven. See *Social Indicators Research* and the *Journal of Happiness Studies* for general literature on the analysis of subjective well-being data.

Figure 2.6 (Veenhoven 1991, p. 11)



“I computed product-moment correlations. These are +0.51 and +0.59 respectively. I would not call that relationship ‘uncertain’ as Easterlin does [...]. How do these correlations fit the presentation in exhibit Ia [left]? That presentation is simply misleading. Easterlin played the classic trick of scales: the scale for national wealth is 2.5 times longer than the happiness scale and logarithmic. If both variables are plotted on equal scales, quite a different picture emerges. See exhibit Ib [right]. Now we not only see a clear positive relationship, but also a curvilinear pattern, which suggests that wealth is subject to a law of diminishing happiness returns.” (Veenhoven, 1991, p. 10)²⁰

A decade later, as a reaction to the “easy” acceptance of Easterlin’s paradox, Hagerty and Veenhoven (2003) furthered Veenhoven’s (1991) claims by advancing their concerns about the implications of such a paradox in terms of the design of public policies:

“It is worrisome that Easterlin’s original work has been accepted so easily, since he himself warned that his data were few and unreliable [...] [A]ccepting the conclusion that economic growth has no effect on happiness would have sweeping implications for

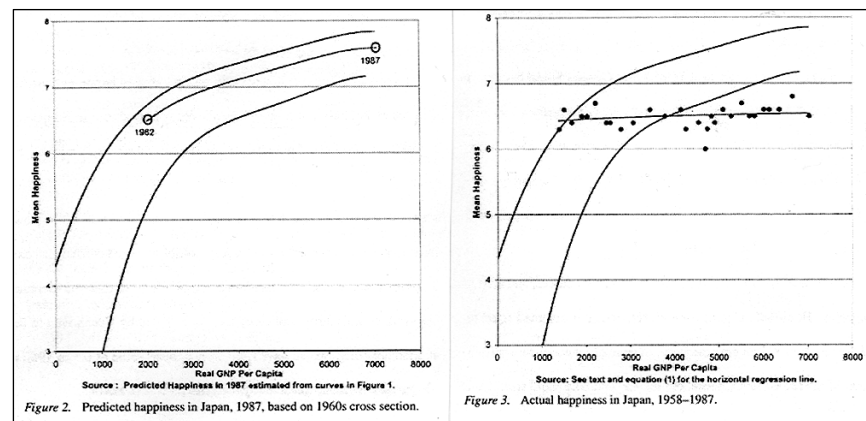
²⁰ In a later paper: “Is Happiness a Trait?” (1994), Veenhoven complemented his position by rejecting the theories which considered the feelings of happiness to be independent from their social and economic contexts: “If happiness is a trait-like disposition rather than a state-like evaluation of life, happiness must remain largely the same in different situations. People living in good conditions can then be expected to be equally happy as people in bad conditions. Change for the better or worse will not render them any happier or unhappier. Some authors claim that this is exactly what empirical research has shown: a remarkable lack of correspondence between ‘objective’ living conditions and ‘subjective’ appreciation of life” (Veenhoven, 1994, p. 115). Veenhoven’s (1994) findings rejected the assumptions about the stability of happiness in the long-run: “These findings knock the bottom out of the argument that happiness is too static a matter to be influenced by social policy. There is thus still sense in pursuing greater happiness for a greater number [...]. The findings also refute the related claim that happiness is not a useful social indicator. Though happiness may be a slowly reacting instrument, it does reflect long-term change for better or worse.” (Veenhoven, 1994, p. 146)

national policy, suggesting that a focus on economic growth will not benefit the long run happiness of citizens.” (Hagerty and Veenhoven, 2003, pp. 2-3)

In 2005, Easterlin replied by showing that, despite economic growth, countries were not becoming happier. He defended his paradox based on the analysis of time series of life satisfaction data and reproduced a new chart showing Japan’s (famous) data for subjective well-being and income (Fig. 2.7):

“Between 1962 and 1987 Japan experienced unprecedented economic growth, with GNP per capita (corrected for price level change) multiplying by 3.5-fold and rising from 22 to 77 percent of the United States level in 1962. If, with this real income growth Japan had followed the trajectory implied by the curves fitted by Veenhoven to the 1960s cross section [Fig. 3.6], then happiness would have risen from a mean value of 6.5 in 1962 to about 7.7 in 1987 (Figure 2) [left]. Is this what actually happened? [...] The answer is no, happiness remained constant despite Japan’s remarkable economic growth [right].” (Easterlin, 2005a, p. 249)

Figure 2.7 (Easterlin, 2005a, p. 249)



In “Feeding the Illusion of Growth and Happiness” (2005b), Easterlin insisted with irony. In spite of the promising development of the economics of happiness, he showed that the program built around his paradox was still far from being accepted by mainstream economists:

“It is surprising to learn that my conclusions have been accepted, let alone ‘easily’. I keep company with economists and I can assure H-V [Hagerty-Veenhoven] that there is very little doubt among my colleagues that focusing on economic growth will increase well-being, evidence to the contrary notwithstanding. Since economics is the most influential

social science in public policy formation, and promises to be so for some time, I think H-V can safely put their anxieties aside.” (Easterlin, 2005b, p. 441)

All along this controversy, Easterlin has been attempting to impose his position concerning the effects of economic policy on subjective well-being. He claims that despite the happiness paradox, there are still possibilities of improving subjective well-being by means of public policies focused on non pecuniary domains. Easterlin’s argument consists in claiming that both hedonic adaptation and social comparisons are “less complete with regards to family circumstances and health than in the material goods domain” (Easterlin, 2003, p. 11181). His aim has been to build a “better theory” of happiness, and his position consists in claiming that “neither the prevailing psychological nor economic theories are consistent with accumulating survey evidence on happiness” (*ibid.*, p. 11176).

“Instead of straining to feed the illusion that a focus on growth creates happiness, one needs to develop an empirically tested causal model that includes the life satisfaction derived from multiple sources – not just material goods, but also family life, health, work utility, and the like. A better understanding of the causes of happiness will provide a more secure foundation for policy recommendations.” (Easterlin, 2005b, p. 441)

(2) The analysis of the recent literature shows that the happiness paradox is one of the main issues in the development of the economics of happiness. However, the latest accounts tend to contradict Easterlin’s (1974, 1995, 2003, 2005a-b) position. Clark, Frijters and Shields’ paper on “Relative Incomes, Happiness and Utility” (2008) gives an overview of the subfield highlighting the fact that the recent literature finds positive correlations between subjective well-being and incomes. It presents a “parallel body of work” producing “a large amount of evidence suggesting that money does matter” (Clark *et al.*, 2008, p. 97)²¹. This “second literature” is summarized in three stylized facts:

“1) A regression of happiness on income using cross-section survey data from one country (with or without standard demographic controls) generally produces a significant positive

²¹ This section is mainly based on Clark *et al.*’s (2008) 50 page paper which is highly documented and intended as a sort of survey of the main findings in the subfield. The outline of Clark *et al.*’s paper is the following: “1. Income, Happiness, and the Easterlin Paradox” (pp. 95-99), “2. Explaining the Easterlin Paradox by Relative Income” (pp. 99-106); “3. Evidence of Comparisons using Happiness Data” (pp. 106-115), “Is Happiness Related to Utility” (pp. 115-122), “5. Some Implications for Economic Theory and Policy Design” (pp. 122-136), and “6. Conclusions” (pp. 136-138).

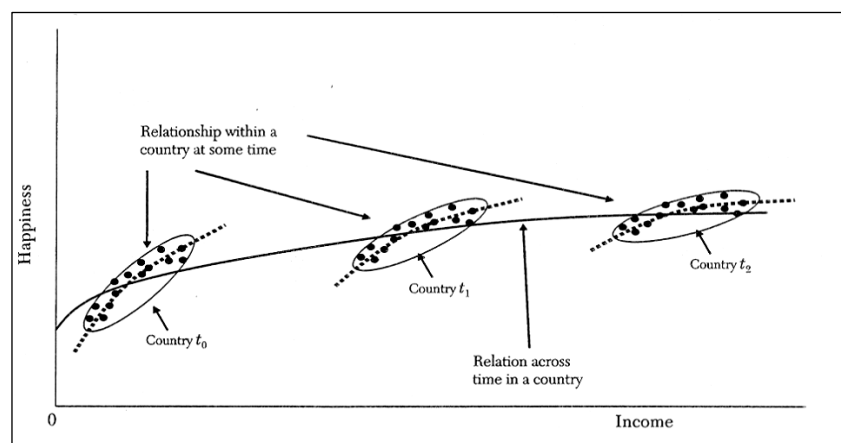
estimated coefficient on income. This holds for both developed and developing countries. However, the income-happiness slope is larger in developing or transition than in developed economies.” (*ibid.*, references omitted)

“2) Recent work has used panel data to control for unobserved individual fixed effects, such as personality traits, and concludes that changes in real incomes are correlated with changes in happiness. Further, a number of these studies have been able to utilize exogenous variations in income to establish more firmly the *causal* effect of income on happiness [...]” (*ibid.*, pp. 97-98, references omitted)

“3) Recent detailed studies of the ‘macroeconomics of happiness’ using very large samples and cross-time cross-country models that control for country fixed-effects, have shown that happiness co-moves with macroeconomic variables including GDP, GDP growth, and inflation.” (*ibid.*, p. 98, references omitted)

According to Clark *et al.* (2008), there is a “bulk of evidence” suggesting that “income does raise happiness” (*ibid.*). Consequently, the new challenge for the subfield would be to render the “significant income coefficient” consistent with the literature on the happiness paradox. Clark *et al.*’s (2008) paper is an attempt to “respond to that challenge” (*ibid.*), and their analysis is supposed to explain both Easterlin’s paradox and the Veenhoven-type argument of “diminishing happiness returns”. Clark *et al.* presented the following figure and explanation (Fig. 2.8).

Figure 2.8 (Clark *et al.*, 2008, p. 101)



“This stylized illustration sums up a pervasive opinion over the last few decades about the relationship between income and happiness at the individual country level. The marginal utility from extra consumption approaches zero as countries become richer. On the contrary, the marginal utility of extra status never approaches zero [...]” (Clark *et al.*, 2008, p. 101, equation omitted)

The most striking feature of Clark *et al.*'s (2008) argument, in comparison to those in the early development of the subfield, is that they first “imagine that happiness scores provide information about utility” (*ibid.*, p. 99), and then proceed by equating happiness with marginal utility in order to provide an explanation of relative incomes. This move is based on a model which is fully consistent with the framework of choice theory²².

Figure 2.9 (Clark *et al.*, 2008, pp. 99-100)

$$(1) \quad U_t = U(u_1(Y_t), u_2(Y_t/Y_t^*), u_3(T - l_t, Z_{1t}))$$

While the first and third parts of the utility function in (1) are standard, the second is less so, and shows the influence of status or habituation. The economic analysis of such relative income effects (or, more generally, interdependent preferences) can be dated back to at least Thorstein Veblen (1899), and then James S. Duesenberry (1949). More recent contributions include Robert A. Pollak (1976), Robert H. Frank (1985), and Jon Elster and John E. Roemer (1991).

The variable Y_t^* is often called “reference group” or “comparison” income, and the ratio Y_t/Y_t^* is called “relative” income.

Clark *et al.*'s (2008) strategy consists in checking whether subjective well-being data meet the “requirements that (decision) utility must fulfil [sic] in textbook treatments” (*ibid.*, p. 115). They check whether it “guides individual choice”, as well as if it is “itself the outcome of both choices and chance factors” (*ibid.*). They also check evidence supporting the “hypothesis that happiness is a good measure of utility” (*ibid.*, p. 116).

According to Clark *et al.* (2008), the analysis of happiness data “provides a valuable alternative, but complementary, approach to the revealed-preference framework that dominates the discipline of economics” (*ibid.*, p. 136). They use subjective well-being data as proxies for utility, and show “how the traditional utility function framework can be readily generalized to incorporate a range of observed behaviors” (*ibid.*):

²² Clark *et al.* (2008) use subjective well-being as a proxy for utility. $U(\cdot)$ is a function combining u_1 , u_2 , and u_3 . Y_t stands for incomes, $(T - l_t)$ for leisure, and Z_{1t} for “other socio-economic and demographic variables” (*ibid.*, p. 99).

“Our final conclusion is that taking relative income seriously is an important step toward greater behavioral realism in Economics, [...], utility functions including relative income terms produce a wide variety of testable predictions regarding both well-being (measured by survey or neurologically) and observable behaviors: it is not true that ‘anything goes.’” (*ibid.*, p. 138)

2.7. Joyful economists and economic policy: a conclusion

A final point concerning the recent development of the subfield is the contrast between Richard Layard’s line of argument in *Happiness: Lessons from a New Science* (2005) and his early writings presented in Section 1.5 (Layard, 1980). Layard’s recent work illustrates the current state of the subfield and reveals some important differences between the early and the recent programs (1). While the first challenged economic theory and was based on analytical arguments, the second draws on empirical findings and presents them as complements to revealed-preference accounts (2).

(1) The first important change between Layard’s 1980 article and his recent book is the style of the presentation. An analysis of utility functions is replaced by a rather literate and illustrated volume²³.

²³ Layard’s book is divided in two parts of seven chapters each: Part One: “The Problem”, is divided in: “1. What’s the problem?” (pp. 3-9), “2. What is happiness?” (pp. 11-27), “3. Are we getting happier?” (pp. 29-39), “4. If you’re so rich, why aren’t you happy” (pp. 41-53), “5. So what does make us happy?” (pp. 55-75), “6. What’s going wrong?” (pp. 77-93), and “7. Can we pursue a common good?” (pp. 95-108). Part Two: “What Can Be Done”, is divided in “8. The Greatest Happiness: Is that the goal?” (pp. 111-125), “9. Does economics have a clue?” (pp. 127-147), “10. How can we tame the rat race?” (pp. 149-165), “11. Can we afford to be secure?” (pp. 167-185), “12. Can mind control mood?” (pp. 187-203), “13. Do drugs help?” (pp. 205-221), and “14. Conclusions for today’s world” (pp. 223-239). See Frey (2008) for another comprehensive account of current developments in the subfield.

Figure 2.10 (Layard, 2005, p. 40)



Being an economist of happiness, Layard (2005) is concerned with the analysis of “long term average happiness” rather than “objective happiness” à la Kahneman (Layard, 2005, p. 17). According to Layard, average happiness is less influenced by daily activities than by key features in the respondents’ life situation (*ibid.*). The first part of Layard’s book presents the main empirical findings which are summarized as the “Big Seven” factors affecting happiness.

Figure 2.11 (Layard, 2005, p. 63)

The Big Seven factors affecting happiness (The first five are given in order of “importance”)
Family relationships
Financial situation
Work
Community and friends
Health
Personal freedom
Personal values

Layard’s account is based on the analysis of the correlates of average happiness rather than abstract theorizing as was the case of his earlier writings (Layard, 1980). Based on empirical studies of happiness between countries, Layard shows that there are six factors closely related to his “Big Seven” which explain 80% of the variations in happiness. These are: “divorce rate”, “unemployment rate”, “level of trust”, “membership in non-religious

organizations”, “quality of government”, and “fraction believing in God” (*ibid.*, p. 71). Concerning his Big Seven factors, he shows that while elements such as, “health”, “income” and the “quality of work” have contributed to improve the factors (*ibid.*, p. 78), some others have deteriorated them. This is the case of “broken families”, “increased crime”, and “decreased trust” (*ibid.*, pp. 78-82)²⁴. According to Layard’s account, these are the main problems to be addressed by public policies.

“Does economics have a clue?” This is Layard’s (2005) question. His answer:

“yes and no. The framework is terrific. We start from the individuals and their wants. Then we have the market where they interact – and where their wants get satisfied, either more or less. What is wrong is the theory of human nature, which is largely based on an outdated version of behaviourism.” (*ibid.*, p. 128)

Without discussing Layard’s views of behaviorism (that is the object of Chapter 3), the important thing to point out is that Layard thinks psychological findings should be used as complements to economic analysis. He shows that even though psychology “has now returned to the study of feelings”, it lacks the “comprehensive framework for policy analysis that economics provides” (*ibid.*). Layard thinks that new elements should be added to economic analysis. He mentions “inequality”, “external effects”, “values”, “loss-aversions”, and “inconsistent behaviors” (*ibid.*, p. 135), approaching the “new” behavioral economists’ position (Chapter 4):

“The strength of economics is that it starts from the idea of people as self-determining agents. No reasonable analysis of what society we want should start anywhere else. Economics also provides a good general framework for policy analysis by attempting to calculate and compare all the costs and benefits. However, the economic model of human nature is far too limited – it has to be combined with knowledge from other social sciences.” (*ibid.*, p. 145)

²⁴ According to Layard’s (2005) account, the explanation of this deterioration comes from “three big changes”: “the change in gender roles”, “the spread of television”, and “the growth of individualism” (*ibid.*, p. 82). Layard’s recent views about happiness and public policies are shared by other contemporary scholars of happiness. Helliwell (2006), for instance, analyzes the correlations between trust and satisfaction data using answers to the following kind of question: “Generally speaking, would you say that most people can be trusted?” This kind of question is asked in the World Values Survey. According to Layard (2005), the “proportion of people who say ‘Yes, most people can be trusted’ varies from 5% in Brazil to as high as 64% in Norway” (Layard, 2005, p. 68).

(2) Interestingly, Layard's main message has also radically changed. Rather than attacking redistribution policies based on theoretical arguments (such as introducing "loss aversions" in preference functions: Section 1.5), he fully justifies them:

"if money is transmitted from a richer person to a poorer person, the poor person gains more happiness than the rich person loses." (Layard, 2005, p. 52)

It is important to point out that Layard's new argument is based on empirical findings which show that redistributions have positive effects on happiness. These findings also indicate that countries with more equal income distributions present higher levels of happiness, allowing him to argue that "inequality is bad because extra income brings less benefit to the rich than the poor" (*ibid.*).

But perhaps the most striking difference between Layard's early and recent writings is that while he used to (theoretically) advocate for lowering income expectations (Layard, 1980), he now criticizes this same argument based on empirical evidence. He strongly rejects the thesis (which he himself defended in 1980) according to which incomes running ahead of expectations have positive effects on happiness and even criticizes its proponents:

"Happiness, they said, depends on what you have (in different domains) relative to your expectations. If this were true, the simplest way to be happy would surely be to lower your expectations and your goals." (*ibid.*, p. 73)

Finally, in regards to the effects of social comparisons (the pursuit of social status), Layard no longer advocates for radical institutional changes or extreme policies such as "changing human nature" or altering utility functions (Layard, 1980)²⁵. His current advice is summarized in the following figure.

²⁵ While in 1980 Layard considered changing human nature by intervening in children's education, now he rather directs his claims against the welfare effects of watching television: "Television has revolutionized our lives in the same way as printing once did [...]. The more television people watch, the more they overestimate the affluence of other people. And the lower they rate their own relative income. The result is that they are less happy [...]. Since television has a negative impact on your perceived position, it is bad for your happiness [...]. We should not go back to a world without television, but we can surely use our television better than we do now" (Layard, 2005, pp. 85-90).

Figure 2.12 (Layard, 2005, pp. 233-234)

A BETTER WORLD: TAKING HAPPINESS SERIOUSLY	
<ul style="list-style-type: none">• We should monitor the development of happiness in our countries as closely as we monitor the development of income.• We should rethink our attitude on many standard issues. On taxes, we should recognise the role they play in preserving the work-life balance. On performance-related pay, we should worry about its tendency to encourage the rat race. On mobility, we should consider its tendency to increase crime and weaken families and communities.• We should spend more on helping the poor, especially in the Third World. The United States at present spends 0.13% of its income on overseas aid, Britain 0.31%.¹⁸ We now understand better how to spend this money.¹⁹ If you want to relieve hunger and misery, here is a ready-made route. We should be proud to make this a goal of our affluent societies.• We should spend more on tackling the problem of mental illness. This is the greatest source of misery in the West, and the fortunate should ensure a better deal for those who suffer. Psychiatry should be a top branch of medicine, not one of the least prestigious.• To improve family life, we should introduce more family-friendly practices at work—more flexible hours, more parental leave and easier access to child care.	<ul style="list-style-type: none">• We should subsidise activities that promote community life.• We should eliminate high unemployment. Here tough-and-tender works best. After a time everyone should be given a chance to work, but should have to take advantage of the opportunity in order to continue receiving support.• To fight the constant escalation of wants, we should prohibit commercial advertising to children, as in Sweden. We should also cut tax allowances for pictorial advertising to adults by business.• Finally, and perhaps most importantly, we need better education, including, for want of a better word, moral education. We should teach the principles of morality not as interesting points for discussion but as established truths to hold on to, essential for a meaningful life. We should teach the systematic practice of empathy, and the desire to serve others. This needs a proper curriculum from the beginning of school life to the end, including detailed study of role models. The curriculum should also cover control of one's own emotions, parenting, mental illness and of course citizenship. But the basic aim should be the sense of an overall purpose wider than oneself.

Layard's "new" advice is based on empirical findings and is less focused on theoretical issues than his earlier recommendations (Layard, 1980). He suggests that nations should monitor happiness measures, and that public policies should be focused on improving factors affecting these measures (i.e. mental illnesses, or the quality of family life). It is worth noting that Layard's "new" approach combines the analysis of happiness data with "the insights of revealed preferences" (Layard, 2006, p. C33). This is quite different from the earlier attempts by economists of happiness, which focused on the analysis of the principles underlying consumer behavior (i.e. social comparisons or income expectations) and opposed choice theory. The following is Layard's conclusion in "Happiness and Public Policy" (2006):

"Thirty years ago population surveys revolutionized labour economics. A similar revolution will soon revolutionize public economics, when psychological data on happiness are at last combined with the insights of revealed preference. This will lead to better theory and better policies." (Layard, 2006, p. C33)

Conclusion to Part I: The Economics of Happiness

Part I was intended to show that the short history of the economics of happiness is not uniform. Chapter 1 maintained that during the twenty-five years following Easterlin's venture to the economic analysis of happiness data, the happiness paradox was used as a support for theoretical arguments related to the economics of affluence. These arguments were supported by only a few empirical studies of happiness (mainly Easterlin's) and were directed against economic theories such as the Keynesian consumption function (Duesenberry, 1949), consumer choice theory (Scitovsky, 1976), and welfare economics (Frank, 1989; Ng, 1997).

Scitovsky's *The Joyless Economy* (1976) is representative of the literature of this period. It consisted in developing an alternative to consumer choice theory by analyzing the factors motivating consumer behavior in affluent contexts. Like Scitovsky's contributions, the contributions of Fred Hirsch (1977) and Robert Frank (1989) opposed consumer behavior in affluent contexts to the predictions of choice theory. They considered the effects of social comparisons as externalities, and proposed the correction of such external effects by the application of taxes. The main findings of the first period (1974-1999) were resumed by presenting Andrew Oswald's (1997) contribution to the *Controversy: Economics and Happiness* published in *The Economic Journal*. These were, mainly, Easterlin's paradox as well as negative correlations between unemployment and happiness.

Since the turn of the century, the subfield has experienced important changes, and has been developed with consideration to the establishment of hedonic psychology rather than opposed to economic theory (Chapter 2). The economic analysis of life satisfaction data has been intensified and presented as an alternative to "objective happiness" measures produced and analyzed by hedonic psychologists. The last sections of Chapter 2 presented the literature on the happiness paradox and the analysis of the policy implications of happiness studies (leaving aside the analysis of the correlates of happiness as well as the Life Satisfaction Approach for valuing environmental goods, to be discussed in Chapter 6). They showed that the literature on the happiness paradox has experienced profound changes. It now rejects Easterlin's early version of the paradox and reframes the analysis in order to fit life satisfaction data within the framework of economic theory (i.e. in utility functions). There

have also been fairly radical changes in the policy recommendations of economists of happiness (such as Layard's, 1980 vs. 2005). Instead of taking "social comparisons" and "aspirations" as arguments altering consumer preferences, economists of happiness now focus on the empirical findings themselves, which are presented as complements to cost-benefit, or revealed-preference accounts.

THE FIVE-MINUTE-PAUSE

The image displays a page of a musical score for Gustav Mahler's Second Symphony, specifically the first movement. The score is arranged in a standard orchestral format with multiple staves for different instruments. At the top, it is marked '27 Tempo I.' and 'riten.'. The instruments listed on the left include Piccolo, Flutes (1. 2. Fl.), Oboes (1. 2. Ob.), English Horn (Engl. Horn), Clarinets (1. 2. Clar. in B.), Bass Clarinet (Bassclar. in B.), Bassoon (1. 2. Fag.), Contrabassoon (Contrafag.), Horns (1. 2. Horn in F, 3. 4. Horn in F, 5. 6. Horn in F), Trumpets (1. 8. Trmp. in F, 2.), Tam-tam (Tamtam (tief)), Cymbals (Becken), Gong (Gr. Tr.), Snare Drums (1. Pauke, 2. Pauke), Harps (1. Harfe, 2. Harfe), Violins (1. Viol., 2. Viol.), Viola, Cello (Cello), and Bass. The score includes various musical notations such as dynamics (ppp, pp, p, f, ff, mf, ppp), articulation (accents), and performance instructions like 'riten.' (ritardando) and 'Tempo I.'. At the bottom of the page, there is a large section of rests for all instruments, indicating a five-minute pause. The text 'Hier folgt eine Pause von mindestens 5 Minuten.' is written below the rests. The score ends with '27 Tempo I.' and 'riten.'.

As noted above, there is an important change of viewpoint between Part I, and Parts II and III of this work. In order to deal with a somehow comparable problem, Gustav Mahler introduced a five-minute-pause (appearing at the end of this music score) between the first and the second movements of his Second Symphony (see Kaplan, 1986; Banks, 1987). Instead of rearranging the whole structure of the dissertation, I adopted Mahler's strategy, which was mentioned in the final examination of this work (acknowledged in the Preface). The reader will now experience this particular adaptation.

II. BYPASSING BEHAVIOR CONTROL

Introduction

“The only way in which the economist can keep his studies from duplicating the psychologist’s work is by taking his psychology from those who have specialized in that field [...]. Any conception of human nature that he may adopt is a matter of psychology, and any conception of human behavior that he may adopt involves psychological assumptions, whether these be explicit or no. If the economist borrows his conception of man from the psychologist, his constructive work may have some chance of remaining purely economic in character. But if he does not he will not thereby avoid psychology. Rather he will force himself to make his own, and it will be bad psychology.” (J. M. Clark, 1918, p. 4)

This part of the dissertation is closely related to John M. Clark’s (1918) claims, quoted above, for it is based on the idea that there are always psychological assumptions in economic accounts of behavior. The following two chapters explore the history of the psychological underpinnings of these accounts. The main thesis of this part of the dissertation is that the development of early economics of happiness (1974-1999) into a recognized subfield (1999-2009) can be explained by a shift in the psychological assumptions supporting each program. Chapter 3 focuses on the historical development of the psychology underlying the economics of affluence and the early economics of happiness. Chapter 4 discusses the history of the psychological approach underlying the recent economics of happiness (i.e. choice theory). Both chapters also deal with the long-standing relationship between economics and psychology.

Chapter 3 shows that the view of consumer behavior endorsed by economists of affluence was influenced by psychological theories of behavior control. It traces the origins of these theories back to the roots of behaviorism and maintains that there has been a long history of relationships between behavior control approaches in psychology and economic programs. This was the case of the early American institutionalism of T. Veblen, W. C. Mitchell and J. M. Clark, which was explicitly based on W. McDougall's functional psychology, and also that of "behaviorist institutionalism" as promoted by W. C. Mitchell, L. K. Frank and M. A. Copeland during the interwar period. Chapter 3 further maintains that behaviorism also had a counterpart from the postwar onwards in both the economics of affluence and the early economics of happiness.

As noted in the introductory chapter, the reconstruction presented in Chapter 3 challenges the standard historical accounts of the relationship between economics and psychology. It maintains that the economic theories of consumer behavior, in particular the ordinalist ones, were far from being behaviorist. While economists such as Hicks and Allen (1934) or Samuelson (1938, 1948) were attempting to rid choice theory of introspection and mentalism by means of theoretical sophistications, behaviorists were attempting to control and predict animal behavior under experimental settings and developing theories of behavior control. This was B. F. Skinner's approach, which applied experimental findings to the analysis of social subjects in widespread volumes such as *Beyond Freedom and Dignity* (1971). Chapter 3 maintains that the economics of affluence (and hence the early economics of happiness) was more closely related to Skinnerian behaviorism than to the so-called "behaviorist mainstream economics" of Samuelson and his followers.

Chapter 4 analyzes the history of the psychological underpinnings of choice theory for roughly the same period as Chapter 3. Mechanic, consistent, and now behavioral, the history of consumer choice theory is claimed to be systematically, though ambiguously, related to the history of psychology. The main thesis of Chapter 4 is that ambiguity has been the rule rather than the exception as far as the historical relationship between the two disciplines is concerned. Just as the approach of mainstream economists to behaviorism was ambiguous, so was the use of psychophysics by Jevons and Edgeworth. Furthermore, the mainstream reception of the information processing studies of Herbert Simon and John von Neumann was equally ambivalent. In what

regards the recent history of the economics of happiness, the chapter shows that its psychological assumptions are consistent with the approach promoted by “new” behavioral economists and equally ambiguous in their relationship to experimental psychology.

Chapter 3. Beyond Freedom and Dignity: From Behavior Control to the Economics of Affluence

3.1. Introduction

Chapter 3 shows that the view of consumer behavior endorsed by economists of affluence (and early economists of happiness) was influenced by psychological assumptions of behavior control. In order to deal with the origins of these assumptions, Section 3.2 traces the narrative to the emergence of naturalist psychology in Britain during the late 19th century. The partisans of this naturalist psychology thought of behavior as the outcome of the interaction of an organism with its environment. Every behavior observed was supposed to be naturally selected (controlled) by the environments in which the organisms evolved. This naturalist approach to psychology had a strong influence on early developments of psychology in America, and behavior control became a paradigm for the analysis of both animal and human behavior. From William James' psychological theories to John B. Watson's behaviorism, passing through the rise and fall of functional psychology, early American psychologists generated a discipline that was also strongly marked by a commitment to humanist and progressive values.

Section 3.3 shows that the naturalist movement was not exclusively a trait of American psychology and that it had its counterpart in economics. Institutionalists such as Thorstein Veblen (1899), Wesley C. Mitchell (1910) or John M. Clark (1918) embraced the instinct psychology of William McDougall (it was in this context that J. M. Clark wrote the lines opening this part of the dissertation). During the interwar period, the behaviorism of John B. Watson and his followers was used as a framework for the analysis of economic phenomena by Mitchell (1925), Lawrence K. Frank (1925), and Morris A. Copeland (1926). However, as Watson's behaviorism, this interwar institutionalism decayed with the fall of the new progressive era. This happened during the late 1920s and marked the end of the first attempts to integrate behavior control frameworks in the analysis of economic behavior.

From the late 1930s to the 1970s, new behaviorisms emerged as dominant programs in American experimental psychology. Section 3.4 deals with behavior control approaches promoted by “neobehaviorists” in general and B. F. Skinner in particular. These behaviorist programs consisted in controlling animal behavior by experimentally shaping the animals’ environments (i.e. mazes or boxes) and served as a basis from which behavior control approaches were extended to the analysis of social subjects. These extensions were particularly forceful from the late 1950s to the late 1970s. During this period, B. F. Skinner produced a social discourse which was as widespread as it was rejected. This rejection was however overtly societal and not methodological, and is presented by historians of psychology as an event illustrating how experimental psychology has been socially shaped.

Section 3.5 challenges the standard historical account of the relationship between economics and psychology presented above (Introduction) by showing that the economic theories of consumer behavior, in particular the ordinalist, were far from being behaviorist. While economists such as Hicks and Allen (1934) or Samuelson (1938, 1948) were (unsuccessfully) attempting to rid choice theory of mentalism, behaviorists were attempting to control and predict animal behavior under experimental settings. Unlike behaviorists, Samuelson and his followers conceived of consumer behavior as purposive and deliberate, and based on the mentalist notion of preference. These ordinalist economists, in addition to rarely observing behavior under experimental settings, were strongly committed to consumer sovereignty as a principle and rejected the idea of consumer behavior as being controlled by the environment.

Section 3.6 concludes the chapter by showing that the economic counterpart to post-war behaviorism was the economics of affluence. It draws upon the economists of affluence’s conception of consumer behavior which was based on the idea that consumption was shaped by external factors present in affluent contexts (Chapter 1). It also shows that, unlike behaviorists, economists of affluence faced claims by mainstream economists, which were presented as scientific arguments. The clearest example of this is M. Friedman’s (1977) attack to J. K. Galbraith and his followers. Chapter 3 concludes by showing that the view of consumer behavior of both economists of affluence and early economists of happiness was based on psychological

assumptions that were incompatible with the psychological underpinnings of consumer choice theory.

3.2. A history of American experimental psychology: naturalist, functional, behaviorist

(1) During the late nineteenth century, the development of the American human sciences (psychology in particular) was strongly supported by scholars trained in German universities (Boring and Boring, 1948; Boring, 1950; Ben-David and Collins, 1966; Sokal, 1984; Mandler, 2007). Wilhelm Wundt's laboratory in Leipzig being the main training place for American psychologists, his experimental approach had a strong influence in the early development of the discipline in America:

“practically all influential psychologists at the turn of the century were students of Wundt's or were students of his students. Experimental psychology was defined by the experiences of the Leipzig Laboratory, and American laboratories were generally opened by his students with imported German instruments.” (Mandler, 2007, p. 59)¹

Besides the German influence, the American approach was strongly shaped by the British “naturalist evolutionary” approach to psychology which was initiated in Charles Darwin's *Expression of the Emotions in Man and Animals* (1872) and Herbert Spencer's *Principles of Psychology* (2nd edition, 1870 and 1872). This naturalist approach suggested the continuity between the human and animal species and paved the way for the development of comparative (i.e. animal) psychology in America². According to E. Boring's *History of Experimental Psychology* (1950), the naturalist evolutionary idea of selection by the environment was applied to the analysis of the human mind, and led to the emergence of an “alternative picture of psychology” (Boring, 1950, p.

¹ Sections 4.1 to 4.3 are based on Boring's *History of Experimental Psychology* (1950) as well as Madden (1965), Daston (1978), Ash (1980), Herman (1995), Mills (1992, 1998), Capshew (1993, 1999), Rutherford (2003, 2006), Mandler (2007), and Igo (2008). See Ash (1980), for a critical account of the history of the institutional development of German experimental psychology as a systematic success.

² According to Madden (1965), the origins of comparative psychology “began definitely as an attempt to answer the Darwinian question about the continuity of mind from animals to man” (Madden, 1965, p. 199).

512). This alternative contained the possibilities of the development of “functional psychology, with its cousin, the mental tests, and its child, behaviorism” (*ibid.*)³. William James’ *Principles of Psychology* (1890) were strongly influenced by the naturalist approach in general and Spencer’s psychology in particular. They were a clear reflection of the fact that American psychology “inherited its physical body from German experimentalism but [...] got its mind from Darwin” (*ibid.*, p. 506):

“few recent formulas have done more real service of a rough sort in psychology than the Spencerian one that the essence of mental life and of bodily life are one, namely, ‘the adjustment of inner to outer relations.’ Such a formula is vagueness incarnate; but because it takes into account the fact that *minds inhabit environments* which act on them and on which they in turn react; because, in short, it takes mind in the midst of all its concrete relations, it is immensely more fertile than *the old-fashioned ‘rational psychology,’* which treated the soul as a detached existent, sufficient unto itself, and assumed to consider only its nature and properties.” (James, 1890, Vol. 1, p. 6, emphasis added)

James’ program was a sort of unexpected synthesis of German experimentalism and British naturalist psychology: on the one hand it was experimentalist, and so it was a step ahead of British experimental psychology⁴; on the other hand, by rejecting Wundt’s atomist conception of psychology (which was focused on the analysis of elementary sensations), it abandoned the German experimentalist tradition for a social (or instinct)

³ Francis Galton [1822-1911], Darwin’s half-cousin, introduced the analysis of mental inheritance leading to the development of functional psychology. The functional approach was absent from Wundt’s psychology which was focused on description. In Boring’s words: “Wundt favored description and generalization, whereas Galton was constantly aware of the practical uses of psychology and was diligent in promoting their use. Wundt wanted to improve psychology; Galton the human race” (Boring, 1950, pp. 487-488).

⁴ According to Boring (1950), and Daston (1978), experimental psychology was contested in Britain where it faced both institutional and “philosophical” difficulties. Boring’s history shows that the development of psychological laboratories was resisted in Britain where psychologists as Galton and McDougall, never held important posts: “from 1890 to 1920, when Germany and America were teeming with laboratories and professional experimental psychologists, great Britain was advancing slowly in the new science only by way of the work of a few competent men.” (Boring, 1950, p. 460) Philosophical psychology “remained in the saddle for a long time” in Britain, where the experimentalists/naturalists “had to work against difficulties” (*ibid.*). For Daston (1978), the British resistance to psychophysiology “cannot be solely attributed to the absence of institutionalized support for psychology” (Daston, 1978, p. 207). The discipline would have also suffered the tension of holding both the (philosophical) commitment to moral values of volition and free-will and the physiological and experimental (scientific) requirements of the new program.

approach⁵. According to Madden (1965), the “young Americans, partly knowingly and partly unwittingly”, imported “the new experimental psychology from Germany” and “changed it beyond recognition” (Madden, 1965, p. 198). They produced comparative psychologies which were “fundamentally influenced by the adaptive principle of natural selection” (*ibid.*, p. 199). These psychologies were focused on the analysis of the evolutionary problem leading the way to the “*objective* psychologies of behaviorism and operationism” (*ibid.*).

(2) There is quite a consensus among historians of psychology on the idea that, despite the German influence, the main forces shaping American psychology arose “not within psychology itself but within American society from about the 1880s onward” (Mills, 1998, p. 2). In Boring’s terms, the functional character of American psychology was, to a great extent, an effect of the new world’s culture:

“If influenced by your culture, you conclude that you should devote yourself to the description of nature, content to say *what* happens and *how* it happens, without asking the question why, then you are concerned with structure, are working in the descriptive tradition [...]. But if you ask *why*, if you try to understand causes, then you are interesting yourself in capabilities, in capacities, and are being a functionalist. It is as natural to be a functionalist as it is to want to predict, to be more interested in the future than in the past, to prefer to ride facing forward on the train. The future concerns you because you think you might change it if you have the ability. The past has gone by, lies there open to description but unalterable.” (Boring, 1950, p. 551)

At the beginning of the 20th century, a department of functional psychology (the study of the adaptive role of mind in animal life) was established at

⁵ Though James [1842-1910] himself was not an experimentalist, he initiated informal laboratories in Harvard in both physiology (Lawrence Scientific School) and comparative psychology (Museum of Comparative Zoology) during the 1870s (Boring, 1950). James’ *Principles* had more of instinct psychology (or “social psychology” as the approach was labeled by the end of the 19th century) than of German experimentalism. According to Cravens and Burnham (1971), this “social psychology” was focused on the analysis of “the neurology of the individual [...]. The psychologists assumed that man [was] an animal whose behavior [was] governed by inherited traits, and they therefore searched for and tried to identify the human traits. Commonly they identified these traits as instincts” (Cravens and Burnham, 1971, p. 641).

Chicago⁶. Animated by John Dewey [1859-1952], George Herbert Mead [1863-1931], Addison Webster Moore [1866-1930] and James Rowland Angell [1869-1949], the Chicago school of functional psychology furthered the development of both comparative and educational psychology and led to the early behaviorism promoted by John B. Watson (J. R. Angell supervised Watson's dissertation). This behaviorism inherited a great part of the ideas of the Chicago school of thought which, according to W. James (1904), was a clear expression of the American values:

“Chicago has a School of Thought! [...]. It coincides remarkably with the simultaneous movement in favor of ‘pragmatism’ or ‘humanism’ [...]. It probably has a great future, and is certainly something of which Americans may be proud.” (James, 1904, pp. 1, 5)

(3) From its early days to the late 1920s, behaviorism was a dominant school in American psychology (Mills, 1998). The early development of the discipline was animated by John B. Watson who also started behaviorism as a movement⁷. According to Watson's (1913) manifesto, the theoretical goal of the program was “the prediction and control of behavior” (Watson, 1913, p. 158), a goal to be approached by means of experimentation with animal behavior. Watson thought of behaviorism as “a purely objective experimental branch of natural science” which recognized “no dividing line between man and brute” (*ibid.*). Rather than “observing” consciousness through introspection, Watson thought psychologists should narrow the object of their discipline in order to capture observable outcomes. Instead of writing psychology out of terms such as “consciousness, mental states, mind, content, introspectively verifiable” and others of their kind (*ibid.*, p. 166), Watson thought psychology should be written in terms of men and animal organisms adjusting “to their environment by means of hereditary and habit equipments” (*ibid.*, p. 167).

⁶ Another department of functional psychology was settled at Columbia by James McKeen Cattell [1860-1944], Edward Lee Thorndike [1874-1949] and Robert Sessions Woodworth [1869-1962] (Boring, 1950).

⁷ Although John Broadus Watson [1878-1958] (Chicago, Johns Hopkins) took the entrepreneur role, the roots of behaviorism can be traced back at least to the 1860s in Russia (I. P. Pavlov (nobel prize, 1904) and V. M. Bekhterev) and to the development of animal psychology under the influence of Darwin's findings in the 1890s (C. Lloyd Morgan, Edward L. Thorndike, Jacques Loeb) (Boring, 1964; Madden, 1965).

Historians of psychology show, however, that by the time Watson published the behaviorist manifesto (1913) he was not yet a behaviorist but just a comparative psychologist. According to Cravens and Burnham's account entitled "Psychology and Evolutionary Naturalism" (1971), it was later in 1917 that Watson "turned to the problem of human instincts in human infants" and started to show that the instinct psychologies (of W. James or W. McDougall) had overestimated the number of instincts in man (Cravens and Burnham, 1971, p. 646). It was only during the 1920s that Watson suggested that rather than being innate, most of the human behavior patterns were the result of conditioning and learning. This conclusion was supported by the analysis of animal behavior in experimental environments where the "entire life history of [the] subjects [was] under careful control" (*ibid.*, p. 647).

It is worth noting that for historians of psychology, the dividing line between comparative psychology and behaviorism lies in the social application of the experimental findings⁸. According to Mills' (1998) account, Watson's "socially oriented and crudely speculative" behaviorism was closely related to the progressive movement and declined with it from the mid-1920s onwards (Mills, 1998, p. 55). Based on this historical fact, Mills shows that the history of behaviorism is not continuous. He explains that Watson's "academic reputation declined" from 1926 onwards (*ibid.*, p. 75), and that the early behaviorism developed by Watson was a "far cry from [the] highly sophisticated and technical work" of the more recent behaviorists (*ibid.*). Watson's program had only an indirect influence on the "new" behaviorisms that emerged during the late 1930s.

3.3. The rise and fall of "behaviorist" institutionalism

It is interesting to point out that there was an economic counterpart to the development of both functional psychology and Watson's behaviorism. These were the early institutionalist approaches of Thorstein Veblen [1857-1929] and Wesley C. Mitchell [1874-1948] (1), and then the interwar program of American economists such as Mitchell himself (2), Lawrence K. Frank [1890-

⁸ For Mills (1998): "Watson's published animal work shows no trace of a behaviorist position" (Mills, 1998, p. 57), and at least until 1913 Watson equated behaviorism with comparative psychology (*ibid.*, p. 70).

1968] (3), and Morris A. Copeland [1895-1989] (4). These institutionalists shared the progressive views of their time and place and thought of consumer behavior in terms of behavior control.

(1) The early institutionalism developed during the first two decades of 20th century by, namely, Veblen and Mitchell was closely related to the instinct-based theories developed by functional psychologists and committed to progressive values. Veblen's "Instinct of Workmanship" (1898) and the *Theory of the Leisure Class* (1899) were based on the naturalist idea that man was an agent that acted "in response to stimuli afforded by the environment in which he live[d]" and that like other animal species, he was "a creature of habit and propensity" (Veblen, 1898, p. 188). His analysis of the institution of the leisure class was indeed based on the idea that both the social structure and the individual character of the members of society (Veblen, 1899, p. 145) were shaped by the instinct-based practices of the leisure classes by which they rendered their leisure and consumption conspicuous. He thought that these practices "shaped" the "habits of thought" and exercised a "selective surveillance over the development of men's aptitudes and inclinations", this "partly by coercive, educational adaptation of the habits of all individuals, partly by a selective elimination of the unfit individuals and lines of descent" (*ibid.*)⁹.

Wesley C. Mitchell's view of consumer behavior was close to Veblen's, and involved in the progressive attempt to apply the social sciences to the art of housekeeping. In "The Backward Art of Spending Money" (1912) he showed that while the art of making money was strongly supported by the progress of natural sciences, the art of spending was less developed and lacked scientific support (Mitchell, 1912, p. 3). While production was organized on the basis of the "business enterprise" and the "money economy", Mitchell showed that the

⁹ According to Veblen's book, the origins of the leisure class could be traced back to the middle and high stages "of the barbarian [feudal] culture" (Veblen, 1899, p. 21). It was formed by social classes which were "exempt from industrial employments" (*ibid.*). Besides the possession of wealth and power, Veblen showed that these classes gained and held "the esteem of men" (*ibid.*, p. 42) by means of their leisure and consumption: "From the foregoing survey of the growth of conspicuous leisure and conspicuous consumption, it appears that the utility of both alike for the purposes of reputability lies in the element of waste that is common to both. In the one case it is a waste of time and effort, in the other it is a waste of goods. Both are methods of demonstrating the possession of wealth, and the two are conventionally accepted as equivalents" (Veblen, 1899, p. 71).

family had remained “the unit for consuming goods” as it was in its “colonial days” (*ibid.*, p. 5). Unlike the “arts of production”, he wrote, the “arts of consumption” were still waiting “upon progress in science” (*ibid.*)¹⁰:

“unfortunately for the art of spending money, the sciences of fundamental importance are not physics and chemistry, but physiology and functional psychology [which] are in a relatively rudimentary condition [...]. No doubt the sciences that will one day afford a secure basis of knowledge for bringing up a family are progressing; but it seems probable that they will long lag behind the sciences that serve industry.” (Mitchell, 1912, pp. 11-12)¹¹

For Mitchell, the big difference between making and spending money came from the fact that unlike business (which was focused on the straightforward end of making money); housekeeping involved “some end beyond the spending”. He claimed that the ends of housewives (the housekeepers) were not only “the happiness of her husband and herself” or “the fair development of their children”, but also (and mainly) “the desire for distinction” which he considered an “inveterate habit” resulting from “biased comparisons” between households (*ibid.*, pp. 13-14). This habit was supposed to control spending behavior:

“the mass of housewives come under the sway of [a] paradoxical impulse. Not for themselves alone, but also for the sake of their husbands and their children, must they make it appear that the family stands well in a world where worth is commonly interpreted as dollar’s worth [...]. Worldly wisdom, therefore, counsels the housewife to make as brave a show as may be with the income at her disposal.” (*ibid.*, p. 16)

Mitchell hoped that in the future, society would be able to “rely upon progress in physiology and psychology to make wider and more secure the scientific foundations of housekeeping” (*ibid.*, p. 18). This would improve the art of spending money, correct conspicuous consumptions and allow housekeeping to catch up the arts of industry.

¹⁰ “we have jealously insisted upon maintaining the privacy of family life, its freedom from outside control, so far as our circumstances have permitted. Reluctantly we have let the factory whistle, the timetable, the office hours impose their rigid routine upon our money-making days; but our homes we have tried to guard from intrusion by the world of machinery and industry.” (Mitchell, 1912, pp. 5-6)

¹¹ In “The Rationality of Economic Activity” (1910), Mitchell presented McDougall’s “social (or instinct) psychology” and its relevance for the understanding of economic behavior. Part of Mitchell’s (1910) paper is presented in Chapter 4 (Section 4.4).

(2) After World War I, institutionalists like Mitchell, Frank and Copeland, following the developments in American psychology, developed a behaviorist form of institutionalism. As noted by historians specialized in the subject, this interwar program, more than the earlier institutionalism, was “primarily concerned with reforming society, expanding economic opportunities, and ameliorating the general welfare conditions” (Asso and Fiorito, 2004b, p. 463; M. Rutherford, 2000).

As noted by Mills (1998), the “continuity with Progressivism” was evident in the interwar program defended by W. C. Mitchell (Mills, 1998, p. 30). Influenced by the development of statistics during WWI, Mitchell’s interwar approach consisted in recasting economic theory “into new forms amenable to statistical attack” (Mitchell, 1925, p. 3). This approach differed from the qualitative analysis (choice theories) of Jevons, Marshall and their followers and consisted in exploring “the mass phenomena” that these “qualitative analysts approached indirectly through their hypothetical individuals” (*ibid.*, p. 5). In Mitchell’s own words, the quantitative approach was an attempt to replicate the work of American psychologists who were “moving rapidly toward an objective conception and a quantitative treatment” of human behavior (Mitchell, 1925, p. 6):

“Their emphasis upon stimulus and response sequences, upon conditioned reflexes; their eager efforts to develop performance tests, their attempts to build up a technique of experiment, favor the spread of the conception that all of the social sciences have a common aim – the understanding of human behavior [sic]; a common method – the quantitative analysis of behavior records; and a common aspiration – to devise ways of experimenting upon behavior.” (*ibid.*)

Mitchell thought economists should try behaviorist experiments in order to give scientific status to their discipline. Even if these experiments lacked “the rigor of the experimenting done in physical laboratories”, because of the narrow “limits within which human beings [could] be manipulated” (*ibid.*, p. 8), he thought they could be useful for controlling economic phenomena, namely the business cycle. He thought statistic measurement was “indispensable to convert society’s blind fumbling for happiness into an intelligent process of experimentation” (*ibid.*). This process, he wrote, should be supported by “experiments upon group behavior” which were clearly behaviorist-inspired as they largely consisted in controlling environments:

“we are already trying such experiments. We have experimental schools, in which the physical and social environments of the children are made to vary, with the aim of studying the relations between the stimuli offered by the schools and the learning response. So, too, we experiment with different systems of remunerating labor, different forms of publicity, different organizations for distributing products, different price policies, different methods of supervising public utilities, and the like.” (Mitchell, 1925, pp. 8-9)

(3) Lawrence K. Frank went even further in regards to behavior control¹². Frank’s “Theory of Business Cycles” (1923) related economic disequilibria to the disparity between the (fast) development of production techniques and the rather fixed character of “the habits and customs (institutions) of men which ma[de] up the money economy” (Frank, 1923, p. 639). Because of these “fixed habits of behavior”, Frank thought that despite technical change, men would recurrently “behave in the same way when exposed to the same stimuli” (*ibid.*). Consequently, he proposed behavior technologies as means to update these fixed habits so as to shape social behaviors in order to gain control over the business cycle:

“There are already under way certain tentative endeavors in the experiment upon habit formation, for example, new varieties of schools. Advertising agencies are conducting (more or less unwittingly) numerous experiments on group behavior, habit breaking, and the like, and in industrial organizations there have been many highly instructive trials made on integration of group habits. [...] the anachronism in habits is not the least of our social difficulties.” (Frank, 1924a, p. 24)

¹² Lawrence K. Frank’s [1890-1968] career and writings (Bryson, 1998): “represent a compelling case study in the linking of social science knowledge with techniques for improvement and control. An officer with the Rockefeller philanthropies and the Macy foundation during the period 1923-42, Frank was uniquely able to bridge the worlds of inquiry and technology. Thus, while he was an associate and intellectual ally of such important figures in American social science as Wesley C. Mitchell, Robert S. Lynd, Margaret Mead, Edward Sapir, Erik H. Erikson, and John Dollard, Frank never lost his fundamental concern for applying the knowledge of the social. More specifically, as the architect and administrator of several major foundation-sponsored programs in child development and parent education and in culture and personality, Frank formulated and advanced a sociopolitical project for the development and dissemination of new, ‘enlightened’ methods for socializing children and adolescents. Through such methods, Frank believed, the individual would be securely integrated within the social, and a cooperative and pacified society would result” (Bryson, 1998, p. 403). See also Asso and Fiorito (2004a) for an account on Frank’s work.

As the institutionalisms mentioned above, Frank's theory was strongly supported by stimulus-response and habit formation frameworks. As soon as it is recognized, he wrote, that "whatever men do (i.e., both the things they respond to and the pattern of response) is a product of their experience, a habit" (Frank, 1924b, p. 36) it would become clear that "the only problem left for economics is to study the variations in the responses evoked by the same stimuli, from time to time and from place to place" (*ibid.*). Frank was focused on the study of historically embedded phenomena, and thought it was impossible to have a "social science of any real moment" before working out "a technique of experimenting with human behavior" (*ibid.*, p. 37). He was optimistic, however, about the future development of the discipline. Social science, he wrote:

"will probably produce a method of developing and removing habits in a group, which suggests the possibility that we may some day work out a method of promoting a truly *social* life by discovering a technique for inculcating the habits needed in social living [...] these habits must change from time to time (as other techniques develop) [...] it is not possible to develop a *social* life until we discover what new habits of social behavior are needed to replace the present and how to do the replacing." (*ibid.*, pp. 37-38)

Because social problems came from "difficulties associated with an institution or custom which once was fairly adequate and efficient" (Frank, 1925a, p. 468), Frank thought the solution should pass by the adaptation of institutions to newer times. However, as scientific discourse was "harmless and impotent to bring change": "men do not change their habits under the spur of words and doctrines" (Frank, 1925a, p. 472), Frank thought behavioral sciences should be directly applied to behavior control:

"if someone would discover a technique of habit-breaking, whereby the ancient customs and institutions cherished by the different social classes could be gently, but effectively, replaced by others more nearly alike and more congruous with the machine technique and its twentieth-century concomitants, it is indisputable that we should all feel happier and more neighborly." (Frank, 1925a, p. 473)

(4) Morris A. Copeland also thought the job of the social scientist should be the study of the habits "of which social organization or property rights consist in any culture, and [to] investigate the history of these prevalent types of behavior – how they came to survive in the process of cultural evolution"

(Copeland, 1926, p. 266)¹³. Among the several topics he dealt with, Copeland was engaged in discussions against the private control of the “educational and informational” techniques of advertising (Copeland, in Cherington and Copeland, 1925, p. 38):

“I wonder if whether we can afford to leave the education of consumers in home economics and standards of living primarily to men who not only are not paid to consider but probably are not aware of the broader social effects of their educational activities.”
(*ibid.*)

It is important to point out this specific feature in Copeland’s work for it serves as a bridge between the early institutionalism of Veblen and Mitchell (their regards on consumption) and the postwar literature on consumer behavior (the economics of affluence). According to Copeland, the importance gained by advertising showed that “consumer’s desires [were] not ultimate determinants of what [got] done by business enterprises” (*ibid.*, p. 40). Advertising, Copeland wrote, “aims to educate consumers’ tastes” (*ibid.*). As these tastes were supposed to be influenced by the strategies of business enterprises, the result was the violation of consumer sovereignty¹⁴.

As noted above, the institutionalism promoted by Mitchell, Frank and Copeland during the interwar period matched quite well with the progressive environment and the conception of science of their time and place. As noted by M. Rutherford (2000), there was “a close linkage between institutionalism and the progressive liberal reform movement” (M. Rutherford, 2000a, p. 298). As the early behaviorism of Watson, this interwar institutionalism was an expression of the American values of that time. According to M. Rutherford, interwar institutionalism

¹³ According to M. Rutherford (2002) Copeland “had a central place within the interwar institutionalist movement” (M. Rutherford, 2002, p. 261). He studied at Amherst College, graduated in 1917 and wrote his Ph.D. thesis in economics under the supervision of J. M. Clark which he finished in 1921. He then moved to Cornell where he stayed until 1927. After three years of work in different institutions Copeland joined the Economics Department of the University of Michigan (1930) and from 1933 to 1944 he worked for the American government. From 1944 to 1952 he worked for the NBER in the “moneyflows” project initiated by W. C. Mitchell and during this period he moved back to Cornell where he retired from in 1965.

¹⁴ As we shall see below, education and learning were core behaviorist subjects. The social scientist, Copeland wrote, “will need to investigate the processes by which typically a child is ‘brought up’ so as to acquire the drives and habits common to most adults, of which in the aggregate our social organization and our institutions consist.” (Copeland, 1926, p. 266)

“could easily have seemed to be a very promising and dynamic program – modern, scientific, engaged in hard critical analysis of the existing economic system and its performance, in tune with the latest in legal and social scientific research, established at leading universities, embedded in leading research institutes, with access to research money and with good government contacts, and involved in important issues of economic policy and economic reform.” (*ibid.*)

It decayed, however. According to recent accounts by historians of economics, the development of “behaviorist” institutionalism by Mitchell, Frank and Copeland generated negative reactions within the movement¹⁵. The context of external circumstances such as the depression, the New Deal, and the advent of Keynesianism, magnified the differences within the camp. According to M. Rutherford, what “had been at the very heart of interwar institutionalism was either knocked down or taken over and recreated in a more orthodox form” (*ibid.*, p. 301).

Behaviorist institutionalism was also strongly attacked because of its aims of behavior control. A clear example of this is Frank Knight’s well-known reaction:

“The social scientist cannot, without being grotesque, place himself over against society in the relation of a gardener to his vegetables [...] the man who would undertake to treat human society merely as material for scientific manipulation, to control it by finding the laws of its response to stimuli and devising stimuli to provoke the responses he might desire, would have to be classed a monster or an imbecile. He might have abundant intelligence, of the scientific sort, but would be lacking in ‘sense.’” (Knight, 1925, pp. 389-390)

It is important to retain the fact that Knight’s attack was explicitly directed against the “sense” of the approach and not against its scientific validity¹⁶.

¹⁵ It is not the object here to deal with the whole history of American institutionalism. It might be useful, however, to mention the fact that the movement was a proper movement in sociological terms despite its ideological heterogeneity which was (in part) responsible for the fall of the program (See Copeland, 1951; Samuels, 2000; Rutherford, 2000, 2001, 2002).

¹⁶ As noted by Hands (2006), Knight’s attack might be credited to his position against all social control, positivism, and pragmatism – a set of ideas which tended to be confused during the interwar period: “this type of mandarin social science was a scientific nightmare for Knight; such scientific monism was exactly the wrong approach to both human science and social policy” (Hands, 2006, p. 586).

3.4. Who's afraid of the Skinner box?: from behavior control to social philosophy

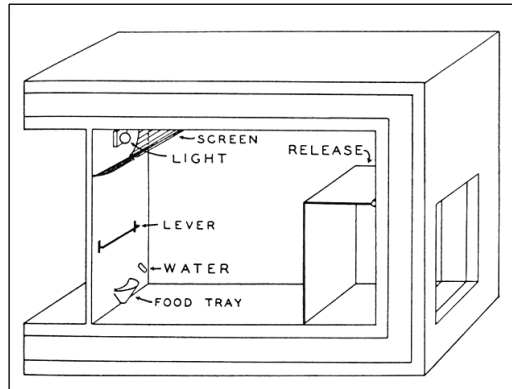
From the late 1930s to the 1970s, there was a renewal of behaviorism in America as “neobehaviorists” started to produce “highly sophisticated and, in some cases, comprehensive psychological theories” (Mills, 1998, p. 4)¹⁷. Of these, B. F. Skinner's contributions (1) were the most visible as he applied his findings on animal experimentation to the analysis of controversial subjects such as baby care, infant teaching and social philosophy (2). During the 1970s, Skinnerian programs of behavior control were strongly contested and socially controlled (3).

(1) As for the earlier behaviorists, behavior, for Skinner, was strictly connected to the growth of habit structures. These structures were conceived as the result of repeated stimulus-response-reinforcement chains, or, in Skinnerian terms, the “history of past reinforcements” encountered by an organism in its interaction with the environment. As this history was supposed to shape individual behavior pattern, an important part of Skinner's program consisted in showing how “seemingly cognitively controlled behaviors could be patiently shaped in the Skinner box” (Mills, 1998, p. 124).

Radical behaviorists like Skinner considered that “to want or desire something [was] to seek that which [had] secured positive reinforcement in the past”, and that to “intend to do something” was “to be guided by one's history of past reinforcements” (*ibid.*, p. 139). They restated mentalist concepts such as “wants”, “desires”, and “intentions” in observable terms which were conceived within behavior control frameworks.

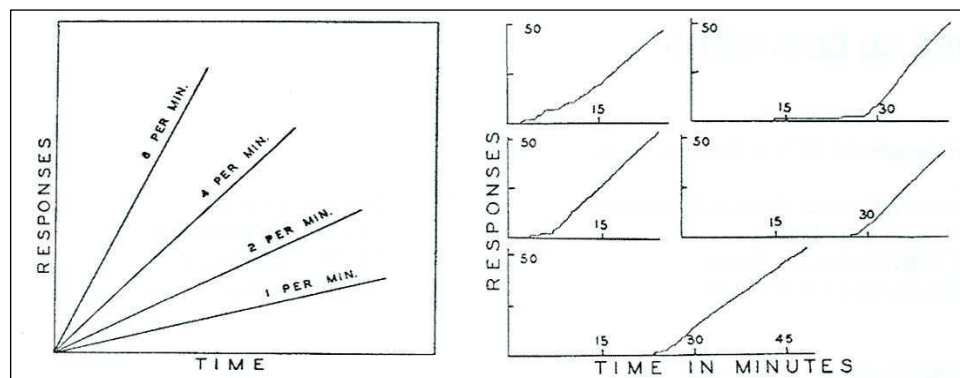
¹⁷ This “neobehaviorism”, however, was by no means a uniform discipline (Mills, 1998). Burrhus F. Skinner [1904-1990] (Harvard), the best-known behaviorist, defended *Radical Behaviorism* as a philosophy of science. He widely promoted the application of his experimental work on Skinner Boxes to human subjects (Skinner, 1938, 1956, 1969). See A. Rutherford (2000, 2003) for an account of the dissemination of Skinner's psychology to the public. Besides Skinner's program, the field was influenced by the variants developed, mainly by Edward C. Tolman [1886-1959], and Clark L. Hull [1884-1952]. Tolman (California, Berkeley) became well-known for his studies of learning in rats using mazes. His main volume, *Purposive Behavior in Animals and Men* (1932), was influenced by Gestalt psychology and produced a less mechanistic theory than the ones of Hull or Skinner. Hull (Wisconsin, Madison) worked on learning and motivation, and showed he could predict and control animal behavior in his *Mathematico-Deductive Theory of Rote Learning* (1940), and *Principles of Behavior* (1943).

Figure 3.1 (Skinner, 1938, p. 49)



The Skinner box (Fig. 3.1) provided controlled environments in which random behaviors of the animals under experiment (mainly hungry pigeons) could be positively reinforced by providing them food through mechanic devises. In this way, desired behaviors were obtained by modifying the environment (the elements in the box) of which these behaviors were supposed to be the outcome. The Skinner box also provided “behavior records” of the responses to positive reinforcements (movements of the lever) which were plotted as numbers of responses against time. Experiments in the Skinner box were intended to obtain rates of response which appeared as slopes in behavior records (Fig. 3.2).

Figure 3.2 (Skinner, 1938, pp. 59, 68)



(2) After World War II, Skinner became increasingly engaged in the social application of his findings and from the 1940s to the 1970s he joined the discourse of the earlier behaviorists by showing that it “was possible to

develop social technologies to shape human beings” (Mills, 1998, p. 149). It was by these means “that science should serve the good of society” (*ibid.*)¹⁸. As did the earlier behaviorists, Skinner transposed behavior control in the analysis of animal behavior to the analysis of social subjects. This included the use of behavior technologies as a means of promoting social efficiency, productivity, and “appropriate modes of socialization” which would lead to “humanist goods (such as a healthy and well-balanced psychic life)” (*ibid.*, p. 153)¹⁹.

In 1945, Skinner introduced his “Air-Crib”, or mechanical baby tender, in the *Ladies Home Journal*. Skinner’s apparatus was intended to be a “labor-saving invention” designed for “the problem of the nursery” (Skinner, 1945, p. 29). It consisted of a “closed compartment about as spacious as a standard crib” where temperature and humidity could be controlled and the passing air filtered so that the baby in care could be freed from clothing (except diapers), bedding, and excessive bathing so that the mother could gain “freedom for other activities” (*ibid.*, p. 32). The Air-Crib was by no means a Skinner box, as there were no reinforcement devices in its mechanism which was conceived as a technological means to improve housekeeping (remember Mitchell’s “Backward Art”). “Baby in a Box” was however the title under which the Skinnerian “inexpensive mechanization of baby care” (*ibid.*, p. 34) was published, a reflection of the skepticism with which his ideas about the social use of technology were received.

In 1948, Skinner presented his ideas to the general public in *Walden two*, a utopian novel about an experimental community designed to deal with postwar problems. For Capshew (1993), this was the “first step in Skinner’s public transformation from experimental psychologist to social philosopher” (Capshew, 1993, p. 836). Although the book did not “sell well” until the end of the 1960s (A. Rutherford, 2000, p. 382; Mills, 1998), over 2 million copies

¹⁸ According to Capshew (1993), it was Skinner’s wartime engagement in “Project Pigeon”: “an attempt to construct a missile guidance system utilizing the conditioned pecking of pigeons” (Capshew, 1993, p. 836) that marked his transition “from inventive scientist to social inventor” (*ibid.*).

¹⁹ According to Mills (1998), behaviorist laboratories became training grounds for “social technocrats who could induce socially desired outcomes in natural settings” (Mills, 1998, p. 88). For Mandler (2007), these views were “consistent with a number of old and new American cultural and social values” (Mandler, 2007, p.101), namely “the drive for scientific management and the time-and-motion studies of Frederick W. Taylor designed to make the American worker more productive at less cost” (*ibid.*).

were sold during the following decade, promoting vivid discussions around the social use of behavior technologies (*ibid.*)²⁰. In the introduction to the 1976 edition of *Walden two*, Skinner acknowledged that the original edition of the utopian novel (1948) was strongly influenced by his vision of post-war problems. The new world, he wrote in the new edition:

“was beginning to face problems of an entirely new order of magnitude – the exhaustion of resources, the pollution of the environment, overpopulation, and the possibility of a nuclear holocaust, to mention only four. Physical and biological technologies could, of course, help [...]. But that would happen only if human behavior changed, and how it could be changed was still an unanswered question [...].” (Skinner, 1976, pp. vi-vii)

Skinner thought these postwar problems were due to the lack of progress of the sciences of human behavior. He thought that while “Aristotle could not have understood a page of modern physics or biology” (Skinner, 1971, p. 6), Socrates and his friends would have had “little trouble in following most current discussions of human affairs” (*ibid.*)²¹. As noted above, from the end of WWII until the late 1970s, Skinner was engaged in a social discourse promoting the use of behavior technologies to improve society. For Skinner, behavior “could be changed by changing its consequences” through operant conditioning (Skinner, 1976, p. viii). He showed that by means of positive reinforcements, behaviors could be corrected using neither coercive measures nor punishments. Applied to consumer behavior, Skinner’s advice was the following:

“To induce people to adapt to new ways of living which are less consuming and hence less polluting, we do not need to speak of frugality or austerity as if we meant sacrifice. There are contingencies of reinforcement in which people continue to pursue (and even

²⁰ A. Rutherford (2000) deals with the reception by the public of Skinner’s technologies of behavior control (mechanical baby tenders, teaching machines, utopian novels and social philosophies).

²¹ Skinner thought the lack of progress in the understanding of human behavior was the effect of powerful ideological preconceptions against the use of technologies of behavior control: “the control of human behavior has always been unpopular. Any undisguised effort to control usually arouses emotional reactions [...]. Those who have explicitly avowed an interest in control have been roughly treated by history [...] this attitude spells trouble for any science which may give birth to a powerful technology of behavior [for] the prediction and control of individual behavior is regarded as little less than the work of the devil.” (Skinner, in Rogers and Skinner, 1956, p. 13)

overtake) happiness while consuming far less than they now consume.” (Skinner, 1976, p. x)²²

(3) Despite Skinner’s humanist principles, the reception of his program was highly controversial (A. Rutherford, 2000, 2003). Skinner himself showed that the negative reception of *Walden Two* was a proof of the “uneasiness with which” government was viewed while attempting to control behavior by positive reinforcements rather than punishments (Skinner, in Rogers and Skinner, 1956, p. 17). For Skinner, the aversion with which his work was received came from misunderstandings in the meanings of both “behavior control” and “freedom”. He thought the (unscientific) discourse on freedom masked the fact that behavior control was *always* present (Skinner, 1971). For Skinner, behavior control was such an everyday practice that the inexperienced eye failed to recognize positive reinforcements in common use²³. Because all “men control and are controlled”, the question to ask was not how freedom could be preserved, but “what kinds of control [were] to be used and to what ends”:

“The problem is to free men, not from control, but from certain kinds of control, and it can be solved only if our analysis takes all consequences into account. How people feel about control, before or after the literature of freedom has worked on their feelings, does not lead to useful distinctions.” (Skinner, 1971, p. 42)

For Skinner, freedom – or in his terms, behavior that “feels free” – was “also the product of a history of conditioning” (Skinner, 1964, p. 483). He believed that the misunderstanding of control in human behavior interfered with the free exercise of a scientific analysis.

²² Skinner’s utopian book *Walden two* made some references to Veblen’s theory as the following extract shows: “do you remember Veblen’s analysis of the lawn in the *Theory of the Leisure Class*?” “I do, indeed,” said Castle. “It was supposed to represent a bit of choice but conspicuously unconsumed pasture.” [...] “That’s right,” said Frazier, with a slight smile. “Well, this is our lawn. But we consumed it. Indirectly, of course – through our sheep. And the advantage is that it doesn’t consume us” (Skinner, 1948, p. 15).

²³ A familiar problem, he wrote, “is that of the child who seems to take an almost pathological delight in annoying his parents. In many cases this is the result of conditioning [...]. The mother may unwittingly promote the very behavior she does not want. For example, she may answer the child only when it raises its voice [...]. The mother behaves, in fact, as if she had been given the assignment of teaching the child to be annoying! The remedy in such a case is simply for the mother to make sure that she responds with attention and affection to most if not all the responses of the child which are of acceptable intensity and tone of voice and that she never reinforces the annoying forms of behavior” (Skinner, 1959, p. 419).

During the 1970s, Skinnerian-type research on behavior was strongly resisted and censured by social control (A. Rutherford, 2006). The most critical reactions followed Skinner's publication of *Beyond Freedom and Dignity* (1971), a volume on social philosophy which extended his scientific findings to the analysis of social subjects. *Beyond Freedom and Dignity* was at the same time a best-seller and Skinner's "most widely debated book" (A. Rutherford, 2006, p. 204). The interesting thing to be noticed here is that Skinner's views were not scientifically but socially contested. His detractors did not base their claims on the scientificity of Skinner's position but on his values. More than caring about the "scientific validity" of Skinner's claims, his detractors reacted by charging Skinner of committing "a serious affront to traditional value systems" by "renouncing the freedom, dignity, and autonomy of human beings" (A. Rutherford, 2000, pp. 385-386). A similar reaction awaited applied research using Skinnerian "behavior modification". According to A. Rutherford's (2006) account, the social reaction against behaviorism and its applications represents a "distinctly illustrative" episode "of the processes through which psychological science and its products" are "shaped, regulated, and modified by the society in which they are embedded" (*ibid.*, p. 218). The following quote presented by A. Rutherford gives a clear picture of the episode:

"Whether or not behavior scientists really can control much behavior, they have recently convinced the public that they can. Many are sorry now that they have done so. For instead of getting Nobel prizes and gratitude, they have been viewed with increasing suspicion, if not revulsion, and threatened with restriction – of funds, of sponsorship, even of access to subjects... And the prospects are for more, not less, restriction... *The social control of behavior control is underway.*" (P. London, 1974, in A. Rutherford, 2006, p. 203)

3.5. Were the ordinalists behaviorists?

As noted above (Introduction), taking the "ordinalist revolution" as a behaviorist move in economics has become common knowledge among economists and even historians of economics:

“A behaviorist movement arose in economics, as theorists attempted to free economics of all psychological elements. This movement contributed to the replacement of the older theory of cardinal utility, with the new notion of ordinal preferences.” (Lewin, 1996, pp. 1294-1295, and many other economists and historians of economics)

The aim of this section is to show that the ordinalists’ conception of consumer behavior was completely at odds with the behavior control approaches held by behaviorists. The fact that economists from I. Fisher [1867-1947] to P. Samuelson [1915-] became engaged in a crusade against the economic use of psychic concepts such as “(cardinal) utility” or “preferences” may seem to suggest that some of these authors were embracing behaviorism. This was not the case (1). It seems that the ordinalist enterprise has been labeled “behaviorist” because of Samuelson’s operationist attempts, which were, albeit, different from Skinner’s (2). As Samuelson’s view of consumer behavior was strongly linked to the assumption of consumer sovereignty, his conception of behavior was by no means behaviorist, but rather consistent with the psychological underpinnings of consumer choice theory (3).

(1) The standard account of the history of economics and psychology consists in taking the ordinalist turn as a progression towards the separation of the two disciplines²⁴. There are, however, several recent accounts by historians of economics showing that even if the theories of E. Slutsky [1880-1948], J. Hicks [1904-1989], R. Allen [1906-1983] and P. Samuelson were genuine attempts to “free” economics from its psychological foundations, they inevitably produced versions of the theory which were based on mentalist elements, and therefore ultimately on introspection²⁵.

Starting from the statement that “the theory of value [did] not need any precise definition of marginal utility”, Hicks and Allen (1934) established the

²⁴ In Giocoli’s (2003) terms there was an “escape from psychology”: “By the term ‘escape from psychology’ I mean the efforts in the first half of the 20th century to cleanse the neoclassical theory of economic behavior of any reference to psychic concepts such as pleasure, motivation, utility and so on. Starting from Fisher and Pareto, going through, among others, Slutsky, Hicks and Samuelson, and ending with von Neumann and Morgenstern, Savage and Debreu, *the process led to a psychology-free theory of rational behavior* under conditions of both certainty and uncertainty” (Giocoli, 2003, p. 41, emphasis added).

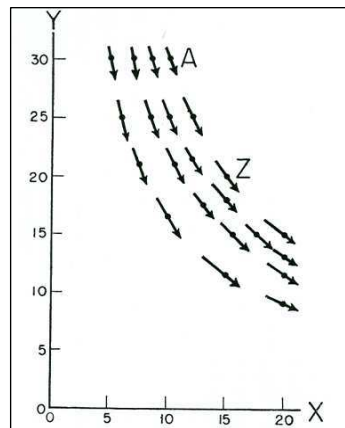
²⁵ The standard account of the “ordinalist revolution” (or escape from psychology) starts with Pareto’s work (sometimes Irving Fisher’s [1867-1947]) and continues with that of Slutsky (1915), Hicks and Allen (1934) and Samuelson (1938, 1947, 1948). It is not the object here to deal with the whole story. See Bruni and Guala (2001) for a critical account of the history of Pareto’s ordinalism. See Hands (2007) for a critical account of the standard history of economics and psychology.

“*marginal rate of substitution* between any two goods” as the starting point of the theory (Hicks and Allen, 1934, p. 55). But even if this starting point was supposed to depend “simply upon the system of indifference-curves” (*ibid.*, p. 56), and even if Hicks and Allen’s “refinements” gave the theory of consumer behavior “a degree of rigorousness and exactness” exhibited by “the natural sciences only” (Georgescu-Roegen, 1936, p. 546), the method of the authors remained “that of the mental experiment aided by introspection” (*ibid.*). Mentalism still permeated the theory²⁶. Two years after the publication of Georgescu-Roegen’s “Pure Theory of Consumer’s Behaviour” (which did not deny introspection), Samuelson’s “Note on the Pure Theory” (1938) added new refinements intended to drop off “the last vestiges of the utility analysis” (Samuelson, 1938, p. 62). These refinements were supposed to take the theory a step further (albeit not a step closer to behaviorism) by “conceptually” observing consumer behavior (Samuelson, 1948).

(2) Operationism (or operationalism) was the methodological reference for both Skinner and Samuelson and it is worth noting that these two authors made simultaneous references to Bridgman’s methodology (Boring, 1950; Mills, 1992, 1998; Hands, 2004). However, the different ways in which these authors handled operationism give a clear illustration of the strong differences between the behaviorists’ conception of behavior and that implied in Samuelson’s theory of revealed preferences. Samuelson’s approach consisted in (conceptually) changing prices and recording bundles so that “the consumer’s preferences could be ‘revealed’ and thus rendered operationally meaningful by the ‘operation’ of preference revelation” (Hands, 2004, p. 957). This means that Samuelson’s operationism was intended to reveal something coming from “inside” the consumer. It is in this sense that Samuelson’s theory involved mentalism.

²⁶ According to Lewin, as the theory involved purposiveness, it assumed “(ordinal) preferences that really mean[t] something psychologically [...]. Hicks and Allen had provided a good foundation for a genuine declaration of independence from psychological hedonism, but not from psychology proper” (Lewin, 1996, p. 1310).

Figure 3.3 (Samuelson, 1948, p. 244)



Samuelson's operationism consisted in drawing "the budget-equation straight line with arithmetical slope given by the observed price ratio" through "any observed equilibrium point, A" as shown in the figure (Samuelson, 1948, p. 244). Ironically, this procedure was supported less by behaviorist experimentation than by insights from Gestalt psychology (Chapter 5). Samuelson did mention a "human guinea-pig" twice in his 1948 paper. However, when passing from "the numerous little arrows" he obtained through the preference revealing operation to the "behaviour curves" (i.e. the indifference curves), he based his conclusions on the "mathematical counterpart" of the "well known observation of *Gestalt* psychology that the eye tends to discern smooth contour lines from such a representation" (*ibid.*, p. 245).

It is clear that rather than "eliminating intentional notions such as preference and utility", Samuelson's theory provided "scientific legitimization for precisely the same concepts" (Hands, 2004, p. 962; Sen, 1973). In Hands' words:

"it is now well-established that the operationalist-inspired project of basing demand theory on revealed preferences was a failure from a variety of different perspectives. Not only did the original project of purging preference and utility from economic analysis fail but the later goal of providing a practical way of 'revealing' those preferences was also unsuccessful even on its own (revised) terms. Whatever the reasons are that economists believe in demand theory, the claim that revealed preference theory has provided

consumer choice theory with incorrigible operational/empirical foundations doesn't seem to be – or certainly shouldn't be – one of them.” (Hands, 2004, p. 962)²⁷

Among behaviorists, it was Skinner who “applied operationism in the most radical way to psychology” (Mills, 1992, p. 76). Skinner’s program consisted in eliminating “motivational states” from behavior analysis by using “drives” which were defined “in terms of operations carried out by an experimenter” (Mills, 1992, p. 76). These drives were “external” to the organisms under study. Hunger, for instance, was defined as the “reduction of an animal’s body weight to 80% of the free-feeding level or placing the animal on a 23 1/2-hour feeding schedule” (*ibid.*).

Skinner replaced motivational states by variations in drive intensity which could “be very precisely correlated with behavioral outcomes (such as the rate and the pattern of bar-pressing or key-pecking in a Skinner box)” (*ibid.*). Because behaviorists had precise control not only over the tasks performed by the animals, but over all the “relevant aspects of [the] animal’s lives”, they could predict, “no matter in what laboratory [...], precisely what the outcome of those tasks would be” (Mills, 1992, p. 77). Skinner’s project did not involve mentalism. It failed, however, because it defined the variables “solely in terms of the operations carried out by the investigator” (*ibid.*). In Mill’s words, the “Skinnerian experiment became no more than an empirical check on the internal consistency of the concepts” (*ibid.*).

(3) Comparing Skinner’s and Samuelson’s approaches, it seems safe to claim that the ordinalists’ account of behavior was clearly not behaviorist if one respects the definition given by behaviorists themselves (which was quite clear)²⁸. Samuelson’s project was far from behaviorism which, in Skinner’s

²⁷ According to Lewin (1996), “utility theory remained almost useless to economists who analyzed actual data” (Lewin, 1996, p. 1304). The few experimental incursions made by economists in order to test demand theory are presented by Moscati (2007), and economic experiments with animal behavior are presented in Kagel *et al.* (1981) and their references.

²⁸ It seems that the “behaviorist mainstream economics” label does not come from the ordinalists but from defenders of the use of introspection in economics such as Lionel Robbins and Frank Knight. These “anti-behaviorist” authors argued in favor of means/ends versions of economics generating methodological debates which were however far from the present discussion about behavior control. In defending his theory about “choice under scarcity” based on the “forward-looking notion of purposive behavior” (Hands, 2009, p. 12) Robbins wrote that purposive behavior was “not susceptible of observation by purely *behaviourist* methods” (Robbins (1932) in Hands (2009, p. 12), emphasis added). Frank Knight considered the ordinalists’ “diminishing ‘coefficient of substitution’ of one good for

terms, consisted in analyzing behavior as the outcome of histories of past reinforcements.

Mirowski (2002) shows that Samuelson's program of revealed preference theory was not intended to fit actual data but consisted in an "operationist restatement of the dictum that in order to qualify as a neoclassical self in good standing", the agent should "pass the test of perfect self-consistency" (Mirowski, 2002, p. 447). It is interesting to realize that what marked the failure of Skinner's program is seen as a progress in the history of economics.

3.6. Dismissing J. K. Galbraith and ignoring the economists of affluence

As noted in Chapter 1, the economics of affluence, as defended by J. K. Galbraith and his followers, became conspicuous from the 1950s to the 1970s. During this period it shared the scene with Skinner's social philosophy. Agreeing with Skinner, economists of affluence thought of consumer behavior as being subject to behavior control (1). Unlike Skinner, however, they faced "freedom and dignity" arguments disguised as scientific claims (2). These claims were based on views of consumer behavior which were incompatible with the program attempted by both economists of affluence and early economists of happiness (3).

(1) Galbraith's analysis introduced in *The Affluent Society* (1958) was furthered in *The New Industrial State* (1967) and spread to the general public in *The Age of Uncertainty* (1977). *The New Industrial State* introduced Galbraith's concept of "revised sequence" which was a complement to the "dependence effect" mechanisms introduced in *The Affluent Society*. The revised sequence was proposed as an alternative to the "economic analysis and instruction" according to which the initiative was assumed "to lie with the consumer" (Galbraith, 1967, p. 211). It was directed against the assumption of consumer sovereignty and based on the idea that firms had the faculty of controlling both the market and the social attitudes of consumers:

another" as a "purely behavioristic principle, or at least purely relative" (Knight, 1944, p. 289, emphasis added).

“The unidirectional flow of instruction from consumer to market to producer may be denoted Accepted Sequence. [...] this sequence does not hold [...]. Instead the producing firm reaches forward to control its markets and on beyond to manage the market behavior and shape the social attitudes of those, ostensibly, that it serves. For this we also need a name and it may be appropriately called The Revised Sequence.” (*ibid.*, p. 212)

According to Galbraith, the mere recognition of the revised sequence sent “to the museum of irrelevant ideas” the notion of an equilibrium reflecting “the maximum of consumer satisfaction” (*ibid.*, p. 213). As in *The Affluent Society*, Galbraith advanced the problem as “not one of original error but of obsolescence” (*ibid.*, p. 215):

“When goods were less abundant, when they served urgent physical need and their acquisition received close thought and attention, purchases were much less subject to management [...]. The model of consumer behavior, devised for these conditions was not wrong. The error was in taking it over without change into the age of the industrial system. There, not surprisingly, it did not fit.” (*ibid.*)

The same year (1967), E. Mishan published *The Costs of Economic Growth* which was also critical concerning the “Myth of Consumers’ Sovereignty”²⁹. Mishan showed that it was not correct to speak of the market as “acting to adapt the given resources of the economy to meet the material requirements of society” unless the wants of consumers existed “*independently* of the products created by industrial concerns” (Mishan, 1967, p. 148):

“not only do producers determine the range of market goods from which consumers must take their choice, they also seek continuously to persuade consumers to choose that which is being produced today and to ‘unchoose’ that which was being produced yesterday. Therefore to continue to regard the market, in an affluent and growing economy, as primarily a ‘want-satisfying’ mechanism is to close one’s eye to the more important fact that, that is has become a want-*creating* mechanism.” (*ibid.*, pp. 148-149)

²⁹ In 1960, Mishan had published a “Survey of Welfare Economics, 1939-1959” advancing the problem of consumer sovereignty as an obstacle for the development of the subfield. Mishan (1960) presented Duesenberry’s theses as well as Galbraith’s analysis of the affluent societies. *The Costs of Economic Growth* (1967) was clearly an economic analysis of affluence. It was divided in ten parts: (1) “Economics: Choice or Necessity?”, (2) “Market Failure”, (3) “Transport and the City”, (4) “The Myth of Consumer Sovereignty”, (5) “Measuring Real Income – an Economic Delusion”, (6) “Institutional Obstacles to Environmental Sanity”, (7) “Towards an Admass Future”, (8) “Pluralism, Conflict and Repression”, (9) “Further Reflections on Things to Come”, and (10) “By Way of Epilogue”. See Johnson (1960) for an early account of the “Political Economy of Opulence”, and Stanfield (1977) for an account on the relationship between Galbraith’s institutionalism and Marxism.

Like Galbraith, Mishan showed that the choices open to consumers were highly controlled by the environments in which they occurred and that there were important institutional restrictions reducing “effective choice” (*ibid.*, p. 152)³⁰. In regards of the control exerted by producers, he showed that an important part of business activities consisted in using resources “not to create satisfactions, but to create dissatisfactions with what people possess[ed]” (*ibid.*, p. 153). He thought business in the fashion industry was directed “to create obsolescence in otherwise perfectly satisfactory goods” (*ibid.*) and that the “increasing frequency of fashion change” had been extended to articles as “automobiles, furniture, hardware, and electrical goods” (*ibid.*).

(2) In 1977, Galbraith spread his views to the general public in *The Age of Uncertainty*, a project consisting in both a BBC series and a book. The aim of the project was to contrast the “great certainties in economic thought” in past centuries with the “great uncertainty” with which social problems were faced after WWII (Galbraith, 1977, p. 7):

“In the last century capitalists were certain of the success of capitalism, socialists of socialism, imperialists of colonialism, and the ruling classes knew they were meant to rule. Little of this certainty now survives. Given the dismaying complexity of the problems mankind now faces, it would surely be odd if it did.” (*ibid.*)

It is worth noting that *The Age of Uncertainty* was published the year after B. F. Skinner’s best-seller edition of *Walden two* (1976, Section 3.4)³¹. At this

³⁰ “Over the decision most vital of all to his well-being, the epoch and society wherein he lives, the individual alas is unable to exercise any choice whatsoever. Born into a certain social and physical *milieu*, born into a certain home, much of the pattern of his life follows as a matter of course. Many of the consequences that arise from nature and nurture, from inherited natural endowments and from upbringing, he will be powerless to influence. Within limits determined by these consequences he is, later, free to choose an occupation but, having adopted it, the material choices that he exercises through the market are thereafter somewhat narrowly conscribed. If, for example, he becomes a stockbroker or bank manager in the City, his choice of clothes, car, residence, even his choice of food and entertainment, will nor differ markedly from that of his colleagues. The conventions followed by friends, associates, and customers will continually weigh with him unless he is ready to forfeit their good opinion of his character and soundness upon which his success depends.” (Mishan, 1967, p. 152)

³¹ *The Age of Uncertainty* was broadcasted in 1977. The book consisted in 12 chapters: “1. The Prophets and Promise of Classical Capitalism” (pp. 11-42), “2. The Manners and Morals of High Capitalism” (pp. 43-76), “3. The Dissent of Karl Marx” (pp. 77-108), “4. The Colonial Idea” (pp. 109-132), “5. Lenin and the Great Ungluing” (pp. 133-160), “6. The Rise and Fall of Money” (pp. 161-196), “7. The Mandarin Revolution” (pp. 197-226), “8. The Fatal Competition” (227-256), “9. The Big Corporation” (pp. 257-279), “10. Land and People” (pp. 280-302), “11. The Metropolis” (pp. 303-323), and “12. Democracy, Leadership, Commitment” (pp. 234-342). The series included 12 episodes based on the book chapters and

moment both Skinner and Galbraith were highly recognized by the American public as scientific personalities addressing problems concerning affluent societies: over consumption, pollution, and the threat of a nuclear holocaust to name a few³². The same year, M. Friedman produced the most conspicuous reaction against the economics of affluence. In his lectures entitled *From Galbraith to Economic Freedom* (1977), Friedman presented his own criticism and those of other economists concerning Galbraith's discourse about the role of industry in the market economy³³. Against Galbraith's argument, focused on the power of industry in shaping "the tastes of the public to satisfy their own interests" (Friedman, 1977, p. 15), Friedman held a "freedom and dignity" discourse about the role of commercial advertising:

"for those of us who believe in the dignity of the individual human being, in the pre-eminence of freedom among human beings as the objective of social organization: we must say that the only way in which we have any right to try to affect the values of others is by persuasion. And that, I may say, includes commercial advertising, which I view as a form of free speech [...]" (*ibid.*, p. 34)

The important point here is that unlike the reactions against behaviorism (Section 3.4), the claims of Friedman and his followers were presented as scientific arguments. They stated that there was "no evidence" (*ibid.*, p. 23) supporting Galbraith's views about the power to control of the "technostructure-oriented firms" (*ibid.*, p. 24), and inquired whether Galbraith's position was "scientist or missionary" (*ibid.*, p. 25)³⁴:

three additional ones featuring a *Weekend in Vermont* in Galbraith's farm (with his wife) and different "world leaders" (and their wives) who discussed the questions raised by Galbraith's program.

³² The last section of Galbraith's book dealt with "The Nuclear Evasion" (Galbraith, 1977, p. 342), which he thought of as the main source of uncertainty: "after the first exchange of missiles, [...], the ashes of communism and the ashes of capitalism will be indistinguishable. Not even the most passionate ideologue will be able to speak of the difference, for he too will be dead. In an age when so much is uncertain, there is one certainty: This truth we must confront" (*ibid.*).

³³ Friedman's lectures took place in 1976, the year he received the Nobel memorial price from the Bank of Sweden. They were published the year after and presented as "an economist's view of Galbraith" (Friedman, 1977, p. 3). Friedman's position consisted in arguing that Galbraith's theories had "never found acceptance in the *academic world*" and showing that their acceptance had "been in the *public world*" (*ibid.*, pp. 35-36). In 1980 Friedman produced *Free to Choose*, a 10-episode audiovisual response to *The Age of Uncertainty*.

³⁴ The preface to the U.K. edition maintained that the aim of the volume was to show that Galbraith's arguments were "based on hypotheses that, when tested by appeal to the evidence" appeared "to be unfounded" (*ibid.*, p. 5). The volume included criticisms to Galbraith's concepts by G. Stigler, H. Demsetz, R. Solow, G. C. Allen, J. Jewkes, and F.

“so far as I know, apart from Galbraith’s own assertion, *there has been no successful defence of this view of the world*. That does not mean there are no defenders of the view. There are many. There are many who accept it. But I know of no scientific studies which have validated that view of the world as meaningful and accurate in the sense that it yields predictions about the behaviour of enterprises, of industry, or of the economy as a whole that can be checked, tested against evidence, and found to hold.” (*ibid.*, p. 29, original emphasis)

(3) Friedman and his followers were eventually successful in excluding the economics of affluence from the mainstream of their discipline, and two important conclusions may be drawn from this event before proceeding to Chapter 4. First, that there is a strong contrast between the reaction of social control faced by behaviorist psychologists such as Skinner (Section 3.4), and the mainstream economists’ reaction to the economics of affluence (J. K. Galbraith’s in particular). The reaction against the economics of affluence was based on freedom and dignity discourses disguised as scientific claims. This reaction shows how economic theory is ambiguously shaped by values.

Second, given the psychology supporting the economics of affluence, it is not surprising that this program failed to make its way to the mainstream of the discipline. The economists of affluence’s approach, as that of the early economists of happiness, opposed economic accounts of behavior by challenging the principle of consumer sovereignty. These approaches were based on views of consumer behavior that stressed the importance of external factors in the formation of preferences. These views were closer, both in content and in context, to behaviorism than to the psychological assumptions supporting consumer choice theory (even in its “behaviorist” version). The best example of the contrast between these two positions is Samuelson’s theory; a theory committed to consumer sovereignty, which was the main source of contention for the economists of affluence:

“In *The Affluent Society*, J. Kenneth Galbraith has eloquently pointed out that Americans today have for the most part gone beyond the level of physiological necessity; that often

McFadzean. The inclusion of these criticisms was based on the claim that “the writings of Professor Galbraith [were] far better publicized than the works of economists who [thought] his work [was] flawed” (*ibid.*, p. 6). The volume maintained that while Galbraith was “one of the best-known economists to non-economists”, his work had “made very little impact on economics or on his fellow-economists” (*ibid.*, p. 8). See Gordon (1968), Meade (1968) and Galbraith (1969) for discussions about *The New Industrial State* (1967).

the consumer flits from one purchase to another in response to pressures of fashion and advertising [...]. Whether or not people would be 'genuinely' happier spending twice as much now, observation suggests that folks in the suburbs now act *as if* they want more income to spend: they take on extra work; they resist tax increases; they end up saving much the same fraction of their incomes as in 1900; and middle-class mothers seem to work harder than their mothers did." (Samuelson, 1970, p. 16)

There was nothing of a behavior control approach in Samuelson's view of consumer behavior. The account defended by Samuelson was based on the idea that the things produced were "determined by the *dollar votes* of consumers" (*ibid.*, p. 40). For most of the readers of Samuelson's *Economics*, consumers are, so to speak, "Sovereign Kings".

Chapter 4. The Economists' Psychologies: Mechanic, Consistent, Behavioral

4.1. Introduction

Chapter 3 established the lack of place for behavior control in economic theories of consumer behavior. It presented the economists' rejection of both functional and behaviorist psychologies through the rise and fall of early and interwar institutionalism, as well as the lack of success of the economists of affluence's approach. Chapter 4 deals with another part of the history. By taking into account the psychological underpinnings of choice theory, it offers a complementary history for Chapter 3. In regards to the economics of happiness, this chapter is intended to show that unlike the early literature (1974-1999), the recent development of the subfield (1999-2009) is supported by a psychology which is not only inconsistent with behaviorism, but moreover compatible with the psychology that currently supports mainstream economics.

As noted above (Introduction), the recent "success" of the economics of happiness (1999-2009) is closely related to the emergence of behavioral economics as a mainstream of the discipline. The main thesis of this chapter is that unlike its presentation in earlier literature (1974-1999), the economics of happiness is now consistent with the mainstream view of economic behavior. As in Chapter 3, this chapter proceeds by exploring a rather long history. Section 4.2 establishes the origins of experimental psychology, giving an overview of the main elements upon which W. S. Jevons and F. Y. Edgeworth developed their theories, namely, elements of psychophysics which were also essential to the development of Wundt's program of experimental psychology. There were, however, radical differences between Wundt's psychology (and Fechner's psychophysics) on one hand, and the psychological assumptions supporting the theories developed by the British marginalists, on the other. In Wundt's case, the analysis of sensations was devoted to the study of three properties: intensity, quality, and feeling. The "fundamental law of psychophysics" (Fechner's Law) was only applied to the

first of these properties and not to the (economic-type) analysis of “feelings” of pleasure or pain.

Section 4.3 shows that during the late 19th century, Fechner’s findings (which were not new) were largely used as a means to provide natural foundations for the human sciences. They were applied in the analysis of different subjects not only by Fechner and Wundt themselves, but also by other scientists who formed analogies out of the fundamental law. This was the case of Jevons and Edgeworth whose strategy was peculiar. Unlike Wundt or the other experimentalists, these economists used psychophysical elements as axioms from which a theory of economic behavior could be deduced. Section 4.4 shows that the use of Fechner’s Law by these early marginalists and some of their followers led to strong reactions by Weber (1908), and Mitchell (1910).

Sections 4.5 to 4.9 deal with the more recent history of the relationship between economics and psychology. Section 4.5 presents N. Giocoli’s (2003) account on the mainstream attempts to introduce dynamic considerations into general equilibrium theory and P. Mirowski’s (2002) analysis of the mainstream reaction to J. von Neumann’s information-processing approach to show that ambiguity has been the rule rather than the exception in the history of the relationship between economics and psychology (Chapter 3 already showed that Samuelson’s “behaviorism” was not really behaviorist). Section 4.6 introduces Harry Helson’s *Adaptation-Level Theory* (1964), and Section 4.7 presents part of a symposium held in the honor of this theory in 1971, which is an important event in the history of the economics of happiness as it produced Brickman and Campbell’s “Hedonic Relativism and Planning the Good Society”. This contribution extended *Adaptation-Level Theory* to the analysis of happiness. Section 4.8 shows that Helson’s theory was applied to the analysis of economic subjects in two different ways: by Scitovsky (1976) and the early economists of happiness on one hand, and by Kahneman and Tversky (1979) on the other. Whereas the first failed to renew consumer choice theory, the second led to the development of (the “new”) behavioral economics, which is now acknowledged as a mainstream subfield. Section 4.9 gives an overview of the recent development of behavioral economics and shows that both behavioral economics and the economics of happiness are being developed by similar strategies: behavioral economics by advancing partial elements of Helson’s theory as behavioral deviations from the standard

analysis of consumer behavior; and the economics of happiness, by refusing the full implications of adaptation in regards to the analysis of happiness data.

4.2. From Fechner's psychophysics to Wundt's experimental psychology

Psychophysics was a method to measure the intensity of a sensation by controlling the intensity of its corresponding stimulus. Gustav T. Fechner [1801-1887] led the program to its maturity in the generalization of his own findings (and those of Ernst H. Weber) which led to the formulation of the "fundamental law of psychophysics" or "Fechner's law" (1)¹. Shortly following Fechner's findings, Wundt founded physiological psychology in an attempt to develop a scientific psychology from psychophysical experimentation. He used Fechner's findings in order to analyze the intensity of sensations, but his program was not restricted to this kind of analysis. It also incorporated the qualities and feelings of sensations (2).

(1) According to Boring (1966), the fundamental law of psychophysics was a serendipitous finding emerging from Fechner's philosophical project of connecting "the material and the mental" (Fechner, 1860, p. 7)². Fechner's

¹ Psychophysics was developed mainly in Germany during the second half of the 19th century but the history of psychophysics is also related to the older histories of both physics and physiology. According to Boring (1966), both Newton (1704) and Bouguer (1729) were involved in the analysis of visual sensations in the early 18th century. Physiologists entered the field during the early 19th as "Charles Bell in 1811 and François Magendie in 1827" found that "sensory and motor nerves constitute[d] different systems, since they [were] connected with the spinal cord at different roots" (Boring, 1966, p. x). Anatomist and physiologist Ernst H. Weber [1795-1878] has been retained in the history for his 1834 contribution where he "worked out the discriminatory thresholds for the tactual sense and formulated the generality that this threshold is proportional to the magnitude of the stimulus for which it is observed, a rule that has come to be called Weber's law." (*ibid.*). The history then turns directly to widespread research on Weber's findings, mainly by Fechner, Helmholtz and their followers.

² Fechner's biography is indeed quite interesting. As presented by Boring (1966): "He was for 7 years a physiologist (1817-1824), for 15 years a physicist (1824-1839); for 12 years and invalid (1839 to 1851); for 14 years a psychophysicist (1851-1865); for 11 years an experimental aestheticist (1865-1876); for at least 40 years throughout this period, recurrently and persistently a philosopher (1836-1879)" (Boring, 1966, p. xvii). Fechner's philosophical project was first exposed in *The Little Book of Life after Death* (1836) in which he started to elaborate his "daylight-view" of mind and matter. This daylight-view "meant the anti-materialistic view, – the view that the entire universe, instead of being dead, is inwardly alive and consciously animated." (James, 1904b, p. x; Boring, 1966)

experiments with sensations comprised the last part of his career and were intended to provide measures of sensation intensities. As direct measurement was not possible, Fechner proceeded to compare sensations by means of inventive experimental methods leading to the determination of limit values or thresholds³:

“every stimulus as well as every stimulus difference must already have reached a certain finite magnitude before it can be noticed at all [...]. The point at which a stimulus or a stimulus difference becomes noticeable or disappears will be called a threshold, for short. This expression can be applied equally well to the sensation or difference between sensations that can just be noticed as well as to the stimulus, stimulus difference or stimulus ratio that is the cause of the sensations at this point.” (Fechner, 1860, p. 199)

Between the “threshold of consciousness” (i.e. the minimum perceivable stimulation) and the “upper limit of sensibility”, the correlation between the intensity of the stimulus and that of the corresponding sensation was found to be regular:

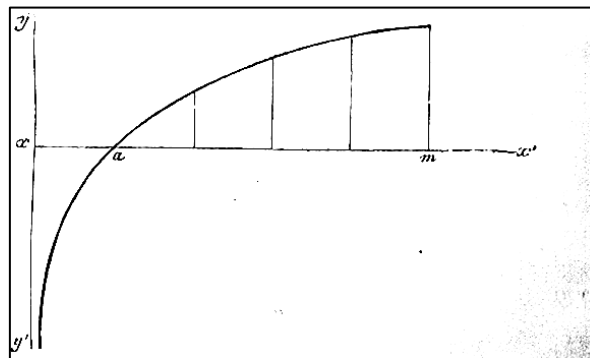
The “formulation stating that the magnitude of the stimulus increment must increase in precise proportion to the stimulus already present, in order to bring about an equal increase in sensation, was first made with some generality by E. H. Weber and supported by his experiments. I have therefore called it Weber’s Law [...]. The mathematical function, on the other hand, that relates stimulus intensity and sensation magnitude had been applied more than a hundred years ago by Euler, and later repeatedly by Herbart and Frobisch [...] somewhat before Euler by Daniel Bernoulli, and later by Laplace and Poisson [...]. If the generality and meaning of this law and this function had been understood earlier, this psychic measure would have been recognized sooner.” (*ibid.*, p. 54)⁴

³ Psychophysical experimentation faced the important weakness of being unable to measure sensations directly, and was focused instead on the study of “the stimuli or stimulus differences that produce equal sensations or equal differences between sensations” (Fechner, 1860, p. 46). Fechner founded a “metric system” that was based on the multiplication of equal units of “just noticeable differences” (*ibid.*, p. 47): “we determine the magnitude of a sensation, which we cannot do directly, by asking how many times it contains the same unit, an operation that we are able to perform directly” (*ibid.*, p. 51). In order to achieve this system, Fechner advanced three different methods for measuring sensitivity: the method of just noticeable differences, the method of right and wrong cases and the method of average error (*ibid.*, p. 60).

⁴ Fechner’s Law (also known as the Weber-Fechner law or the fundamental law of psychophysics) refers to the combination of Weber’s Law and the mathematical relation (Bernoulli’s). The term “Weber’s Law,” refers usually to “Weber’s simple statement that the just noticeable difference in a stimulus bears a constant ratio to the stimulus” (Boring, 1966, p. xiv; Schneider, 1975).

Figure 4.1 shows the mathematical expression of Fechner's Law as presented in Wundt's *Principles of Physiological Psychology* (1874). According to the mathematical equation $E = C \ln(R)$, with $C =$ constant value, sensation intensity (E) was a logarithmic function of stimulation intensity (R). The curve in the figure shows the values of E corresponding to arithmetical growths of R . " α " represents the "threshold of consciousness", and " m " the "upper limit of sensibility".

Figure 4.1 (Wundt, 1874, p. 406)



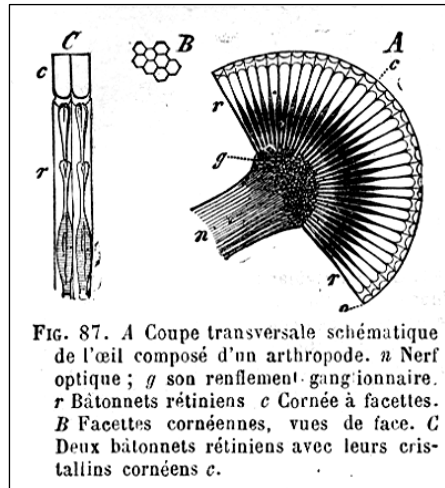
(2) It is important to point out that that Fechner's psychophysics was not experimental psychology, but rather sensation analysis⁵. Physiological psychology was founded by Wilhelm Wundt by combining physiological studies with sensation analysis and association laws (Boring, 1950; Introduction). The first part of Wundt's *Principles* was devoted to the analysis of "The Bodily Substrate of the Mental Life" (pp. 21-304) and dealt with human and comparative nerve physiology⁶. The second part named "On Sensations" (pp. 305-565) was divided in four chapters. The first of these: "Origins and General Properties of Sensations" (pp. 305-362) dealt with

⁵ Fechner's methodology was, however, essential for the development of psychology. In Boring's words: if Fechner founded experimental psychology, "he did it incidentally and involuntarily, and yet it is hard to see how the new psychology could have advanced as it did without an *Elemente der Psychophysik* in 1860" (Boring, 1966, p. xvii).

⁶ This section is based on the first volume (parts 1 and 2) of the second edition of Wundt's *Principles* (French translation by É. Rouvier, 1886). The volume advanced the physiological and psychophysical foundations of Wundt's theory. The second volume (parts 3 to 6) dealt with more complex psychic processes, and was more closely related to association psychology. It analyzed the association of the simplest possible elements into mental processes, the interconnexions of mental processes themselves, as well as different principles of mental development.

sensory physiology and was supported by detailed illustrations as in the following eye section scheme.

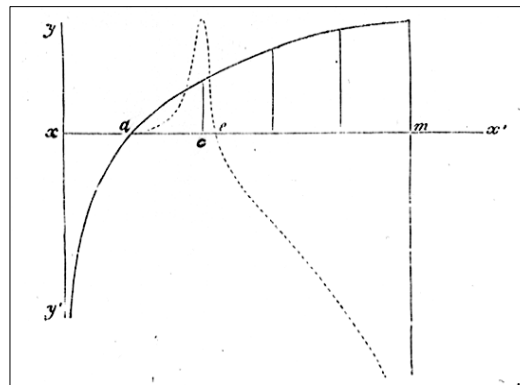
Figure 4.2 (Wundt, 1874, p. 324)



The remaining chapters dealt with Wundt's three properties of sensations: "Sensation Intensity" (pp. 363-413), "Sensation Quality" (pp. 414-524), and "Feeling" (*Gefühlston*) (pp. 525-565). Feeling analysis dealt with the pleasures or pains which were supposed to depend on the intensity (not the quality) of sensations. Wundt showed that there were no "absolutely pleasant or painful" qualities for sensations and that in the case of every single quality, feeling was "a function of intensity" (Wundt, 1874, p. 532)⁷. Wundt thought of feeling as a continuous variable which, as in the case of sensation intensity, was understood as a function of the intensity of the stimulations. Figure 4.3 shows Wundt's representation of both the intensity and the feeling of a sensation (hereafter Wundt's curve). Points "c" and "p" (appearing between the threshold points "α" and "m") correspond respectively to the "maximum pleasure" and the "indifference" points.

⁷ According to Wundt (1874, p. 305) sensations were the simplest possible states of consciousness. Of the three properties of sensations already mentioned, this narrative deals just with two: intensity (Figure 4.1) and feeling (Figure 4.3). Wundt's analysis of quality was divided in four parts: (1) "general sensations" (pressure, temperature and movement) (pp. 414-432), (2) "sensations of taste and smell" (pp. 432-437), (3) "sensations of hearing" (pp. 437-463), and (4) "visual sensations" (pp. 463-525).

Figure 4.3 (Wundt, 1874, p. 529)



As shown in the figure, for increasing stimulation intensities, the feeling of a sensation was supposed to increase in pleasure, attain a maximum pleasure value, and decrease, passing through an indifference point, to reach negative values (pains). However, even if according to Wundt's curve pleasure feelings were associated to moderate stimulus intensities, Wundt found that the curve was not stable in time. Dynamic studies revealed that persistent stimulations did not give regular values of feeling but oscillating values which were essentially a function of duration⁸:

“sensation duration essentially determines the corresponding feeling [...]. That is why the feeling never stays for a long time at a constant value but oscillates between its two opposites once stimulation is maintained. If a pain persists for a long time it comes closer to the indifference point, [...], and a pleasure feeling can finally become a pain as sensibility increases with the stimulation persistence.” (*ibid.*, pp. 547-548)

Wundt's oscillation principle led to feeling accounts which went beyond the application of direct analogies based on Fechner's Law (taking pleasures and pains as sensations). As we shall see in detail (Sections 4.5 to 4.8), Wundt's principle led to adaptation theories, some elements of which ended up making their way into economics, but that, only very recently.

⁸ As noted above, this section is based on the second edition of Wundt's *Principles* which makes part of Wundt's early research. According to Boring, at this stage of the elaboration of Wundt's *Principles*, feeling “was but an attribute of sensation” (Boring, 1950, p. 329). During the final period of Wundt's life (after the turn of the century), feeling analysis took an increasingly important part in Wundt's *Principles* (5th and 6th editions) replacing the “older parsimonious theory of pleasantness-unpleasantness” (Boring, 1950, p. 331) by a more general one.

4.3. Economic analogies out of the “fundamental law”

The late 19th century saw the widespread application of psychophysical insights to the analysis of human and social subjects. Diverse phenomena were reframed in order to fit stimulus/sensation representations and explained by psychophysical analogies which were supposed to give natural foundations to the human sciences:

“One can hardly imagine the fad this perspective triggered among the scientists of that time, physiologists and psychologists first, and social scientists next, who were all looking for scientific guarantees. There were many who, in one or another way, tempted to bring their subject matter back to an analogous mathematical relation to the one discovered by Fechner between physical stimuli and sensations, and to ensure in this way the scientific status of their practices.” (Feuerhahn, 2005, p. 783)

This fad approached economics as psychophysical insights were applied to the understanding of economic phenomena by Fechner and Wundt themselves (1), and also by Jevons, Edgeworth and some of their followers (2).

(1) Economic analogies were already present Fechner’s *Elements of Psychophysics* (1860). Fechner’s chapter on “Weber’s Law” consisted in extending Weber’s findings to the analysis of different phenomena. It related Weber’s findings to increasingly “broader” types of stimulations starting by light (pp. 116-146), and passing then to sound (pp. 146-152), weights (pp. 152-167), temperature (pp. 167-175), extensive magnitudes (visual and tactual measures of size) (pp. 176-197), and physical and mental wealth (pp. 197-198). The last two pages of the chapter extended the use of Weber’s law to economic phenomena by taking physical possessions as stimulations and psychic values as sensations:

“our physical possessions (*fortune physique*) have no value or meaning to us as inert material, but constitute only a means for arousing within us a sum of psychic values (*fortune morale*). In this respect they take the place of stimuli. A dollar has, in this connection, much less value to a rich man than to a poor man. It can make a beggar happy for a whole day, but it is not even noticed when added to the fortune of a millionaire. Weber’s Law can handle this kind of situation. If the same amount is to be added to what Laplace called the *fortune morale*, the addition to the *fortune physique* must be in proportion to the physical possessions already there.” (Fechner, 1860, p. 197)

Wilhelm Wundt applied the analogy to the analysis of wealth and happiness in the following way:

“The elevation of happiness, in its relation to the growth of the goods of fortune, follows Weber’s law within certain limits since the possessor of 100 thalers is as happy of the addition of one thaler than the possessor of 1000 thalers is of one of 10 thalers.” (Wundt, 1874, p. 530)

As one may expect, however, Wundt’s analysis of happiness went beyond the raw application of the analogy. It made reference to feeling analysis and the oscillation principle:

“the better placed individual to appreciate small happiness oscillations, is the one whose happiness is simply proportional to the growth of external goods. Under this limit, the absolute value of the fortune goods is small; above this limit, the oscillations of their values, as observed in ordinary circumstances, are too insignificant, in their relative magnitude, to give a sufficient satisfaction to the individual. This is confirmed by the experience of every century, for ‘the aurea mediocritas’ offers the most favorable conditions for the happiness feeling.” (*ibid.*, p. 530)

It is important to underline that Fechner’s and Wundt’s analogies were not new ways of approaching the analysis of human subjects but rather two cases in a much broader move which consisted in applying mathematical relations (namely Bernoulli’s) to the analysis of human subjects⁹.

(2) In economics, the most famous analogies came from W. S. Jevons (1871) and F. Y. Edgeworth (1877, 1881). Jevons’ analogies were not directly influenced by the findings of Weber, Fechner, or Wundt (Chaigneau, 2002; Maas, 2005) but were more closely related to the British “physiological theories of William Carpenter, Henry Maudsley, and Thomas Laycock” (Maas, 2005, p. 154). He applied these theories to the analysis of economic

⁹ More than applying Fechner’s Law to the analysis of broader subjects, the move consisted in applying D. Bernoulli’s (1738) interpretation of the Saint-Petersburg paradox (which was a function relating mathematical v/s moral expected values) in different ways (this is what Fechner did). Jevons’ reference was, indeed, Bernoulli (not Fechner or Wundt). In order to justify his use of arbitrary assumptions as hypothetical axioms, he showed that Bernoulli had also made “assumptions of an arbitrary kind, and was then able to obtain reasonable answers to many important questions”: “It is almost self-evident that the utility of money decreases as a person’s total wealth increases.” (Jevons, 1871, pp. 159-160). See also Wundt (1874, p. 558), and Feuerhahn (2005) for accounts on Bernoulli’s interpretation of the Saint-Petersburg paradox.

subjects mainly through Richard Jennings' *Natural Elements of Political Economy* (1855) whose psychological analogy for consumption was the following¹⁰:

“the degree of each sensation which is produced, is by no means commensurate with the quantity of the commodity applied to the senses. [...] the increments of sensation resulting from equal increments of the commodity are obviously less and less at each step, – each degree of sensation is less than the preceding degree.” (Jennings in Jevons, 1871, pp. 55-57)

Jevons' *Theory of Political Economy* (1871) was supposed to rely as much on physiology as it was intended to “reframe the ‘laws of human wants’ in terms of mechanics and hence to introduce the ordinary methods of the physical sciences” to economic analysis (Maas, 2005, p. 154). At first sight, one may think Jevons' approach was in tune with the naturalist (monistic) arguments held by psychophysicists (i.e. British physiological psychologists). However, this was not exactly the case. Unlike the psychophysicists' approach which was inductive and based on physiological observations, Jevons embraced a hypothetical deductive method based on the use of “simple inductions” as axioms from which he could “proceed to reason deductively with great confidence” (Jevons, 1871, p. 18). For Jevons, “the laws of supply and demand” could be deduced from simple physiological axioms (*ibid.*).

It is important to point out that Jevons was producing his own account of behavior rather than adhering to what experimental psychologists were doing. Jevons' “economic psychology” (the first in our history) was labeled “mechanics of self-interest” by the early institutionalists mentioned in Chapter 3. This mechanics was definitely closer to the (philosophical) psychology of J. S. Mill than to the psychophysicist's approach or Wundt's experimental psychology¹¹. Jevons' psychophysical insights were no more than axioms in a

¹⁰ Extensive literature is available on this subject. See Stigler (1950), Zafirovski (2001), Chaigneau (2002), Maas (2005), Bruni and Sugden (2007) and their references. See also Chaigneau (2002) and Maas (2005) for detailed accounts about Jevons' contributions to economics and other disciplines.

¹¹ According to Mass (2005), Jevons' position was similar to that of the British Victorian psychologists. The image of man implied in psychophysiology, Maas writes, “was difficult to reconcile with the notorious issue of free will that proved to be the recurring fishhook in mind-matter debates throughout the Victorian Age” (Maas, 2005, p. 155). Philosopher-psychologists (mainly associationists) such as J. S. Mill, stipulated that “freedom of the will was a capacity of human nature of which introspection gave immediate evidence” (*ibid.*, p. 159). Because of his “reliance on introspection” Mill could maintain both “freedom and causation” (*ibid.*), without trying or even seeing natural experiments (*ibid.*, p. 173).

theory of mechanics which dealt with purposive behavior and pleasure maximization¹².

F. Y. Edgeworth's approach was similar to Jevons'. Both the *New and Old Methods of Ethics* (1877) and *Mathematical Psychics* (1881) made explicit reference to Fechner's Law and were intended to produce mathematical representations of pleasures and pains¹³. In the first of these two volumes, Edgeworth analyzed pleasure feelings through "an analogous reasoning to the one used in the case of simple sensations" (Edgeworth, 1877, quoted in Chaigneau, 2002, p. 28). He replaced the stimulus by "material means (for example wealth)", and took "pleasure as a sensation" (*ibid.*). However, as noted above, physiological psychologists (especially Wundt) did not use Fechner's Law as an approach to the analysis of feelings of pleasure or pain, but to the analysis of the intensity of sensations. Edgeworth was clearly aware of this distinction (curves in Figure 4.3) for he explicitly mentioned "Wundt's curve of pleasure and pain" (Edgeworth, 1881, p. 65). He merged however Wundt's curve and Fechner's Law in a single axiom of "diminishing marginal pleasure":

"All the formulae suggested for the relation between quantity of stimulus and intensity of sensation agree in possessing the property under consideration [...]; whether in the general case by analogy from the Fechnerian experiments on the senses or by a more *à priori* 'law of relation' in the sense of Wundt. [...] not only is the function connecting means and pleasure such that increase of means does not produce a proportionate increase of pleasure; but this effect is heightened by the function itself so varying (on repetition of the conditions of pleasure) that the same means produce less pleasure." (*ibid.*, p. 62)¹⁴

¹² "to maximize pleasure, is the problem of Economics [...]. By a *commodity* we shall understand any object, substance, action, or service, which can afford pleasure or ward off pain [...] *utility* [denotes] the abstract quality whereby an object serves our purposes, and becomes entitled to rank as a commodity." (Jevons, 1871, pp. 37-38)

¹³ In *Mathematical Psychics*, Edgeworth suggested that Bain's "moral arithmetics" were "perhaps to be supplemented by a moral differential calculus, the Fechnerian method applied to pleasures in general" (Edgeworth, 1881, p. 60). The first principle of this method, he wrote, "might be: Just-perceivable increments of pleasure of all persons, for all pleasures, are equateable" (*ibid.*). See Chaigneau (2002) for a detailed account on the influence of psychophysics in Edgeworth's writings

¹⁴ Bruni and Sugden (2007) deal with this subject by directly relating Edgeworth's work to that of behavioral economists. For a present-day reader, they write, "Edgeworth's discussion of the determinants of pleasure has a particularly interesting feature [for] Edgeworth proposes *two* distinct mechanisms of diminishing sensitivity to stimuli" (Bruni and Sugden, 2007, p. 152). The second mechanism consisted in taking into account the fact that "the more a given pleasurable experience is repeated, the less pleasure it gives". Bruni and Sugden refer to this as "the *law of accommodation*" which they relate to both behavioral economics and the

As Jevons, Edgeworth borrowed only parts of physiological psychology which he merged in an axiom out of which he developed a *Mathematical Psychics*. This psychological account proceeded by borrowing “‘ready-made’ laws from other sciences” (Maas, 2005, p. 286) which were used as “heuristic instruments to generate hypothesis” (*ibid.*). It is important to point out that both Jevons and Edgeworth (and some of their followers) produced theories which they thought of as parts (or “branches”) of psychology. These were economic psychologies in J. M. Clark’s terms opening this part of the dissertation (Clark, 1918, p. 4).

4.4. Two reactions: Weber (1908), Mitchell (1910)

The psychologies of Jevons, Edgeworth and their followers led quite immediately to two types of reactions: (1) the dualist endorsed by Max Weber (1908), and (2) the naturalist/monist endorsed by the early institutionalists. While Weber thought economists should develop their own approach by isolating just part of the motives of economic behavior (an “ideal-typist” approach), the institutionalists defended the naturalist view of behavior promoted by psychologists such as William McDougall.

(1) Max Weber’s “Marginal Utility Theory and the Fundamental Law of Psychophysics” (1908) was a strong reaction against the idea of taking Fechner’s Law as a foundation for utility theory¹⁵. According to Weber, it was not possible to consider the economic account of behavior as an application of psychophysics, for the two approaches were opposed in their very essence.

economics of happiness: “The psychological concept of accommodation used by Edgeworth is essentially the same as *adaptation*, which present-day psychologists have used to explain reference-dependence. Adaptation is also one of the central ideas in the now rapidly growing literature on the economic determinants of happiness” (Bruni and Sugden, 2007, p. 152).

¹⁵ Max Weber’s paper was a reaction to Lujjo Brentano’s *Development of Value Theory* (1908). Brentano was not a marginalist but an historian of economics who (as many others) related marginal utility theory to “the so-called Weber-Fechner law” (Weber, 1908, p. 25). Weber’s critical analysis was introduced in the following terms: “If an individual who possesses a thousand marks experiences a sensation of increased “happiness” of a certain intensity upon obtaining a hundred-mark increase of what he possesses, then be it noted, *this same individual*, if he should possess a million marks, would experience an increase of a hundred thousand marks with the *same* intensity of feeling of happiness [...] does all this bear on the questions which economic theory seeks to answer?” (Weber, 1908, p. 27)

Weber endorsed a means/ends definition of economics and thought that rather than relating stimuli to sensations, the marginalist enterprise should be devoted to the analysis of the relations between “needs”, “means of satisfaction” and “satisfaction of needs”. The economic problem was thus the reverse of the problem of physiological psychologists:

“while the fundamental law of psychophysics instructs us about how an *external* stimulus evokes psychic conditions (‘sensations’), economics, rather, is concerned with the fact that in virtue of such ‘psychic’ conditions a specifically oriented *external* behavior (action) is evoked. This external conduct then, to be sure, has its proper return effect on the ‘need’ out of which it arose, as it obviates or at any rate seeks to obviate this need through ‘satisfaction’ [...] we have here a most complex and far from unambiguous process, that could in any event only quite exceptionally be equated with a simple ‘sensation’ in the psychological sense.” (Weber, 1908, pp. 27-28)

As the problems to which psychophysics and economics were directed were opposite rather than similar, Weber thought the methods appropriate for the solution of the economic problems were “*not* within the range of applied psychophysics or psychology”. These disciplines, he thought, have “nothing to do with such a solution” (*ibid.*, p. 28). According to Weber, just three conditions were necessary to justify the approach of marginal utility theory: (i) that men were “*among other things*, motivated also by ‘needs’”, (ii) that with increasing consumption of commodities and labor output, need satisfaction was attained as “*other*, ‘unsatisfied’ needs appear[ed] to be more urgent”, and (iii) that men were able to act “expediently” in the light of “experience” and of “prior calculation” (*ibid.*). As these conditions bore no relation to the Weber-Fechner law, Weber claimed that there was no reason for economics to be based on psychophysics¹⁶. He thought that “the most general hypotheses and assumptions of the ‘natural sciences’” were at the same time “the most irrelevant ones” for economics (*ibid.*), and that economists had no reason to envy the empirical foundations of any science:

“The ‘everyday experience’ from which our theory takes its departure is of course the common point of departure of *all* particular empirical disciplines. Each of them aspires beyond everyday experience and must so aspire, for thereon rests its right to existence as a

¹⁶ “Every ‘psychologist’ must surely turn up his nose at such ‘everyday experiences’ as a foundation for a scientific theory. Take the very concept of ‘need.’ What a crude category of ‘vulgar psychology’! What vastly different physiological and psychological causal chains ‘need’ can start up!” (Weber, 1908, p. 29)

‘science.’ But each of them in its aspiration ‘goes beyond’ or ‘sublimates’ everyday experience in a different way and in a different direction. Marginal utility theory and economic ‘theory’ generally do this not, say, in the manner and with the orientation of psychology but rather pretty much in opposite ways. Economic theory does not, one may say, break down *internal* experimental correlates of everyday experience into psychical or psychophysical ‘elements’ (‘stimuli,’ ‘sensations,’ ‘reactions,’ ‘automatisms,’ ‘feelings,’ and so on). Instead, it seeks to ‘understand’ certain ‘adaptations’ of man’s *external* behavior to conditions of existence of a quite specific sort that are *outside* man himself.” (Weber, 1908, p. 31)

Weber thought economics was not (and should not be) based on any natural law. It was rather a theoretical construction supported by the “heuristic assumption” that economic action ran “its course on strictly ‘rational’ terms” (*ibid.*):

“Marginal utility theory, in order to attain specific objects of knowledge, treats human action as if it ran its course from beginning to end under the control of *commercial calculation* [...]. For its purposes, marginal utility theory treats the ‘psyche’ of all men (conceived of as isolated entities and regardless of whether they are involved in buying and selling) as *merchant’s soul*, which can assess quantitatively the ‘intensity’ of its needs as well as the available means of their satisfaction. It is in this way that the theory attains to its theoretical constructions. But all this is certainly *opposite* to the procedure of any ‘psychology’!” (Weber, 1908, pp. 31-32)

For Weber, the aim of economics was not to explain actual behavior, but to approach this behavior by supposing strictly “rational” actions. This was a valid approach, he thought, not because of its reliance on psychophysics or any other natural science, but because of its heuristic significance as a means for improving the understanding of particular cultural-historical facts as the development of business in the modern economies:

“As we all know, the assumption does not hold – and the empirical course of those proceedings for the understanding of which the theory was formulated accordingly shows only an ‘approximation’ (varying considerably according to the particular case) to the theoretically constructed course of strictly rational action [...] under today’s conditions of existence the approximation of reality to the theoretical propositions of economics has been a *constantly increasing one* [...]. The heuristic significance of marginal utility theory rests on this *cultural-historical* fact, but not on its supposed foundation in the Weber-Fechner law.” (Weber, 1908, p. 33)

Unlike psychology, Weber thought economics should proceed in its own methodological terms by advancing “series of *conceptually* constructed events” (*ibid.*, p. 34) or “ideal-types”. Even if these “ideal purities” were seldom “to be found in the historical reality of any particular time” (*ibid.*), Weber thought “economic history” should be constructed out of a sum of these “heuristic instrumentalities of analysis” (*ibid.*). He thought the task of the social scientist was to combine these instruments in order to grasp the complexities of the “empirical manifold” (*ibid.*).

(2) The institutionalist reaction to the “mechanics of self-interest” (Mitchell, 1910, p. 109) was markedly different from Weber’s¹⁷. Institutionalists like Veblen (1909), Mitchell (1910), and Clark (1918), thought economists should not build their behavior accounts out of “a few principles of human nature” (Mitchell, 1910, p. 97). They rather thought economists should take their “psychology from those who [had] specialized in that field” (Clark, 1918, p. 4). These institutionalists thought the “cause and effect” relations explored by modern psychologists ran in the right direction (from past to present) (Veblen, 1909, p. 625). Unlike the “deductive”, “a priori” or “subjective” accounts of behavior, the “new” psychology had “an objective, impersonal, materialistic character and force” (*ibid.*)¹⁸. As noted in the previous Chapter (Section 3.3), this psychology was instinct-based and functional. Based on William McDougall’s *Introduction to Social Psychology* (1909), Mitchell (1910) thought of the new approach as a “positive science of conduct” which would become “an evolutionary natural history of mind” (Mitchell, 1910, p. 100).

¹⁷ Like Weber’s, however, this second reaction was also directed to the inappropriateness of psychophysics as a foundation for economic theory. Snow’s (1924) paper against Z. C. Dickinson’s purpose of relating marginalism to psychophysics is a good example of the institutionalist view of this problem. He reacted against Dickinson’s *Economic Motives* (1922) in the following terms: “What is the value of taking this unsubstantiated psychology to explain economic theory? Psychologists know that the Weber-Fechner law has never been proved conclusively or even approximately to apply to the feelings. They know also that there is a fundamental difference between the sensations and feelings. Sensations are definitely localized; they submit readily to being picked out and observed; they can be made an object of direct attention. Feelings, however, fade away when attended to; they are general and they cannot be localized” (Snow, 1924, p. 496).

¹⁸ For Veblen, the marginal-utility school shared the postulates and the logical method of “the English classical economists of the nineteenth century” (Veblen, 1909, p. 622). From his viewpoint, both “the classical school in general and its specialized variant, the marginal-utility school, in particular”, took “the traditional psychology of the early nineteenth century hedonists as their common point of departure” (*ibid.*). This psychology was “accepted as a matter of course or of common notoriety” and held “quite uncritically” (*ibid.*).

Unlike the mechanics of self-interest (and Weber's conception of utility theory), the psychology of reference for the institutionalists considered the analysis of human phenomena in all their complexity:

“From Mr. McDougall's standpoint the simple psychological premises of this mechanical type of economics are wholly inadequate, if not radically mistaken. But such a verdict involves no refutation of writers like Jevons, Fisher and [J. B.] Clark. For the mechanics of self-interest, like its prototype, rational mechanics, does not profess to take into account complex reality [...]. While economists of that mental bent which is peculiarly sensitive to the claims of logical order and precision have been perfecting the mechanics of self interest, their colleagues of a realistic turn have sought to keep economic science in close touch with economic life. To men of the latter temperament, logical precision smacks more of scholasticism than of science when attained by sacrificing faithfulness to fact.” (*ibid.*, pp. 109-110)¹⁹

According to Mitchell, the mechanics of self-interest was, at best, an approach to half of the economic problem: the “art of making money” (Section 3.3). It had strictly nothing to say in regards to the arts of spending:

“the assumption of rationality fits *the activities of consumption* nowhere outside of economic treatises. Men, and more specially women, plan the spending of money upon personal satisfactions with far less attention than they give to their plans for the spending of money upon business ends. Passing whims, carelessness about prices, ignorance of qualities, obstinate preference for old ways are left wide scope. In McDougall's terms, habit, suggestibility, and the instincts of emulation and imitation must be brought in, if we are to account for our own subservience to fashion, our conspicuous waste, and our *slovenly dependence on the advertiser*. The assumption of rationality is inadequate to explain the facts.” (*ibid.*, p. 200, emphasis added)

For this and other reasons, economists like Mitchell were against the specialization suggested by Weber. They thought economists were reserving too narrow a subject for themselves. Such a field, they thought, did not “hold wide enough to require all the energy of one company of scientific workers” (*ibid.*, p. 202):

“the attempt to limit economic theory to ‘an analytical study of the motives which govern men in business and industrial life,’ and to avoid bothering about ‘what the primitive man

¹⁹ “The reason why Mr. McDougall is so confident of the indispensability of psychological knowledge for all students of the social sciences is that he takes the evolutionary type of social science for granted as the only type in line with the trend of scientific thought.” (Mitchell, 1910, p. 113)

may or may not have done,' has positive as well as negative disadvantages. It not only leaves untreated those evolutionary problems which loom so large in the perspective of modern science; but it also prevents the theorist from comprehending the true character of the limited range of phenomena with which he professes to deal." (*ibid.*, p. 205)

Mitchell thought mainstream economists ignored the fact that the behavior they reduced to hedonic (or mechanical) accounts was "largely the product of the money economy" (*ibid.*, p. 208). This was an historical fact which could only be grasped "from the evolutionary view-point, in the light of functional psychology" (*ibid.*)²⁰. The mechanics of self-interest was among the "ill results of separating the task of framing a theory of the current economic regime from the task of explaining how this regime ha[d] come to be" (*ibid.*, p. 215).

The institutionalists' aim was to develop economics by embracing the scientific method of functional psychologists. Embracing this opportunity, Mitchell wrote:

"economics will assume a new character. It will cease to be a system of pecuniary logic, a mechanical study of static equilibria under non-existent conditions, and become a science of human behavior." (Mitchell, 1914, p. 47)

4.5. Consistent rather than adaptive behavior

As shown in Chapter 3, economics did not become the "science of human behavior" the institutionalists expected and the analysis of the complex phenomena of adaptation, habituation and learning was kept away from the mainstream of the discipline with the rejection of both functional psychology

²⁰ The history of the "money economy" was one of the main institutionalist subjects. During "the long time that men have been gaining their mastery over the use of money" they thought, "pecuniary concepts have been gaining a subtler mastery over men" (Mitchell, 1910, p. 209). As a consequence, any attempt "to explain the present as an accomplished fact without constant reference to the process of evolution" would lead to "artificial, superficial, and incomplete" theories (*ibid.*). These theories, the institutionalists claimed, led to oversimplified accounts which were disguised under the universalistic concepts of hedonic psychology: "Substitute pleasure for profit and pain for loss, let the unit of sensation stand for the dollar, replace accounting by hedonistic calculus, interpret self-interest as the maximizing of net pleasures instead of net profits and the transformation is complete. The creature of hedonic psychology, like the creature of the money economy, has substantially no instincts, no emotions, or habits, which are not embodied in the pursuit of pleasure along the road of calculation" (*ibid.*, p. 213).

and behaviorism. It must be noted, however, that the institutionalists' attempts were not the only attempts to bring the analysis of adaptation and learning into economics. Two other episodes in this history are worth mentioning before entering the analysis of Adaptation-Level Theory: (1) the attempts by economists to introduce dynamic considerations into general equilibrium theory, and (2) the ambiguous reaction to information-processing analysis by mainstream economists²¹.

(1) According to Nicola Giocoli's *Modeling Rational Agents* (2003), there were a few mainstream attempts to introduce dynamic considerations into general equilibrium theory during the interwar period. These attempts consisted in reformulating and extending "the notion of economic equilibrium to a multi-period, multi-agent setup, and of the ensuing acknowledgment that such a reformulation and extension called for an analysis of the way economic agents formulated and revised their plans" (Giocoli, 2003, p. 135). Such an analysis involved the "explicit appraisal of the out-of-equilibrium functioning of the economic system" (*ibid.*), and the theoretical efforts of the economists involved in this program were directed to the analysis of the "assumptions on the agent's foresight, the stability of the economic system and above all, the question of *learning*" (*ibid.*, original emphasis).

In order to develop dynamic theories, it was necessary to abandon the perfect foresight assumption. In Giocoli's terms, it was necessary to "Escape from Perfect Foresight" (*ibid.*, pp. 135-199):

"every economist involved in the program recognized that the precondition for dynamic theory, as well as for a meaningful investigation of expectations and learning, was the abandonment of the assumption of perfect foresight." (*ibid.*, p. 136)

Giocoli (2003) claims that this "escape" was animated during the 1920s and 1930s by E. Lindahl, G. Myrdal, O. Morgenstern, T. Hutchinson, and J. Hicks

²¹ This section presents just an overview of these two episodes in order to show that there is a history of ambiguous relations between economics and psychology rather than just a few isolated events. To every psychology presented in this part of the dissertation: (1) physiological psychology, (2) behaviorism, (3) information-processing and (4) adaptation theory, there has been an economic counterpart, however obscure the relation between these approaches may be. This Section is based in Nicola Giocoli's *Modeling Rational Agents* (2003) for the first part and Paul Edwards' *Closed World* (1996) and Philip Mirowski's *Machine Dreams* (2002) for the second. The reader should turn to this literature in order to explore the subject in detail.

and consisted in modeling the “out-of equilibrium working of the system” in terms of “the revision of the expectations and plans of agents endowed with only limited information and forecasting abilities” (*ibid.*, p. 144). It was an attempt to elude the clauses of “unlimited forecasting and calculating abilities” which were essential “consistency conditions” in the Austrian economic models (especially of von Mises and Hayek) which were explicitly deprived of “empirical content” (they were close to Max Weber’s conception of economic theory as presented in Section 4.4). Giocoli shows that the escape failed because there was “no room for learning” in economic theory (*ibid.*, p. 185). The attempts to introduce disequilibrium and learning issues clashed with both the “consistency view of rationality” and the “purely formal characterization of the agent that were forcefully rising from contemporary choice theory” (*ibid.*, p. 137). What remained from the attempt to introduce dynamics into general equilibrium theory were just “the reduced version of dynamics developed in Samuelson’s *Foundations*”, and “the general equilibrium model of Arrow and Debreu, which did embrace a multi-period approach but compressed all the dynamics into a time-zero, instantaneous equilibrating process” (*ibid.*).

(2) Philip Mirowski (2002) shows that something similar to Giocoli’s “escape from perfect foresight” happened with the incursion of mainstream economists to information-processing studies. This episode took place during and after World War II and also ended with the “Arrow-Debreu [1954] formalization of Walrasian general equilibrium” (Mirowski, 2002, p. 270). Information-processing studies and digital computing (the “cyborg” sciences in Mirowski’s terms) were developed by scholars of which John von Neumann [1903-1957], Alan Turing [1912-1954] and Herbert Simon [1916-2001] were the most prominent figures (P. Edwards, 1996; Mirowski, 2002). Their programs were extended to the analysis of human subjects and applied to the study of thinking, learning and problem solving, as well as to the analysis of adaptive behavior and evolution theory²². Partly because of the

²² The best example of the latter was the mathematical analysis of automata developed by John von Neumann (see Mirowski, 2002, pp. 94-152). For people involved in the development of computer sciences, an automata was an “information-processing mechanism that exhibited self-regulation in interaction with the environment, and therefore resembled the structure and operations of a computer” (Mirowski, 2002, p. 141). Research on automata implied the analysis of “logical prerequisites for information processors to be capable of creation of successors logically more complicated than themselves” (*ibid.*), and dealt with

limited character of information-processing, which depended on the storage capacity of the computing machines, the approach was useful for psychologists as Herbert Simon who were able to simulate human phenomena with programs that were “set up to analyze how people, tasks, and networks” were “interrelated in complex, dynamic, and adaptive systems” (Sent, 2004, p. 740). According to A. Newell, J. C. Shaw and H. Simon’s “Elements of a Theory of Human Problem Solving” (1958), the importance of the digital computer for the theory of higher mental processes lied in providing them with “a much profounder” idea than they previously had of the characteristics a mechanism should possess in order to “carry out complex information-processing tasks” (Newell *et al.*, 1958, p. 163). The digital computer, they wrote, was a “device capable of realizing programs, and hence, of actually determining what behavior [was] implied by a program under *various environmental conditions*” (*ibid.*, p. 165, emphasis added).

According to Mirowski’s “history of economics *from the cyborg point of view*” (Mirowski, 2002, p. 29), the impact of information-processing studies on economics was ambiguous. While information-processing studies (namely von Neumann’s) consisted in the analysis of “what could in principle be subject to computation” (*ibid.*, p. 21), the main economic attempts to introduce such an approach were based on “market computation” which was supposed to be perfect as it was the counterpart of the perfect foresight assumptions (Mirowski, 2002; Giocoli, 2003).

Mirowski claims that the mainstream economists’ reaction to von Neumann and Morgenstern’s *Theory of Games and Economic Behavior* (1944) consisted in rejecting the “cyborg vision of von Neumann” (Mirowski, 2002, p. 270, original emphasis), while keeping parts of the *Theory* which were not intended by the authors as “serious theory of cognitive information processing” (i.e. the “von Neumann-Morgenstern expected utility”) (*ibid.*, p. 280). In the end, the economists’ “version of the economic agent as information processor” (Mirowski, 2002, p. 279) implied unlimited information-processing capacities, an issue which was completely at odds with the information-processing approach, but suitable to both “the walrasian model” and the version of equilibrium proposed by John Nash [1928-], whose

“the conditions under which simple automata gave rise to increasingly complex automata” (*ibid.*). By developing this program, mathematicians such as John von Neumann were making their way to “a formalized logical theory of evolution” (Mirowski, 2002, p. 141).

vision of a game was incompatible with von Neumann's (Mirowski, 2002; Giocoli, 2003):

“Even though much of the mathematics was cribbed from inspirational texts by von Neumann, the flagrant disregard for any cognitive prudence or computational relevance could not be attributed to the cyborg inspiration as much as to the Walrasian heritage.” (Mirowski, 2002, p. 282)

Mirowski maintains that “the lesson derived by Arrow, Debreu, and Nash from Bourbaki was that questions of existence of equilibrium were really just demonstrations of the logical consistency of the model” (*ibid.*, p. 410). There was no space for learning or for adaptive behavior in these kinds of accounts²³.

4.6. From psychophysics to *Adaptation-Level Theory*

While adaptation and learning theories were failing to make their way into economics (Sections 4.2 to 4.5), different approaches to the analysis of these subjects were developed by psychologists working on psychophysical experimentation. Unlike the economists who applied Fechner's Law as an axiom out of which they developed their own psychological accounts (Section 4.3), these psychologists (who were not behaviorists) proceeded by testing and reformulating Fechner's law and by extending psychophysical experimentation to the analysis of new phenomena (1). These extensions were developed during the 1950s and 1960s and synthesized in the form of an *Adaptation-Level Theory* by Harry Helson in 1964. Helson's theory was devoted not only to the analysis of perception, but also to that of affectivity, motivation, learning and interpersonal behavior (2).

²³ “the Nash program shares a fundamental attribute with its Walrasian cousin that it is generally non-computable; and there are few sins more cardinal in the cyborg creed than a mathematical procedure being noneffective [...]. The Nash equilibrium stands as the mathematical expression of the very essence of the [...] mentality of impervious rationality” (Mirowski, 2002, p. 340, 342).

(1) The origins of the Adaptation-Level (AL) theory can be traced back to Harry Helson's experimentations on visual sensations²⁴. Helson's early studies (1929-1938) held in the tradition of the psychophysical analysis of visual phenomena and were devoted to the analysis of color perception in general and "photopic adaptation" in particular (Helson and Judd, 1932). In 1938, Helson's "Fundamental Problems in Color Vision" introduced the concept of "adaptation reflectance" to explain the relations between the visual phenomena of "contrast, constancy and adaptation", which were previously object of separate studies (Helson, 1938, p. 439). Helson's contribution to the "psychology of color" consisted in formulating a "general principle governing constancy, contrast, conversion and adaptation" (*ibid.*, p. 449)²⁵:

"The failure to recognize that a single principle operates in every act of vision has been responsible for the many conflicting views regarding the relation of contrast to constancy, of adaptation to constancy and of constancy to conversion, *etc.* Our principle makes but one assumption, fully borne out by the facts, *viz.*, in every act of vision provided the eye is subjected to a unitary illumination there is established an adaptation reflectance or

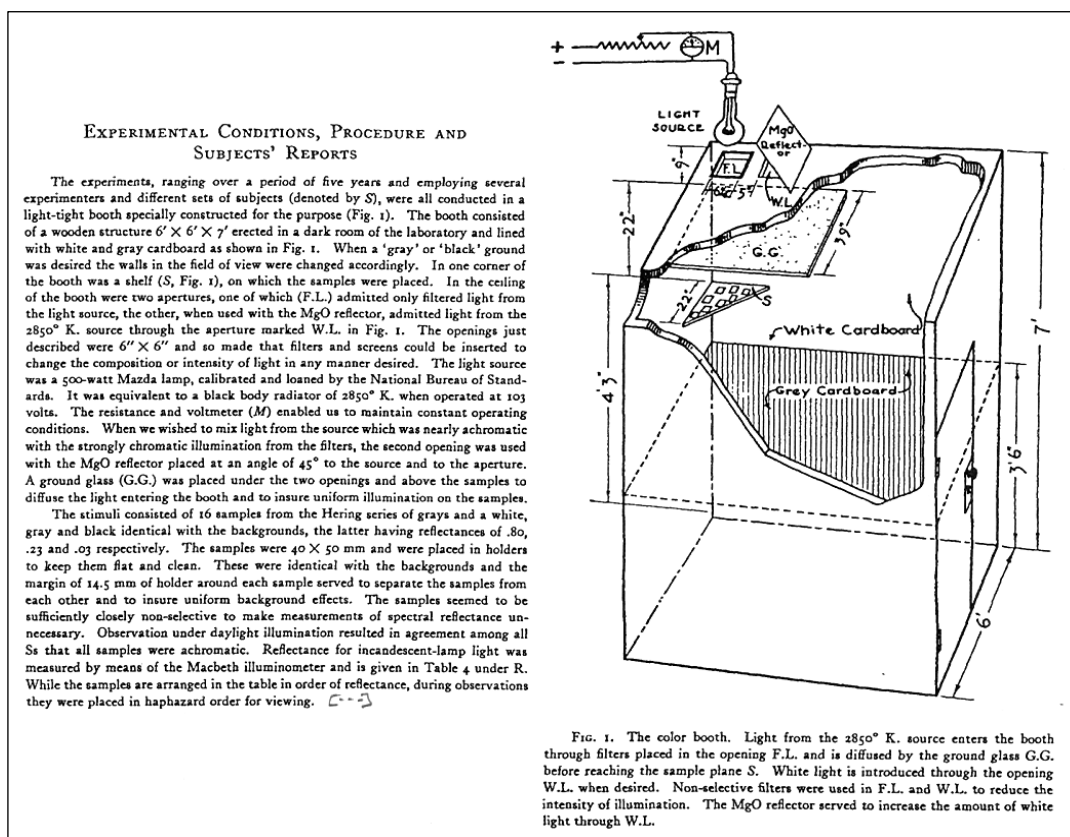
²⁴ Harry Helson [1898-1977] started a doctorate in philosophy at Harvard (1924) but shifted to psychology after his "intellectual encounter with E. G. Boring as a second-year graduate student" (Bevan, 1979, p. 153). Afterwards (1925), he moved to Cornell (Titchener's department), to the University of Kansas in 1926, and to Bryn Mawr College in 1928 where he stayed 21 years. Later in his life he moved to the University of Texas in 1951, to Kansas State University in 1961, to York University (Toronto) in 1968 and finally to the University of Massachusetts from which he retired in 1971 (Bevan, 1979). This year, a symposium was held in order to honor his contributions (*Adaptation-Level Theory: a Symposium*, edited by M. H. Appley). According to Bevan's account: three of Helson's "contributions are truly monumental. The earliest, a critical review of the Gestalt movement, was his doctoral dissertation. Published in the *American Journal of Psychology* in 1925 and 1926, it introduced Gestalt psychology to the American psychological community and brought him immediate favorable recognition. The second was his discovery of the principle of color conversion. The outcome of a more than a decade-long series of experiments with various collaborators, it made possible our understanding of the perception of surface colors over a full range of ambient illuminants. These color experiments were the origin of his third and most significant contribution, his exposition of the general theory of adaptation level. A species of Gestalt theory that quantitatively takes into account not only the contribution of the present context to a wide range of behaviors but that of prior stimulation as well, it brought relativity to psychology in a most fundamental way." (Bevan, 1979, p. 154)

²⁵ It is worth noting that Helson's project of advancing a general theory of adaptation was partly influenced by his early work on Gestalt psychology (Helson, 1926, 1933 and references). In Judd's words: Helson's "thesis was on the psychology of Gestalt, and for me the important part of this psychology is summarized by the statement that all the parts of the environment combine to influence the responses that are observed, and that the form in which the parts are combined may be crucial. A corollary to this statement is that to analyze the environment into parts, each to be studied separately, is often foredoomed to failure because it may not be possible to synthesize the responses to the separate parts in order to predict the response to the total situation. Adaptation level (AL) conforms to the idea that the situation must be evaluated as a totality" (Judd, 1971, p. 305).

achromatic point such that there exists at least one sample whose reflectance is such that it will be seen as achromatic. This adaptation reflectance is determined chiefly by the background reflectance but is influenced by reflectances of the samples in the field [...]” (*ibid.*, pp. 469-470)

Helson explored this problem by means of experiments he performed in the “color booth” presented in the following figure. These studies showed that there was “a single mechanism underneath the varied phenomena of color vision in all conditions” (*ibid.*, p. 474)²⁶.

Figure 4.4 (Helson, 1938, pp. 443-444)



²⁶ This general principle related chromatic phenomena to an “adaptation reflectance” reference level (which was an achromatic level): “samples above the adaptation reflectance take the hue of the illuminating color; samples below it, the hue complementary to the illuminating hue; while samples near the adaptation reflectance are either achromatic or greatly reduced in saturation” (Helson, 1938, p. 449). The “all-important concept of adaptation reflectance” (*ibid.*, p. 453) introduced the idea that the achromatic point was not an absolute level (of the sample or the background), but a relative level which was “established with reference to the conditions obtained *within* the viewing situation itself and [was] a function of the weighted average reflectances of samples and background” (*ibid.*, p. 452).

A decade later (1947), Helson published “Adaptation-Level as Frame of Reference for Prediction of Psychophysical Data” where he extended the previous findings in the field of vision to “problems associated with shifts in scale-value when the comparison or anchoring stimulus [was] changed” (Helson, 1947, p. 1). This implied two things. First, the extension of Helson’s principle of “adaptation reflectance” to the more general principle of Adaptation Level (AL) that could be applied to the analysis of new kinds of psychophysical data (mainly weight lifting and sound intensities). Second, the extension of the scope of the theory from constant to variable stimulation experiences:

“The concept of adaptation level (AL) must not be restricted to the effects of prolonged adaptation to more or less constant stimulation with greatly reduced capacity for response as a final end-state. There is an AL for every moment of stimulation, changing in time and with varying conditions of stimulation. It is a function of *all* the stimuli acting upon the organism at any given moment as well as in the past. To arrive at a quantitative value for the AL it is necessary to weight all factors in the situation confronting the organism as well as factors within the organism. [...] a good first approximation to the AL in vision was obtained by taking a weighted logarithmic mean of all stimuli and background in the field times a constant.” (*ibid.*, p. 3, original emphasis)²⁷

(2) After its formulation in 1947, Helson’s theory was extended beyond the boundaries of psychophysical experimentation and applied to the analysis of affectivity, motivation, behavior and many other subjects. According to Appley (1971):

“Since its formulation by Helson in 1947, adaptation-level (AL) theory has been extended from its origin in psychophysics and perception to such varied fields as learning, cognitive processes, motivation and affectivity, transposition and the immediate size problem, stimulus generalization, personality, intelligence testing, social psychology, clinical psychology, and mathematical model building to name but a few.” (Appley, 1971, p. 1, references omitted)

²⁷ According to Corso (1971), AL theory was at this stage “a quantitative extension of the classical notion of perceptual relativity which recognize[d] a general factor (AL) encompassing the various components in the immediate stimulus configuration, the context, previous experience, etc. [...] the theory assert[ed] that AL [was] equal to the weighted *geometric* mean of all stimuli affecting the particular judgment.” (Corso, 1971, pp. 27-28)

It is worth noting that unlike the economists' use of psychophysics during the late 19th century (Section 4.3), the psychological analyses of Helson and his associates were always supported by experimentation²⁸. In 1964, Helson published the final (broadest) version of the AL theory. The book synthesized decades of research by different authors in different fields which were unified and presented as an *Experimental and Systematic Approach to Behavior*²⁹:

“The basic premise of this book is that an individual's attitudes, values, ways of structuring his experiences, judgments of physical, aesthetic, and symbolic objects, intellectual and emotional behavior, learning, and interpersonal relations all represent modes of adaptation to environmental and organismic forces. These forces do not act willy-nilly upon the organism from without, nor do they erupt spontaneously from within. Stimuli impinge upon organisms already adapted to what has gone before, and internal states depend upon previously existing internal conditions as well as external inciters to action [...]. The pooled effect of these [...] stimuli determines the adjustment or adaptation level underlying all forms of behavior.” (Helson, 1964, p. 37)

As noted above (Section 4.2), the history of AL theory may be traced back to Wundt's feeling analysis which was originally “used by psychologists to denote merely the decrement in sensory responses following prolonged exposure to stimulation” (Helson, 1971, p. 5). Throughout the history just mentioned, Helson extended this psychophysical analyses arriving to a general theory of behavior.

The AL theory of motivation made part of the general program advanced by Helson in 1964. It stressed the importance of affectivity (i.e. feelings and emotions) as a motivating factor leading to behavior:

“Feelings and emotions are potent inciters or inhibitors of action. Feeling states initiate, direct, and terminate almost all types of behavior and thus play a dominant role in individual modes of adjustment [...]. Learning and performance levels also depend to a

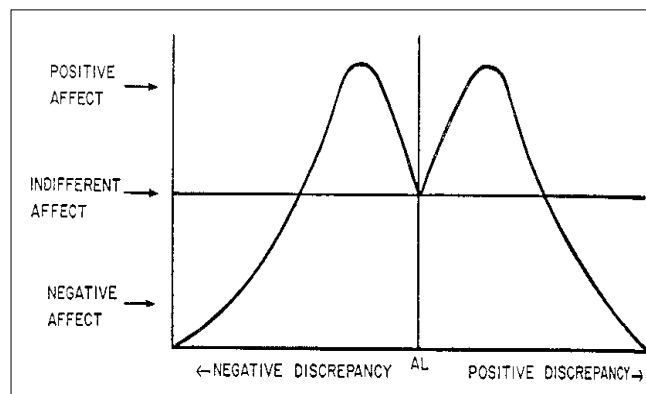
²⁸ Experimentation was also the source of Helson's and other's reformulations of Fechner's Law. According to Avant's contribution to the 1971 symposium: “There have been, since Fechner's law and his classical methods, four major developments in the field of psychophysics: Thurstone's law of comparative judgment; Stevens' power law; Swets, Tanner, and Birdsall's theory of signal detection; and Helson's adaptation-level theory” (Avant, 1971, p. 19). See also Michels and Helson's (1949) article on the reformulation of Fechner's Law.

²⁹ The outline of Helson's book was the following: “The Concept of Adaptation” (pp. 36-63), “Seven Basic Characteristics of Behavior” (pp. 64-124), “Psychophysical Judgment” (pp. 125-231), “Perception” (pp. 232-327), “Affectivity and Motivation” (pp. 328-390), “Learning and Performance” (pp. 391-452), “Cognition and Thinking” (pp. 453-520), “Personality” (pp. 521-582), and “Interpersonal Behavior” (pp. 583-660).

great extent upon affective states, and the role given to reinforcement in most learning theories attest the importance of affectivity as a motivating factor.” (Helson, 1964, p. 328)

The AL theory of affectivity and motivation was developed by psychologists McClelland and Clark in 1953 and incorporated to Helson’s general theory in 1964. This part of the theory (which was close to Wundt’s feeling analysis) assumed that positive affect was “the result of small discrepancies from sensory adaptation levels” and that negative affect was “the result of larger discrepancies” (*ibid.*, p. 382). Figure 4.5 corresponds to the McClelland-Clark representation of the theory. It consists in a double (mirror-like) representation of Wundt’s curve as presented in Figure 4.3, showing how affectivity depends on discrepancies from adaptation levels.

Figure 4.5 (Helson, 1964, p. 382)



According to this part of AL theory, stimuli “possess motivating power” if different enough from the AL (*ibid.*). It has been applied to the analysis of learning in general, as well as to the formation of expectations, levels of aspiration and other “internal norms”. Taking these norms as principles underlying motivational processes AL theory has been applied to analysis of behavior³⁰:

“The importance of internal norms in motivational processes can perhaps best be expressed in the dictum: There is no basic change in behavior without there being first a change in the internal norm underlying that behavior [...]. Investigation of the norms men

³⁰ The analysis of expectations was another application of this theory. Expectation levels were “considered to be adaptation levels” which were influenced by learning (*ibid.*). Both the analysis of rats running mazes and experimental studies on human behavior showed that “small departures from expectation” were pleasant, while “radical wholly unexpected departures [were] unpleasant” (*ibid.* p. 384).

live by, the manner in which they arise and develop, is one of the most important areas for psychological investigation.” (Helson, 1971, pp. 15-16)

A final important point about AL theory is that unlike the psychologies presented in Chapter 3, Helson’s program was not behaviorist as its main aim was the understanding rather than the control or prediction of behavior. Though it was certainly influenced by both behaviorism and Gestalt psychology, AL theory was far from having direct applications to the analysis of social phenomena. This was perceived as a deficiency of the theory by Helson and his associates. Part of the 1971 symposium held on the honor of AL theory was focused on this deficiency and consisted in applying Helson’s theory to the analysis of social subjects:

“At this very moment, we are living in a time of crisis, the causes of which are rooted in psychological mechanisms such as we have been studying for years in both human and subhuman subjects. We know that relative rather than absolute values and quantities determine reaction patterns. The presence of extreme contrasts has accentuated the dissatisfactions and disequilibria that are found in our society today [...]. The perception of these contrasts in modern media of communication, most of them visual, affects every individual who watches television, goes to the movies, or looks into the popular magazines. The facts learned about the role of anchors and much else concerned with internal norms carry directly over to our perception of social, political, and economic conditions. Using this knowledge rightly may show that psychology is more than a purely academic subject and that our experiments and theories can enable us to understand what is going on in the world about us as well as in the contrived world of the psychology laboratory.” (*ibid.*, p. 16)

4.7. Adaptation-Level Theory and the planning of the good society

Philip Brickman and Donald T. Campbell’s “Hedonic Relativism and Planning the Good Society” (1971), was a direct product of the AL theory symposium. It was inspired by discussions on the “political-economic-social” implications and applications of the theory that took place at its final session³¹. They were intended to transform “laboratory-derived principles into

³¹ According to Appley’s introduction to *Adaptation Level Theory: A Symposium* (1971), Brickman and Campbell’s contribution “was prepared after, rather than before, the symposium on AL theory. It [was] in a sense, the joint product of the indicated authors and all

possible real-world practices” (Appley, 1971, p. 285) (1). In 1978, P. Brickman, D. Coates and R. Janoff-Bulman (1978) published their empirical studies of subjective well-being data which corroborated the “relative happiness” principle predicted by AL theory (2). As noted above (Chapter 2), this principle has largely influenced the development of happiness studies.

(1) Brickman and Campbell’s contribution to the AL theory symposium was intended as an illustration of “how findings on hedonic adaptation level (AL) and level of aspiration (LA) should be taken into account in planning for happiness and the good society” (McClelland, 1971, p. 303). It maintained that happiness was subject to the “fundamental postulate of AL theory” according to which the subjective experience of “stimulus input” was “a function not of the absolute level of that input but of the discrepancy between the input and past levels” (Brickman and Campbell, 1971, p. 287):

“We may take it as a given that if a society in which people are happy is possible at all, it will be possible only if it incorporates an understanding of AL phenomena, an understanding which past writers appear to have lacked. [...] no planning for the good society (a task for optimists) can be successful unless it is done by people who thoroughly understand the relativistic and elusive character of subjective pleasure (an understanding of pessimism).” (*ibid.*, pp. 288-289)

In introducing their purpose, Brickman and Campbell advanced the concept of the “hedonic treadmill” which has become central for the development of happiness studies (Chapter 2):

“the nature of AL phenomena condemns men to live on a hedonic treadmill, to seek new levels of stimulation merely to maintain old levels of subjective pleasure, to never achieve any kind of permanent happiness or satisfaction.” (*ibid.*, p. 289)

Despite this pessimistic fact, Brickman and Campbell thought it was possible to improve both individual and social happiness by means of “wise and foolish ways” (*ibid.*). From the planner’s viewpoint, they thought there were “certain distributions of goods over time, persons, and modalities that [would]

the symposium participants. Campbell presented an outline of his intentions during the final session of the symposium and invited the participants to help him write the article. Whether due to his inspired leadership or the unusual nature of the group, a highly spirited 2-hr discussion ensued. The transcript of this discussion then formed the basis for the article that Brickman and Campbell prepared.” (Appley, 1971, p. 285). The article was followed by a comment by D. McClelland (pp. 303-304).

result in greater happiness than others” (*ibid.*). According to Brickman and Campbell, both “getting off” the hedonic treadmill and “keeping stimulus levels above ALs” in order to generate continuous pleasure (*ibid.*, p. 290) were respectively titanic and impractical solutions³². In order to plan the happy society, they rather considered the possibility of “preventing hedonic ALs from continuously rising, or specifying situations in which AL phenomena either [did] not hold or [could] be modified” (*ibid.*). The authors advanced three different phenomena underlying hedonic ALs: temporal comparison (or discrepancy between present and past rewards), spatial comparison (discrepancy between areas of competence), and social comparison (discrepancy between self and similar others). Making reference to welfare economics, they showed that the effects of these phenomena complicated the analysis of the optimality of welfare distributions in such a strong way, that the inclusion of AL changes should be approached via empirical rather than theoretical analyses (note the closeness with Easterlin’s claims presented in Chapter 2):

“welfare economics is already so overburdened by abstract complications that it seems unfair to add to its theoretical problems. Instead what we recommend, both for welfare economics and for social psychology, are empirical studies of the effects of various distributions of goods over times, modes of competence, and persons [...]. Even without any theoretical underpinning at all, it would seem most useful simply to map out, empirically, how not only the mean but also the variance and the skewness of a distribution of rewards affects satisfaction. This should be done for each of the distributions discussed (temporal, spatial, social).” (*ibid.*, p. 299, references omitted)³³

³² “there may be no way to permanently increase the total of one’s pleasure except by getting off the hedonic treadmill entirely. This is of course the historic teaching of the Stoic and Epicurean philosophers, Buddha, Jesus, Thoreau, and other men of wisdom from all ages. Unfortunately, renouncing the hedonic treadmill is a very difficult thing for men to do at least until, like St. Augustine, they have traveled the full path from innocence to corruption. Even in renouncing the pleasures of the flesh, however, men may experience AL phenomena in their pursuit of piety or saintliness.” (Brickman and Campbell, 1971, p. 300)

³³ Brickman and Campbell were well aware of the problems faced by welfare economics presented in Chapter 1 (they made explicit reference to Duesenberry, 1949; Brown, 1952; and Mishan, 1960). Economists, they wrote, “have long been very gloomy about measuring and comparing the subjective satisfactions that people derive from goods, because such measurement would involve assumptions that they feel are untenable (such as that people have equal capacity for satisfaction). But social psychologists have long been committed to trying to assess such satisfactions in laboratory settings and recently have shown interest in trying to assess more general satisfactions with the ‘quality of life’.” (Brickman and Campbell, 1971, p. 288)

(2) P. Brickman, D. Coates and R. Janoff-Bulman's (1978) "Lottery Winners and Accident Victims: Is Happiness Relative?" was inspired in Brickman and Campbell's (1971) contribution to the AL theory symposium. It consisted in producing and analyzing happiness data in order to explore the effects of "extreme" events as winning a lottery or suffering a crippling accident³⁴. As noted in Chapter 2, it was this paper that triggered much of the controversy about the relative character of happiness (the other was Easterlin's). According to Brickman *et al.* (1978), "the limits of the proposition that happiness [was] relative" had never been tested. They proceeded by means of the following argument:

"If happiness were completely relative, groups that had received extremes of good and bad fortune in life – winning a million dollars versus suffering a crippling accident – should differ from one another in happiness much less than we might expect. The most general framework for considering this possibility is adaptation level theory (Helson, 1964), whose application to the pursuit of pleasure and questions of happiness has been developed by Brickman and Campbell (1971)." (Brickman *et al.*, 1978, p. 918)

For Brickman *et al.* (1978), the effects of both "contrast and habituation" strongly reduced the positive and negative feelings generated by an event³⁵. They thought of their study as a contribution to the "small but growing body of literature" supporting the "notion that happiness [was] relative" (*ibid.*, pp. 925-926), and claimed that the fact of overestimating "the magnitude,

³⁴ The authors performed two studies. The first consisted in face-to-face and phone interviews to 29 accident victims at a "major rehabilitation institute" (Brickman *et al.*, 1978, p. 918), 22 lottery winners from which "7 won \$1 million, 6 won \$400,000, 2 won \$300,000, 4 won \$100,000 and 3 won \$50,000, and a control group of 22 participants. It measured both "general happiness" and "everyday pleasure": "For a measure of general happiness, respondents were asked to rate how happy they were now (not at this moment, but at this stage of their life). They were also asked to rate how happy they were before winning (for the lottery group); before the accident (for the victim group); or 6 months ago (for the control group). Finally, each group was asked to rate how happy they expected to be in a couple of years [...]. For a measure of everyday pleasure, respondents were asked to rate how pleasant they found each of seven activities or events: talking with a friend, watching television [...]. All ratings were made on 6-point scales ranging from 0 for "not at all" to 5 for "very much" (*ibid.*, p. 919). The second study consisted in similar interviews to 44 buyers and 42 nonbuyers of lottery tickets in order to test the happiness differences between these groups.

³⁵ Brickman and his associates were already aware of the early economic analysis of happiness data (namely Easterlin's study). It is interesting to notice that the language of their article was still influenced by visual concepts such as "contrast". By contrast effects they meant the following: "Although lottery winners felt very good about winning the lottery, they took less pleasure than controls in a variety of ordinary events and were not in general happier than the controls [...], such an event may be expected to depress ratings of related but less spectacular events, by contrast" (Brickman *et al.*, 1978, p. 923).

generality, and duration of other people's feelings" led to "serious" biases in interpersonal relations (*ibid.*, p. 926). Their conclusion was the following:

"If observers overestimate the extent to which winning a lottery or being crippled affects the psychological state of participants, this fact itself can have serious consequences. Others may cut off interaction with participants, either to avoid potentially embarrassing social comparison or simply to avoid having to respond to what they expect are extremes of happiness or despair. This tendency for others to reduce interaction can in turn make it harder on people who have experienced such extreme events to evolve a new adaptation to their social environment. If observers could be made aware that severe outcomes do not have as great an impact as might be expected, as the results of the present study show, they might find it less threatening to interact with others who have experienced such outcomes." (*ibid.*)

4.8. From AL theory to behavioral economics

During the 1970s, AL theory was applied to the analysis of economic subjects in at least two different ways. As shown above (Chapter 1), it was used by Scitovsky in 1976 as a theoretical support for *The Joyless Economy* and also in Robert Frank's writings about the role of context in consumer behavior (1). Besides these uses of Helson's theory, the AL principle was also an argument in Kahneman and Tversky's "Prospect Theory" (1979) (2). It is interesting to point out that while Scitovsky's attempts failed to give birth to a new theory of consumer behavior, Kahneman and Tversky's program eventually made its way to the mainstream of the discipline.

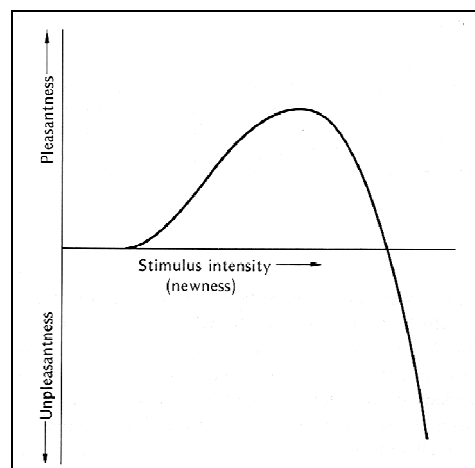
(1) As presented in Chapter 1, the analytical part (the first half) of *The Joyless Economy* (1976), was based on "motivational psychology" which, in Scitovsky's words, corresponded to "that part of the body of physiological psychology which [was] the most relevant to establishing, extending, and correcting, when necessary, the economists' theory of consumer behavior" (Scitovsky, 1976, p. *xii*). Scitovsky's theory analyzed consumer choice between stimulation and comfort in affluent contexts. It explored the affective effects of consumption by means of the analysis of novelty as a source of pleasure. Scitovsky's chapter on "The Pursuit of Novelty" showed that perfect

comfort and lack of stimulation were “restful at first”, but that they soon became boring and then disturbing (Scitovsky, 1976, p. 31). It showed that also “extreme” novelty was disturbing:

“New and surprising sensations are sometimes frightening and shunned, at other times attractive and sought after. The apparent conflict is resolved when we distinguish differences in degree. The new and surprising is always stimulating, but it is attractive only up to a limited degree, beyond which it becomes disturbing and frightening. Attractiveness first increases, then diminishes with the degree of newness and surprisingness. That kind of relation is common in psychology [...]” (*ibid.*, p. 34)

Scitovsky’s analysis was based on the reproduction of Wundt’s curve (Fig. 4.6): “the inverted U-shaped curve which relates the pleasantness of a sensation to its intensity” (*ibid.*). The question raised by Scitovsky was: “what determines the optimal and most satisfying degree of newness?” (*ibid.*)

Figure 4.6 (Scitovsky, 1976, p. 35)



Scitovsky used Wundt’s curve to explain the effects of novelty on consumer’s well-being. The consumption of novelty was supposed to provide the stimulation necessary to fight the boredom of comfort. Based on Wundt’s curve, Scitovsky showed that the stimulation provided by newness was pleasant only if avoiding both “full redundancy” (no newness) and “perfect originality” (too much newness). The following quotation shows that besides Wundt’s analysis, *The Joyless Economy* was also based on AL theory:

“Experiments measuring the arousing effect of such simple sensory inputs such as light and sound have shown that the point of origin, that is, the level where there is no arousing effect, is not the absence of light or sound, but whatever level of it the organism is already experiencing and has adapted to. *This point of origin has been called the adaptation level*

[...] moderate divergences are pleasant, great divergences unpleasant. Sometimes this is graphically expressed by a so-called butterfly diagram, which is obtained by drawing two inverted U-shaped Wundt curves, one to the right and one to the left of the adaptation level as the point of origin.” (Scitovsky, 1976, p. 40, emphasis added)³⁶

Scitovsky’s account of consumption was based on the idea that consumers developed a “view of the world” by “the accumulation of personal experience”, and that this view was the basis of their strategies used “for living – for surviving” (*ibid.*, p. 42). As the environments where consumption took place were continuously changing, Scitovsky thought consumers should continually update their “world view by perceiving new information, processing it, and relating it to [their] previously accumulated fund of knowledge” (*ibid.*). Out of this analysis Scitovsky concluded that in order to be pleasant, consumption required certain skills which were necessary to transform novelty in optimum levels of pleasure:

“the fact that for the maximum enjoyment it must come combined with the already familiar, implies that to enjoy [novelty] a person must first acquire related knowledge. In other words, the enjoyment of novelty requires learning; the consumption of novelty is skilled consumption.” (*ibid.*, p. 58)

In Scitovsky’s view, consumer choice theory failed to recognize the “need for novelty and variety” as well as the “need of consumption skills” to make possible the enjoyment of certain forms of consumption (*ibid.*, p. 150). He also showed that the American education failed to produce skilled consumers (remember Mitchell’s “Backward Art”):

“Until the end of the eighteenth century, education was a privilege of the leisure class and consisted, appropriately enough, of training in consumption skills [...]. In the nineteenth and early twentieth centuries, education in the United States became universal, but it also became more and more a training in production skills and less and less a preparation for the enjoyment of life [...]. Ever since then, economic forces have continued to press for

³⁶ This description corresponds to McClelland and Clark’s diagram (Figure 4.5). The pleasant, Scitovsky wrote, was in between the two extremes: “In short, some redundancy is essential to render anything new pleasantly stimulating, and the degree or amount of redundancy has much to do with how pleasant it is. Just as perfect originality or no redundancy is unpleasant because it is bewildering, so perfect banality or full redundancy is unpleasant because it is boring. The pleasant lies in between, and here too, an inverted U-shaped Wundt curve seems to describe the way in which pleasantness depends on redundancy” (*ibid.*, p. 48).

the progressive crowding out of a liberal, humanistic education by the requirements of science and technology.” (*ibid.*, p. 229)

As noted above (Chapter 1), Scitovsky thought that the American bias in consumption came from a problem of education. He showed that the few sources of stimulation consumed by the American were “watching television, driving for pleasure, and shopping – all of which [were] sources of stimulation requiring no skill” (*ibid.*, p. 232). Like the other economists of affluence, Scitovsky thought the problem lied somewhere else than in expanding the possibilities for consumer choice:

“The remedy is culture. We must acquire the consumption skills that will give us access to society’s accumulated stock of past novelty and so enable us to supplement at will and almost without limit the currently available flow of novelty as a source of stimulation. Different skills of consumption open up different stores of sources of stimulation, and each gives us greatly enhanced freedom to choose what we personally find the most enjoyable and stimulating [...]” (*ibid.*, p. 235)³⁷

(2) Helson’s theory was also a reference in Kahneman and Tversky’s “Prospect Theory” (1979). Kahneman and Tversky’s paper consisted in two main parts: it first tested “expected utility theory” against empirical evidence, and then developed the core of the prospect theory³⁸. According to the empirical evidence presented by the authors, it was not so much with the final state of wealth or welfare that the agents were concerned when confronted to uncertain outcomes, but rather with relative gains and losses. Unlike expected

³⁷ “The novelty of this book lies in introducing novelty as an object of desire and a source of satisfaction. To devote a whole book to the trivial task of adding one more source of satisfaction to the economist’s already long list would be absurd, if it were not for the fact that the stimulus of novelty is among the most fundamental of human needs, and that novelty is a rather special commodity, as special as virginity or a delicate flower, and very unlike the economist’s stock in trade. My fellow economists have been too earnest and puritanical to recognize the consumer’s need for novelty, and the American consumer himself is also too earnest and puritanical to admit to himself his own desire for stimulation and novelty [...]. We get and pay for more comfort than is necessary for the good life, and some of our comforts crowd out some of the enjoyments of life. Far from being bad news, that is really good news, for it means that more people can attain the good life than would be possible if our way were the only one leading to it. Change in life-style, however, is bound to be very slow and very difficult.” (Scitovsky, 1976, pp. 282-284)

³⁸ Kahneman and Tversky’s “Prospect Theory: An Analysis of Decision under Risk” consisted in: “1. Introduction” (p. 263), “2. Critique” (pp. 263-273), “3. Theory” (pp. 274-284), and “4. Discussion” (pp. 284-289). This Section deals just with the theoretical part of the paper which is related to Helson’s theory. Detailed analyses of Kahneman and Tversky’s (1979) paper abound in the literature on behavioral economics presented in Section 4.9.

utility theory, prospect theory defined gains and losses as relative to a reference point:

“people normally perceive outcomes as gains and losses, rather than as final states of wealth or welfare. Gains and losses, of course, are defined relative to some neutral reference point. The reference point usually corresponds to the current asset position, in which case gains and losses coincide with the actual amounts that are received or paid.” (Kahneman and Tversky, 1979, p. 274)

Besides their remarks about the reference points, Kahneman and Tversky’s theory made explicit reference to Helson’s AL theory of perception:

“An essential feature of the present theory is that the carriers of value are changes in wealth or welfare, rather than final states. This assumption is compatible with basic principles of perception and judgment. Our perceptual apparatus is attuned to the evaluation of changes or differences rather than to the evaluation of absolute magnitudes. When we respond to attributes such as brightness, loudness, or temperature, *the past and present context of experience defines an adaptation level*, or reference point, and stimuli are perceived in relation to this reference point. Thus, an object at a given temperature may be experienced as hot or cold to the touch depending on the temperature to which one has adapted.” (*ibid.*, p. 277, emphasis added)

Kahneman and Tversky showed that the same adaptation-level principle applied “to non-sensory attributes such as health, prestige, and wealth” (*ibid.*, p. 277). What is worth noting, however, is that rather than borrowing Helson’s analysis of motivation and behavior, Kahneman and Tversky used just part of Helson’s theory (the AL theory of perception) and advanced their own psychology by proposing prospect theory “as a model of choice” (*ibid.*, p. 284).

4.9. Bypassing behavior control: a conclusion

While the analysis of consumer behavior proposed by economists of affluence and early economists of happiness failed to integrate to the core of the discipline, Kahneman and Tversky’s (1979) approach paved the way for the development of behavioral economics as a mainstream subfield. This section concludes the historical account presented throughout this chapter by showing

that behavioral economics is the “new” economic psychology, that it “bypasses” behavior control, and that it is not radically different from the older psychologies supporting mainstream economics (1). It presents the policy recommendations made by behavioral economists and shows that they also avoid behavior control issues (2).

(1) As shown above (Introduction), there is a rather extensive body of literature dealing with the emergence of behavioral economics as a mainstream subfield. This literature has been written both by behavioral economists, who present the main elements of their subfield, and by historians of economics, who deal with its development in relation to that of other parts of the discipline³⁹.

M. Rabin’s “Perspective on Psychology and Economics” (1998) is written from the viewpoint of a behavioral economist and gives examples of psychological research that can teach economists “about making $U(x)$ more realistic than under standard economic assumptions” (Rabin, 1998, p. 11). It reviews studies about biases in judgment that “lead people to make errors when attempting to maximize $U(x)$ ” as well as “evidence that people have difficulties evaluating their own preferences” (*ibid.*, p. 12). The interesting thing to point out about the emergence of behavioral economics, is that despite all these qualifications the approach is still based on the framework of choice theory. In Rabin’s terms:

“Mainstream economics employs a powerful combination of methods: methodological individualism, mathematical formalization of assumptions, logical analysis of the consequences of those assumptions, and sophisticated empirical field testing. I believe these methods are tremendously useful, and an underlying premise of this essay is that we should strive to understand psychological findings in light of these methods.” (*ibid.*, pp. 12-13)

According to recent surveys about the history of behavioral economics (especially Sent, 2004) there are important differences between the “old” and the “new” programs. Sent shows that the “old” behavioral economics promoted by George Katona and Herbert Simon failed because of their radical

³⁹ It is not the object here to present the history of behavioral economics in detail, but just a few important traits of it. Historical analyses can be found in Loewenstein and Adler (1995), Loewenstein (1996, 1999), Rabin (1998, 2002), Sent (2004), Camerer *et al.* (2005), Bruni and Sugden (2007), Heukelom (2009), and Angner and Loewenstein (forthcoming).

departures from mainstream economics. Sent's survey deals mainly with Simon's program consisting in finding out "empirical laws that described behavior correctly and as accurately as possible" (Sent, 2004, p. 742)⁴⁰. However, Sent shows that this "old" behavioral economics "never really caught on" whereas "new" behavioral economists are "the rising stars of the profession" (*ibid.*, p. 750)⁴¹:

"Old behavioral economics relied heavily on the insights of Simon, who started from the conviction that neoclassical economists were not all that serious about describing the formal foundations of rationality [...]. Yet, Simon's ideas are missing from the more recent developments. Instead, these rely on the insights from Kahneman and Tversky that use the rationality assumption of mainstream economics as a benchmark from which to consider deviations." (*ibid.*, p. 750)

Interestingly, Harry Helson's theory appears as a support for some of the main "deviations" considered by behavioral economists. In Rabin's (1998) terms: overwhelming evidence "shows that humans are often more sensitive to how their current situation differs from some reference level than to the absolute characteristics of the situation (Harry Helson 1964)" (Rabin, 1998, p. 13). Adaptation-level theory supports the "reference dependence" framework underlying the "endowment effects", "loss aversions", and "diminishing sensitivities" on which much of the new behavioral economics is based. According to Rabin's (1998) account:

"While some economists have over the years incorporated reference dependence into their economic analysis, it is fair to say that the ways and degrees to which reference points influence behavior have not fully been appreciated by economists. Researchers have identified a pervasive feature of reference dependence: In a wide variety of domains, people are significantly more averse to losses than they are attracted to same-sized gains. One realm where such *loss aversion* plays out is in preferences over wealth levels [...]. Loss aversion is related to the striking *endowment effect* identified by Thaler (1980) [...], another important reference-level effect is *diminishing sensitivity*: The marginal effects in

⁴⁰ Chapter 5 is entirely devoted to the analysis of George Katona's behavioral economics.

⁴¹ Economists involved in the development of behavioral economics have been honored with MacArthur Foundation \$500,000 fellowships (S. Mullainathan), "genius" awards (M. Rabin), John Bates Clark medals (A. Shleifer, M. Rabin) and Nobel memorial prizes (D. Kahneman). See Sent (2004).

perceived well-being are greater for changes close to one's reference level than for changes further away." (*ibid.*, pp. 13-15, references omitted)⁴²

These three effects are of course not the only deviations considered by behavioral economists⁴³. But the important point here is that adaptation-level principles are considered as deviations rather than elements making up part of the core of the theory. This suggests that there are other psychological assumptions involved in the development of behavioral economics.

The main psychological theory supporting the development of the "new" behavioral economics is Behavioral Decision Research (BDR) which is a particular branch of psychology. Initiated by Ward Edwards (1954, 1961), BDR was introduced as an attempt to get psychologists interested in choice theory. W. Edwards' seminal paper (1954) was indeed a survey of choice theory published in the *Psychological Bulletin*. Edwards' survey reviewed both the "theoretical literature" and the "rapidly increasing number of psychological experiments" relevant to it (W. Edwards, 1954, p. 380):

"In recent years the development of the economic theory of consumer's decision making (or, as the economists call it, the theory of consumer's choice) has become exceedingly elaborate, mathematical, and voluminous. This literature is almost unknown to psychologists, in spite of sporadic pleas in both psychological and economic literature for greater communication between the disciplines." (*ibid.*)⁴⁴

⁴² Camerer and Loewenstein (2004) summarize the same subject in the following terms: "In classical consumer theory, preferences among different commodity bundles are assumed to be invariant with respect to an individual's current endowment or consumption. Contrary to this simplifying assumption, diverse forms of evidence point to a dependence of preferences on one's reference point (typically the current endowment). Specifically, people seem to dislike losing commodities from their consumption bundle much more than they like gaining other commodities." (Camerer and Loewenstein, 2004, p. 15)

⁴³ Rabin (1998) also mentions "altruism", "reciprocal altruism", "the law of small numbers", "anchoring", the difference between "experienced" and "decision" utilities, "framing effects", "preference reversals", "context effects" and "time-variant preferences" as sources of deviations from standard utility functions.

⁴⁴ Edwards' (1954) paper entitled "The Theory of Decision Making" consisted in (1) "The Theory of Riskless Choices" (pp. 380-389), (2) "Application of the Theory of Riskless Choices to Welfare Economics" (pp. 389-390), (3) "The Theory of Risky choices" (pp. 390-403), (4) "The Transitivity of Choices" (pp. 403-405), (5) "The Theory of Games and of Decision Functions" (pp. 405-410), and (6) "Summary" (pp. 410-411). His 1961 article: "Behavioral Decision Theory" was also a review based on Savage's *Foundations of Statistics* and the development of the "subjectively expected utility maximization model (SEU model)" as an alternative to Von Neuman and Morgenstern's "subjective probability-objective value model" (W. Edwards, 1961, pp. 474-475).

In *Kahneman and Tversky and the Making of Behavioral Economics* (2009), F. Heukelom maintains that early developments in BDR consisted in creating a “new field in psychology that applied economic theories to psychological problems”, and that “Kahneman and Tversky introduced and adjusted Edwards program back into economics” three decades later (Heukelom, 2009, p. 2). It is in this sense that the psychology on which behavioral economics is based is an “economic psychology”. Heukelom shows that the economic agents of the “new” behavioral economists are essentially rational but make decisions that deviate from strictly rational outcomes:

“Man is rational, but because human beings apply heuristics to reconstruct decision problems to manageable proportions, and because they have a specific perceptual system, their reasoned decisions may deviate from the normatively correct solution” (Heukelom, 2009, p. 92)⁴⁵

In order to deal with these “reasoned decisions”, behavioral economists incorporate cognitive and perceptive deviations as complements to rational choice theory. However, even if the incorporation of cognitive and perceptive deviations has “broadened the scope” of consumer choice theory, there have been “limits to broadening the scope as well” (*ibid.*, p. 133), namely in regards to the formation of preferences. Behavioral economists are not focused on the analysis of the formation of preferences but maintain that preferences are exogenous entities:

“Giving up the exogeneity of preferences would have far reaching implications. It would mean that behavioral economists had to give up a fundamental tenet of economics, namely that preferences are given. That would have in turn far-reaching consequences for the theory of decision making itself.” (*ibid.*, p. 135)

Behavioral economics is mainly choice theory complemented with “deviations” coming from cognitive psychology and psychological accounts of perception such as adaptation-level theory. This program is altogether different to both the “old” behavioral economics and the “early” economics of happiness. These two programs were close together and based on alternative psychological assumptions.

⁴⁵ As noted above, it is not the object of this section to provide a full account of behavioral economics. Floris Heukelom’s dissertation entitled *Kahneman and Tversky and the Making of Behavioral Economics* (2009), deals with the history of economics and psychology from the viewpoint of Kahneman and Tversky’s work and that of some of their followers.

(2) The policy recommendations made by behavioral economists give what are perhaps the clearest examples of the relation between behavioral economics and the behavior control approaches advanced by economists of affluence. They show that behavioral economists are not behaviorists as they are far from conceiving consumer behavior as a matter of behavior control. In *Nudge: Improving Decisions about Health, Wealth and Happiness* (2008), R. Thaler and C. Sunstein put forth their policy recommendations under the label of “libertarian paternalism”. Their advice is strongly focused on preserving the agents’ freedom of choice and their arguments are presented in a “freedom and dignity” discourse which stands in line with M. Friedman’s claims as presented in Chapter 3⁴⁶:

“To borrow a phrase from the late Milton Friedman, libertarian paternalists urge that people should be ‘free to choose.’” (Thaler and Sunstein, 2008, p. 5)

On one hand, they maintain that the libertarian part of their strategy consists in designing “policies that maintain or increase freedom of choice” (*ibid.*). On the other, they claim that their aim is to self-consciously steer people’s choices:

“The paternalistic aspect lies in the claim that it is legitimate for choice architects to try to influence people’s behavior in order to make their lives longer, healthier, and better. In other words, we argue for self-conscious efforts, by institutions in the private sector and also by government, to steer people’s choices in directions that will improve their lives.” (*ibid.*)

Although the paternalistic facet of Thaler and Sunstein’s book comes strikingly close to what behaviorists would have proposed in terms of social policies, the authors are extremely careful about keeping their distances with behavior technologies. Instead of “behavior controls” and “positive reinforcements” their approach is presented in terms of “choice architectures” and “nudges”:

“A nudge, as we will use the term, is any aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly

⁴⁶ As the whole program of behavioral economists *Nudge* (2008) is based on the distinction between the perfect rationality of the “homo oeconomicus” (the “Econs” in the book’s terms) and the limited one of humans. The book is divided in four parts: (1) “Humans and Econs” (pp. 19-109), (2) “Money” (pp. 113-163), (3) “Society” (pp. 167-224), and (4) “Extensions and Objections” (pp. 227-254). This section is also based on Camerer *et al.* (2003), and Thaler and Sunstein (2003).

changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid. Nudges are not mandates. Putting the fruit at eye level counts as nudge. Banning junk food does not.” (*ibid.*, p. 6)⁴⁷

Despite its closeness with behaviorism, these “new” behavioral economists present their strategy within the framework of choice theory. Instead of conceiving consumer behavior as the outcome of consumers’ interaction with the environment (they are changing environments in their “fruit” examples thus acknowledging that choices are not really free), they maintain that people are free to make rational choices but “need nudges for decisions that are difficult and rare” (*ibid.*, p. 79). Behaviorism is not even mentioned by these “new” behavioral economists and there is not a single reference to Skinner’s social philosophy in the whole book. Is this not yet another ambiguous episode in the history of the relationship between economics and psychology?

Conclusion to Part II: Bypassing Behavior Control

The history of the relationship between economics and psychology, as presented throughout Part II of the dissertation, shows that there are inevitably psychological assumptions involved in economic accounts of consumer behavior. Part II thus challenged the standard historical accounts stating that “psychology” has been “in” or “out” of economics (Hands, 2009).

Chapter 3 traced the history of behavior control approaches in psychology back to the emergence of the evolutionary naturalist accounts of H. Spencer first and W. James next leading to the development of both functional psychology and behaviorism in America. It maintained that there were economic counterparts to these behavior control approaches, such as the “behaviorist institutionalism” of W. C. Mitchell, L. K. Frank, and M. A.

⁴⁷ The cafeteria example is common in this literature. In Thaler and Sunstein’s (2003) paper it runs as follows: “Consider the problem facing the of a company cafeteria who discovers that the order in which food is arranged influences the choices that people make. To simplify, consider three alternative strategies: (1) she could make choices that she thinks would make the customers best off; (2) she could make choices at random; or (3) she could maliciously choose those items that she thinks would make the customers as obese as possible. Option 1 appears to be paternalistic, which it is, but would anyone advocate options 2 or 3?” (Thaler and Sunstein, 2003, p. 175).

Copeland. Unlike standard accounts of the history of economics and psychology, which take the “ordinalist turn” as a behaviorist move by mainstream economists, Chapter 3 showed that ordinalist economists were far from what behaviorists were doing, for their accounts were still based on the mentalist concept of preferences (mentalism was still “in”).

Chapter 4 maintained that there are always psychological assumptions involved in economic accounts of behavior, and showed that the history of the relationship between economics and psychology is much more complex than the ones which present psychology as being “in” or “out of” economics. It showed that different psychologies have been involved in the development of choice theory, and that the history of the latter is ambiguously related to psychophysics (Sections 4.2-4.4), behaviorism (Section 3.5), information-processing studies (Section 4.5), and Adaptation-Level Theory (Sections 4.6-4.9).

Concerning the history of the economics of happiness, chapters 3 and 4, were intended to support the thesis according to which the history of the subfield is marked by a turning point at the end of the 20th century. The early literature (1974-1999), which focused on theoretical issues and criticized consumer sovereignty, adopted behavior control assumptions. It explored the formation of preferences, especially in affluent contexts, rather than taking preferences as given. However, these economists’ views of consumer behavior were at odds with the psychological assumptions of consumer choice theory. This confrontation explains why their theories were not accepted.

Recent developments in the economics of happiness (1999-2009) are based on a new strategy. The happiness paradox has been fashioned to fit traditional economic analysis (i.e. utility functions) and the subfield has been built with consideration to hedonic psychology rather than opposed to consumer choice theory. This strategy is consistent with the one adopted by the “new” behavioral economists, who focus on the analysis of utility functions, and use several different psychological elements as complements to economic accounts of consumer choice.

III. SUBJECTIVE QUANTIFICATION IN ECONOMICS

Introduction

Unlike the early economics of happiness (1974-1999), which focused mainly on theoretical issues and was supported by just a few empirical studies, the more recent development of the subfield (1999-2009) has been animated by empirical analyses and follows a different strategy. This strategy promotes the economics of happiness as part of a larger movement against economists' skepticism of the use of survey data. In Easterlin's (2002) words (Chapter 2), the new subfield challenges a "disciplinary paradigm stipulating that what people say is irrelevant to understanding their feelings or behavior". Chapters 5 and 6 take the history of the economics of happiness as a viewpoint to analyze the history of the economic use of subjective data.

Chapter 5 deals with the history of subjective quantification and economic *theory* and is based on George Katona's attempt to develop an "economic psychology" or "behavioral economics" out of the analysis of interviews exploring the motives and attitudes of businessmen and consumers. It rejoins Katona's first incursions into the analysis of economic subjects. These incursions happened during the early 1940s and consisted in attempting to build a theory of economic behavior out of the analysis of detailed interviews with businessmen. From 1946 onwards, Katona shifted from the analysis of business behavior to the analysis of consumer behavior. He co-founded the Survey Research Center of the University of Michigan and was in charge of producing and analyzing both the Survey of Consumer Finances and the Survey of Consumer Attitudes. However, the attitudinal data produced under Katona's program of behavioral economics were strongly contested by

economists who evaluated the data produced by the Survey Research Center in terms of their predictive power rather than as theoretical tools making part of Katona's whole program. This episode led to the failure of Katona's attempt to develop a program of behavioral economics as well as the failure of the Federal Reserve's attempts to produce subjective data on expectations as a means to predict aggregate demand. Most of these programs were discontinued during the 1970s.

Chapter 6 is about subjective quantification and economic *policy*. It shows that the analysis of subjective data has made its way into economics at the expense of abandoning the use of the data as theoretical tools and maintains that the main uses of subjective data respond to policy needs. They are used as "tools for government" (Desrosières, 2008a, p. 7). The first sections of the chapter deal with the development of two of the main domains currently involved in the production and analysis of subjective data. The first of these domains corresponds to the analysis of economic expectations and is directly related to the history presented in Chapter 5. It is animated on one hand by the production and analysis of the Index of Consumer Sentiment by the Survey Research Center of the University of Michigan, and on the other by the analysis of the Survey of Economic Expectations initiated in 1993 by Charles Manski, Jeff Dominitz and their associates at the University of Wisconsin. A second important domain involved in the economic analysis of subjective outcomes is the use of contingent valuations by ecological and environmental economists to evaluate natural resources. The emergence of this domain in economics is the clearest example of the recent development of the use of subjective data in response to policy needs rather than to theoretical discussions.

The last sections of Chapter 6 evaluate the economics of happiness in terms of its usefulness in guiding either private or public policies. It shows that, unlike the production and analysis of the Index of Consumer Sentiment or the use of contingent valuations, there is no evident economic application for the economists of happiness' studies. These sections are based on A. Desrosières' (2000, 2003, 2008) historical account which emphasizes the role of statistics as tools for government. The chapter concludes by showing that the latest developments in the economics of happiness are indeed attempts to provide a concrete use to the economic analysis of life satisfaction data.

Chapter 5. Subjective Quantification and Economic Theory: Debates on the Use of Subjective Data

5.1. Introduction

Chapter 5 deals with the history of subjective quantification and economic *theory*, and is based on George Katona's attempt to develop an "economic psychology" or "behavioral economics" out of the analysis of interviews exploring the motives and attitudes of businessmen and consumers. Section 5.2 starts by reviewing the "Measurement without Theory" and the "Full-Cost" controversies from the viewpoint of Katona's program. It shows that Katona's influence in these debates has been largely overlooked. Katona's *Price Control and Business* (1945), one of his first attempts to develop a theory of economic psychology out of interviews, was sponsored jointly by the Cowles Commission and the NBER. Several passages in Koopmans' papers suggest indeed that his measurement-without-theory position against the NBER-type analysis of economic aggregates was partly based on (or at least related to) Katona's inquiry into the motives of the behavior of businessmen.

As for the measurement without theory controversy, the main historical reconstructions of the full-cost controversy tend to overlook the fact that Machlup's paper was also based on Katona's work. The full-cost controversy has been read with regards to the development of the marginalist program rather than focused on the controversy itself, and this reading tends to neglect the fact that Machlup's critical analysis of Lester's questionnaires was based on Katona's methodological writings about the production and analysis of subjective data.

Section 5.3 takes the narrative back to Katona's first incursions to the analysis of economic subjects. This happened during the 1940s as his interests switched from the "psychology of learning and teaching" (*Organizing and Memorizing*, 1940), to the analysis of wartime inflation (*War without*

Inflation, 1942), and then to his monograph for the Cowles Commission (*Price Control and Business*, 1945). Section 5.4 deals with Katona's work after he moved from the Cowles Commission to the Bureau of Agricultural Economics in Washington (1946), and then to the University of Michigan (1947-1972), where he participated in the foundation of the Survey Research Center (SRC) and pursued his aim of building a theory of economic psychology out of the analysis of detailed interviews.

Section 5.5 shows that despite Katona's background as a psychologist of learning and teaching by understanding, he was misunderstood by economists. During the 1950s, an evaluation of the Survey of Consumer Finances conducted by the SRC was requested by the Federal Reserve Board. The different items of the survey had been conceived by Katona as composing parts of a holistic approach (he was a Gestalt psychologist). However, the consultant committee requested by the Board analyzed the survey data in terms of their individual predictive power rather than as composing parts of the whole theoretical program Katona was attempting to develop. The differences between these two positions led to a controversy between the Federal Reserve and the SRC which was extended to more general discussions about the quality of subjective data on economic expectations. During the 1950s and 1960s there were numerous debates and adjustments in the Federal Reserve surveys which were finally discontinued during the 1970s. Section 5.6 deals with these programs as well the division resulting from the controversies mentioned above. It shows that the distinction between the analysis of "attitudes" (SRC) on one hand and that of "buying intentions" and "purchase probabilities" (NBER) on the other, led to different approaches, and eventually to the end of the analysis of subjective data as a means to develop or test economic theories.

5.2. Revisiting the measurement without theory and the full-cost controversies

The history of subjective quantification and economic theory can be related to well-known events in the history of economics. The aim of this section is to revisit (1) the measurement without theory and (2) the full-cost controversies

and to read them from the viewpoint of this history rather than through the light of subsequent developments in mainstream economics.

(1) Philip Mirowski's (1989) account of the measurement without theory controversy presents the event as

“merely the most famous clash in a long-running battle between neoclassical economists (mainly affiliated with the Cowles Commission) and anti-neoclassical or ‘institutionalist’ economists (mainly associated with the National Bureau of Economic Research) to define the very meaning of economic empiricism and the role of stochastic explanation.”
(Mirowski, 1989, p. 65)

According to this narrative, Tjalling Koopmans' (1947) review of Burns and Mitchell's *Measuring the Business Cycle* (1946) is supposed to summarize the Cowles Commission's position which is pictured as a sort of unified defense of “Walrasian neoclassical economics” against the NBER's approach:

“The overwhelmingly significant aspect of this text, and of those in the controversy that followed, is that the battle is primarily over images of science [...]. Koopmans kept insinuating that theory meant Walrasian neoclassical economics, although he never quite comes out and says it *viva voce*. Again, this is because neoclassical economics is an imitation of physics; he is simply incapable of entertaining the notion that there are alternative programs of legitimate economic research. To smooth over what is obviously a very contentious point, he instead kept appealing to methodological individualism [...]. The persistent appeal to individual behaviour was used by Koopmans as a ruse, a shorthand for neoclassical theory; there can be no other logical meaning to such a claim.”
(Mirowski, 1989, p. 78, reference omitted)

Such an account leads to a dualistic reading of the episode. It makes Koopmans an incarnation of both Walrasian economics and the program of the Commission, leaving no room for alternative explanations of Koopmans' claims¹. Because the controversy has been read as a clash between two antagonist positions, it accentuates the differences between the NBER and the Cowles Commission leaving aside the joint parts of the history, namely

¹ A similar argument was held by Koopmans against Vining's (1949a) reply to his 1947 paper. In a footnote, he avowed to be embarrassed of being credited the whole econometric approach regardless the contributions of Fisher, Frisch or Tinbergen: it is “embarrassing to me to see the methodological approach that resulted from the cumulative effort of these men to be referred to as “Koopmans' methods” or the “methods of Koopmans' group”” (Koopmans, 1949, p. 86).

George Katona's project of founding an "economic psychology" out of the analysis of interviews with businessmen. Katona's *Price Control and Business* (1945) was supported by both the Cowles Commission and the NBER but has been excluded from the history of the controversy². This history has been constructed regarding later developments in econometrics and general equilibrium theory rather than focused on the environment of the controversy itself. Katona's monograph for the Cowles Commission fits Mirowski's (1989) account as follows³:

"individuals were never the object of Cowles' equations, because there was no even remote prospect of actually retrieving individuals' preferences or prices. Further, the Cowlesmen had little respect for survey techniques or participant observation of social actors. This was illustrated in the cool reception given to the survey on wartime price controls conducted by George Katona under the temporary auspices of Cowles."
(Mirowski, 1989, p. 78, reference omitted)

The fact that the Commission's projects turned, eventually, into mainstream programs of econometrics and Walrasian equilibrium theory seems to have conditioned the narratives of the original episode. They overlook the influence of Katona's program which consisted in the analysis of the motives of business behavior and happened roughly the same time the controversy took place. Of course Koopmans' papers were directed against the NBER's program of quantitative analysis. But that was also the case of Katona's project which may be hardly considered "neoclassical" or "Walrasian". Moreover, several passages in Koopmans' papers suggest that he was aware of the implications of Katona's use of survey data. Koopmans mentioned the existence of economic theories "based on evidence of a different kind than the observations embodied in the [NBER's] time series" (Koopmans, 1947, p. 166), and that these theories were based on "knowledge of the motives and

² Katona's "investigation was carried out by the Committee on Price Control and Rationing, which was organized under the joint auspices of the Cowles Commission for Research in Economics and the Conference of Price Research of the National Bureau of Economic Research" (Katona, 1945, p. viii).

³ According to Mirowski's (2002) updated version of the controversy and the relations between the Cowles Commission and the NBER, the "most standard" reconstructions are focused on the Commission's role in the development of econometrics (see Epstein, 1987; Morgan, 1990; Christ, 1994 and references). Unlike these accounts, Mirowski's story explores the Commission's work on "the Walrasian system of price theory" (Mirowski, 2002, p. 216). None of these versions of the story take Katona's work into account excepting Epstein's three pages on "Wartime Price Controls and the Cowles Commission" (1987, pp. 82-85) on which Mirowski's quote is based.

habits of consumers and of the profit-making objectives of business enterprise, based partly on introspection, partly on interview or on inferences from observed actions of individuals” (*ibid.*, p. 166).

In 1949, Koopmans insisted in this point by replying to Vining (1949a) and showing that besides introspection, “the interview or questionnaire, and even small scale experiments” were available as sources of “knowledge about individual behavior” (Koopmans, 1949, p. 88). Although he recognized that there was a mathematical emphasis in the theories based on “hypotheses regarding the behavior of individual consumers, laborers, entrepreneurs, investors, etc.” (*ibid.*), he did not discard the empirical analysis of motives and other factors underlying individual (and not aggregate) behavior:

“we need not and should not confine ourselves to assuming rational behavior. What we need to measure is actual behavior, as shaped by habit, culture, ideals, imitation, advertising, prejudice and misinformation as well as by the narrower economic motives usually referred to as rational. Therefore, besides using deductions from the economic motive, we need to formalize and strengthen our knowledge of the other motives of modern economic man, through observation, interview, and sampling study, drawing on whatever results other social studies have to offer.” (*ibid.*, p. 88)⁴

As we shall see in the next section, Katona’s *Price Control* monograph was devoted to the analysis of “detailed interviews with a small sample of businessmen” (Katona, 1945, p. 4). These interviews were intended to explore “the types and motives of business adjustments” rather than compiling “quantitative data on prices, sales, costs, and profits” (*ibid.*) which was the NBER-type approach to the analysis of price controls. From this perspective, Katona’s studies offered a good support to Koopmans’ position against the quantitative program of the NBER.

(2) Interviewing businessmen in order to understand their behaviors was exactly what started the full-cost controversy with Fritz Machlup’s (1946) attack to Lester’s “Shortcomings of Marginal Analysis for Wage-Employment

⁴ Koopmans achieved his reply to Vining (1949a) by claiming that in the case of finding himself as the “Commissar” pretended by his contender, he would, namely, “ask economists to overhaul their theories of individual behavior, modified in the light of sociology and social psychology” (Koopmans, 1949, p. 91).

Problems” (1946) that triggered the marginalist response to the full-costers⁵. However, as for the measurement without theory controversy, the historians’ efforts to reconstruct this event have been focused on the analysis of subsequent developments in mainstream subjects rather than on the episode itself⁶. These efforts have been mostly focused on half of Machlup’s criticism which was, however, explicitly twofold.

Machlup’s paper consisted in two parts: “I. Marginal Analysis of the Single Firm” (pp. 520-536) which has been largely studied as an affair of economic methodology, and “II. Empirical Research on the Single Firm” (pp. 536-553) which has received less attention⁷. The second part was focused on the validity of the questionnaire data and was largely devoted to the analysis of Lester’s study. Unlike the Oxford Group’s project (based on detailed interviews), Lester’s study was based on answers to mailed questionnaires. Machlup thought the analysis of these questionnaires was “misled by pitfalls

⁵ This section deals with the controversy between Lester and Machlup in 1946 and 1947 and not with the earlier project of the Oxford Group which consisted in finding out “how businessmen would react to government intervention to control the trade cycle” (Lee, 1981, p. 340). It is however worth noting that the Oxford Group used a methodology of detailed interviews which was similar to Katona’s. They proceeded by asking “a sample of entrepreneurs how they did in fact make up their minds what to do when faced with specified changes in their business position” (Harrod, 1939, p. 2). Some results were published in Hall and Hitch’s “Price Theory and Business Behaviour” (1939) concluding that entrepreneurs charged prices which were “equal to prime cost plus an allowance for overheads” (*ibid.*). Based on the results of the interviews, Harrod showed that theorists would get results “closer to the truth by assuming” the full-cost principle rather than proceeding by “equating marginal cost to marginal revenue” (*ibid.*). In 1939, after “Hitler’s invasion to Poland” (Lee, 1981, p. 348), the Oxford Group “disbanded, most of its members being absorbed into the war effort” (*ibid.*). The fact that the confidential “files which contained the entire proceedings of the Group were burned” (*ibid.*), makes it difficult to reconstruct this episode. See Lee (1984), however, for detailed account on the Oxford Group’s project and Mongin (1992) for an account of empirical analyses of full-cost pricing by other authors during the 1940s and 1950s.

⁶ There is a vast literature on this subject. The main references are Harrod (1939), Hall and Hitch (1939), Lester (1946, 1947), and Machlup (1946, 1947). For secondary literature see Machlup (1955, 1967), Lee (1981), Mongin (1992, 1997, 2000), Boulier and Goldfarb (1998) and their references.

⁷ The first part consisted in dismissing the “empiricists” attack upon marginal analysis through the well-known “driver analogy” (see Boulier and Goldfarb, 1998). This argument has been extensively analyzed by historians of economics as it eventually led to the instrumentalist position defended by Friedman (1953) and Machlup (1955) (see Mongin, 1992, 1997, 2000 and references). Machlup’s (1946) analogy was intended to show that the theory of the firm (or the driver) did not imply that businessmen (or drivers) “really” behaved according to the calculating principles of the theory. The variables included as hypotheses were not supposed to be in the businessman’s (or the driver’s) mind. They were included, however, in order to provide theoretical accounts of the businessmen’s (or the drivers’) actions.

of semantics and terminology” as well as “naïve acceptance of rationalizations in lieu of genuine explanations of actions” (Machlup, 1946, p. 536)⁸. According to Machlup, the vast majority of businessmen had “never heard of expressions such as elasticity of demand or supply, sloping demand curves, marginal revenue, marginal cost” (*ibid.*, pp. 536-537). How could they think in such terms, he wrote, if unaware of the words and concepts? Even if aware of the terms employed, Machlup thought businessmen would reveal what businessmen decisions “should” be rather than actual decisions. Based on these arguments, he claimed that Lester’s subjective data were unreliable:

“Psychologists will readily confirm that statements by interviewed individuals about the motives and reasons for their actions are unreliable or at least incomplete. Even if a person tries to reconstruct for himself in his memory the motives and reasons for one of his past actions, he will usually end up with a rationalization full of afterthoughts that may make his actions appear more plausible to himself [...]. It takes an experienced analyst to disentangle actual from imaginary reason, and to separate relevant from irrelevant data, and essential from decorative bits of the information furnished. Written replies to questionnaires are hopelessly inadequate for such purposes.” (Machlup, 1946, p. 538, reference omitted)

Machlup’s attack was directed against the “method of mailed questionnaires without supporting interviews” (*ibid.*, p. 548) and based on Katona’s writings (Katona, 1945, 1946). He showed that Lester’s questionnaires were “inadequate for empirical studies of business conduct” (*ibid.*), and claimed that “Lester’s questionnaires suffered not merely from the inherent weaknesses of the method but also from defects in formulation” (*ibid.*, p. 548-549)⁹.

⁸ Lester replied by arguing that Machlup had misread his study: “Professor Machlup’s critical comments are directed at one questionnaire, although he mistakenly thinks that he is dealing with three questionnaires, which may help to explain why so much of his criticism miscarries” (Lester, 1947, p. 137). Lester’s aim was to test “the validity of conventional marginal theory and the assumptions on which it rest[ed].” (Lester, 1946, p. 81). He showed that “the answers of the replying executives [were] sufficiently consistent, firm by firm, and so overwhelmingly support[ed] certain reasonable conclusions that there [could] be little doubt about the correctness of the general results” (*ibid.*). Lester’s main conclusion was that unlike the marginalist explanation, businessmen did not decide of employment as a function of wage rates: “It is clear from numerous interviews that most business executives do not think of employment as a function of wage rates but as a function of output. When questioned regarding the employment effects of increased or reduced wages they usually end up by stating that orders, not wage changes, are the important factor in output and employment” (Lester, 1946, p. 67).

⁹ The references omitted in the preceding and the following quoted are Katona’s *Price Control and Business*: “only detailed interviews can probe into the motives behind business

It is worth noting that even though Machlup's position was explicitly based on Katona's work, the latter is absent from the literature on the controversy¹⁰. It is also interesting to point out that Machlup's defense of marginalism in 1946 was *not yet* an "as if" defense, as his argument was based on discussions about concrete rather than hypothetical behavior:

"Business men do not always 'calculate' before they make decisions, and they not always 'decide' before they act. For they think that they know their business well enough without having to make repeated calculations; and their actions are frequently routine. But routine is based on principles which were once considered and decided upon and have then been frequently applied with decreasing need for conscious choices." (Machlup, 1946, pp. 524-525, references omitted)

The instrumentalist interpretation of the theory of the firm became an argument supporting the marginalist position during the 1950s. Only then, the discussion turned to the version defended by Friedman (1953) or Machlup (1955). In 1946, however, Machlup's conclusions were far from being instrumentalist arguments:

"I conclude that the marginal theory of business conduct of the firm has not been shaken, discredited or disproved by the empirical tests discussed in this paper. I conclude, furthermore, that empirical research on business policies cannot assure useful results if it employs the method of mailed questionnaires, if it is confined to direct questions without carefully devised checks, and if it aims at testing too broad and formal principles rather than more narrowly defined hypotheses." (Machlup, 1946, pp. 553-554)¹¹

decisions" (Katona, 1945, p. 210, quoted by Machlup, 1946, p. 538) and his "Psychological Analysis of Business Decisions and Expectations" (1946). As we shall see, Katona's work was based on the analysis of interviews intended to explore the concrete (not hypothetical) behavior of businessmen.

¹⁰ Mongin's (1992) account is a good example. It makes reference to empirical studies by Eiteman (1947), Edwards (1952), and Earley (1955, 1956) without even mentioning Katona's work.

¹¹ According to the later "as if" argumentations, the economic agent (in this case "the firm") was not supposed to be a "real" (i.e. observable) agent. This was, however, not the case in 1946 (see Mongin, 1992, 1997, 2000); otherwise Machlup would have probably written just half of his paper. From Machlup's (1967) own revision of the controversy one can read that twenty years after the original episode, there were still "widespread" confusions in regards to the "purposes of the theory of the firm": "My charge that there is a widespread confusion regarding the purposes of the 'theory of the firm' as used in traditional price theory refers to this: The model of the firm in that theory is not, as so many writers believe, designed to serve to explain and predict behavior of real firms; instead, it is designed to explain and predict changes in observed prices (quote, paid, received) as effects of particular changes in conditions (wage rates, interest rates, import duties, excise taxes, technology, etc.). In this causal connection the firm is only a theoretical link, a mental construct helping to obtain how

5.3. Katona's psychology, wartime inflation, and the effects of price controls

As noted above, Katona's *Price Control and Business* (1945) consisted in studying economic behavior through interviews and was sponsored by both the NBER and the Cowles Commission (2). The monograph consisted in applying "psychological methods to economic research" (Katona, 1945, p. viii) and made part of Katona's project of developing an "economic psychology". The origins of this economic psychology can be traced back to his work on the psychology of learning and teaching as well as his early studies on wartime inflation (1)¹².

(1) Katona's *Price Control and Business* (1945) was at the same time war planning research and his first attempt to develop an economic psychology out of the empirical analysis of the attitudes and motives underlying economic behavior. Katona's work as a psychologist held in the *Gestalt* tradition and was devoted to the analysis of learning and teaching. His main argument consisted in stating that it was better to "learn by understanding" whole processes than to learn by memorizing specific associations¹³. *War without*

one gets from the cause to the effect. This is altogether different from explaining the behavior of the firm" (Machlup, 1967, p. 9).

¹² George Katona [1901-1981] was born Hungarian and obtained his doctor degree in experimental psychology from the University of Göttingen in 1921. He worked in Germany as a psychologist and economic journalist in a context of "enormous inflation which could not be adequately explained by the prevalent economic theories" (Wärneryd, 1982, p. 1). In 1933, the journal of which he was the editor was "confiscated by Hitler" after what he emigrated to the America (*ibid.*, p. 2). He taught psychology at the *New School*, and worked for two years in the Cowles Commission after what he moved to the Division of Program Surveys of the Department of Agriculture in 1946 and to the University of Michigan in 1947 where he co-founded the Survey Research Center and worked until (and after) his retirement in 1972.

¹³ Katona's main work as a psychologist was published in 1940: *Organizing and Memorizing: Studies in the Psychology of Learning and Teaching* and became a "classic" reference for psychologists in this field. Katona's volume was the outcome of experimental studies of learning processes. It compared different methods of learning and opposed Gestalt theory to the dominant learning theories of his time which were based on "behavioristic associationism" (Katona, 1940, p. 26). According to Katona's theory of learning, the subject-matters should be taught "as parts of whole processes" rather than as separate outcomes. He showed that "learning by memorizing" was a "different process from learning by understanding" (*ibid.*, p. 53) and concluded the volume by stating that pupils "should learn to learn by understanding" rather than "merely learn to memorize" (Katona, 1940, p. 260). *Organizing and Memorizing* was structured as follows: "I. The Field of Research" (pp. 3-31), "II. Experiments on Two Processes of Learning" (pp. 32-54), "III. Introduction to the Problem of Transfer of Training" (pp. 55-80), "IV. Experiments Concerning Transfer of Training" (pp. 81-107), "V. Theory of Transfer of Training" (pp. 108-136), "VI. Forgetting

Inflation (1942) was Katona's first attempt to apply this psychology to the analysis of economic subjects. It consisted in analyzing wartime inflation in the American economy and started from the premise that inflation was "not the automatic effect of economic factors" (Katona, 1942, p. 4), but rather strongly influenced by psychological variables¹⁴. He described the standard economic accounts of wartime inflation in the following terms:

"Government expenditures for war materials increase the amount of money in the hands of the consumers, while at the same time the large-scale conversion of industrial plants to armament production diminishes the quantity of goods available for consumption; more money competes for fewer goods [...]. In this theory the so-called law of supply and demand is considered the fundamental principle of economic life. Price rises are brought about by demand exceeding supply, price declines by supply exceeding demand." (*ibid.*)

Katona's aim was to challenge this kind of accounts which were based on "an incomplete conception of the origins of inflation" (*ibid.*). He showed that according to this "pessimistic" account of the problem, inflation was bound to be considered an inescapable effect of war planning. The psychologist's aim was to show that inflation was "not the automatic effect of economic factors" (*ibid.*). Instead of quantifying economic aggregates such as available income and supply of "civilian goods" (what the NBER did), Katona thought that the theory should incorporate the analysis of past experiences of the economic agents, their specific position in society as well as their understanding of their economic situation as a whole:

"More money in the hands of the people, less merchandise in the stores, these are stimuli to which sellers as well as buyers respond. How they respond depends on many factors, among which are the past experience of the responding persons, the setting of the stimuli, and the way in which the stimuli are understood. The prediction of a response from knowledge of the stimuli alone, without consideration of the other factors, cannot have general validity. [...] different behaviors in the same objective situation, are possible. This does not mean that there are no scientific laws of human behavior, but only that these

and the Effect of Repetition" (pp. 137-163). "VII. Grouping and Learning" (pp. 164-192), "VIII. Memory Traces" (pp. 193-204), "IX. Retention of Principles and Facts" (pp. 205-231), "X. The Prototype of Learning" (pp. 232-253), and "Educational Implications" (pp. 254-260).

¹⁴ *War Without Inflation* was intended as a *Psychological Approach to Problems of War Economy*. It was devoted to the analysis of the following subjects (Chapters 1 to 8): "How inflation arises" (pp. 3-23), "Price fixing and rationing" (pp. 24-52), "Psychological preparation of price fixing" (pp. 53-76), "The consumer facing inflation" (pp. 77-99), "Taxation" (pp. 100-128), "Saving" (pp. 129-148), "Government publicity" (pp. 149-182), and "After the war" (183-196).

laws are more complex than the mechanistic and invariable connection between a specific stimulus and a specific response.” (*ibid.*, pp. 6-7)

Katona considered variations in economic aggregates only as preconditions for “potential inflation” (*ibid.*, p. 7). These would lead to “actual inflation” only if supported by frames of reference giving rise to “inflationary expectations” (*ibid.*, p. 14)¹⁵. The inclusion of these frames of reference was supposed to allow more accurate explanations of the reactions of consumers and businessmen to economic stimuli. Consequently, Katona thought the fight against wartime inflation had a double character:

“The general thesis of this book is that there are two ways of fighting inflation. One is legislative and administrative action, such as taxation and price fixing; the other is orientation of the public to bring about the best possible responses to wartime conditions and measures.” (*ibid.*, p. 97)

Besides the economic policies of taxation and price fixing, Katona thought it was necessary to make the public understand the purposes of price controls. In other words, that it was necessary to adjust the frames of reference of the public to rally economic behaviors with the wartime objectives¹⁶. According to Katona’s economic psychology, there were two main ways to achieve the adjustment of these frames of reference. It could be sought after by means of behaviorist methods (i.e. conditioning) or by instructing the people about the wartime objectives. In a sense, Katona was advocating for controlling behavior in a way which was at odds with the (behaviorist) institutionalist view of control (Chapter 3)¹⁷. Rather than conditioning responses of the population, Katona thought the government should instruct people by making active use of its role as an announcer:

¹⁵ Katona’s paper “The Role of the Frame of Reference in War and Post-War Economy” (1944) was specifically devoted to the analysis of this subject.

¹⁶ Katona showed that wartime inflation was largely the outcome of inflationary frames of reference formed, by both the government and the media: “Government officials, politicians, newspaper editors and radio commentators have supplied much material for the creation of a framework from which inflationary expectations can be derived [...]. Inflationary attitudes that cannot be changed easily arise when people believe that they have knowledge of facts and trends which make the coming of inflation appear to them inevitable. In this case their understanding will strongly influence their behavior [...]. Without the existence of certain economic and political facts accounting for them, enduring expectations of inflation cannot emerge” (Katona, 1942, p. 15).

¹⁷ Katona’s learning theory was opposed to the reinforcement theories developed by behaviorists and there is indeed an interesting contrast between Copeland’s (1942) and Katona’s (1942) views about wartime controls.

“it is not the function of Government announcements to condition the people. Conditioning is that form of teaching which creates specific associations by frequent repetition [...]. Government publicity [...] must be directed toward achieving the cooperation of the public. For that purpose the material must be presented to the people in such a way that they will be able to make up their own minds. The objective, then, is not to elicit thoughtless acceptance but to encourage thinking, to the end that the essential points of the situation may be grasped.” (*ibid.*, pp. 159-160)

In order to get full cooperation of both businessmen and consumers, Katona thought government announcements should be based on knowledge about the attitudes of the economic agents. Katona’s psychological studies were supposed to be a means to collect this valuable information:

“In order to determine the best ways of insuring public cooperation with specific regulations and appeals, more accurate information about the attitudes of various population groups is desirable. Before private business enterprises introduce a new product or policy, psychological studies – market research – are frequently undertaken to determine the probable reception, as well as the best possible method for obtaining one. Government should be at least as much interested in the response to its measures as is business.” (*ibid.*, 172)

(2) Katona’s *Price Control and Business* (Cowles Commission, 1945) was devoted to the analysis of the “actions of American businessmen as affected by price regulations and other wartime conditions” (Katona, 1945, p. 2). The main features of the study were the following (*ibid.*, pp. 4-5):

- a) *The subject-matter*: business actions under price control, and only incidentally the practices of the OPA [Office of Price Administration].
- b) *The method*: detailed interviews with a small sample of businessmen, better suited for discovering the types and motives of business adjustments than for compiling quantitative data on prices, sales, costs, and profits.
- c) *The sample*: manufacturers and distributors of a few important consumer goods in the Chicago area, and not all businessmen affected by price control.
- d) *The period covered*: April, 1942 to April, 1944, and not the entire period of price control.

Katona’s own description of the method shows that his project was an alternative to the NBER-type analysis of aggregate data. This project was twofold. It was intended at the same time as a recollection of data to support

war planning, and as a test of the potentiality of the method of interviews as a “legitimate tool of economic research”¹⁸:

“To gather material for an appraisal of price control and to aid the fight against inflation were not the sole purposes of this study. A relatively undeveloped method of research, rarely used in the field of economics, was chosen not only because of the expectation that it would yield insight into trends that could not otherwise be discerned but also for the purpose of studying the potentialities of the method itself. The interviewing of businessmen as a legitimate tool of economic research, its advantages and its shortcomings, and the types of interviews best suited to the purpose – these are matters that transcend the interest in price control.” (*ibid.*, p. 5)

The method used by Katona was rather innovative. It consisted in gathering the data through questionnaires designed to make businessmen discuss with qualified interviewers who were granted “relatively wide freedom” (*ibid.* p. 8). These interviewers were capable of reformulating or even omitting questions and changing the order of the different sections of the questionnaires¹⁹.

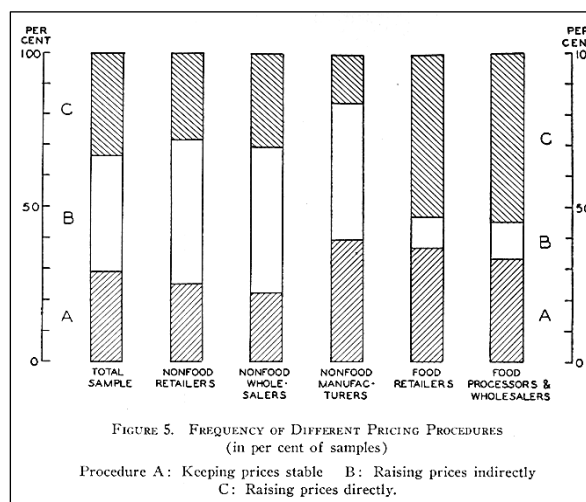
For the most part, the monograph consisted in the analysis of survey data on costs and prices which were not subjective (Figure 0.1). These data provided information about different pricing procedures for different types of activities. The pricing procedures were divided in three large categories: “direct price increases, indirect price increases, and price stabilities” (*ibid.*, p. 31). These quantitative studies were summarized in the following figure²⁰.

¹⁸ Statistics from business questionnaires were already in use for war planning during WWI (Mitchell, 1919). See Madge (1941) and Cantril (1942) for other uses of survey data during WWII and Galbraith (1943) for a more orthodox account about price controls.

¹⁹ Instead of using questionnaires asking “poll” or “multiple-choice” questions or proceeding instead by “informal discussion of the respondent’s individual problems, without the use of any questionnaire”, Katona’s method consisted in the “intermediate technique of detailed interviews” (Katona, 1945, p. 6). This consisted in using “open questions” which were suitable to gather “free-answers”.

²⁰ Katona’s monograph consisted in three parts besides the introduction and conclusions: “Pricing Procedures under Price Control” (pp. 31-95), “Factors Favoring or Impeding Price Stability” (pp. 97-171), and “Changes in the Relative Position of Different Firms” (pp. 173-208). The first of these parts was focused on the analysis of the pricing procedures and the categories in the figure were groups made of the following answers to the interviews: “legal direct price increases”, “illegal direct price increases”, “indirect price increases: quality deterioration”, “reduction in number and size of markdowns”, “uptrading: shift to better-grade merchandise” and “keeping prices stable” (Katona, 1945, pp. 32-90).

Figure 5.1 (Katona, 1945, p. 94)



As shown in the figure, the effects of price control were found to differ for products of different categories. Further analyses of the data showed that the pricing procedures also differed within categories, and even for businessmen facing the same regulation conditions²¹. In order to explain these differences, Katona turned to the analysis of the effects of attitudes on the pricing procedures. This feature of the study went to the core of Katona's project:

“We must go further in the study of psychological factors by analyzing the businessmen's attitudes toward price control. Without them the list of variables that influenced pricing actions cannot be complete because the pricing decisions of different firms differed greatly even where the objective conditions (given type of regulation, certain supply and demand conditions, etc.) appeared to be the same. Moreover, psychological factors must have been the link that determined the degree of importance attached to the various 'objective factors.' How changes in objective conditions influence decisions and actions depends upon the reacting person, because an individual acts in terms of what changes in the environment *mean* to him.” (*ibid.*, p. 157)

Attitudes towards price control were classified in three categories (cooperative, intermediate, hostile). Figure 5.2 shows that the relation found

²¹ The last chapter of the third part of the monograph consisted in the analysis of the effects on the pricing procedures of: different types of regulations (“price freeze”, “formula ceilings” and “dollar-and-cent ceilings”, pp. 100-114), the use of “rationing as an aid to price control” (pp. 115-124), “the role of the market structure” (pp. 125-129), “wartime changes in supply and demand” (pp. 130-141), “influence of price control on demand” (pp. 142-156), and “businessmen's attitudes toward price control” (pp. 157-171).

between these attitudes and the pricing procedures was positive (the more cooperative the attitudes the more stable the prices)²².

Figure 5.2 (Katona, 1945, p. 171)

ASSOCIATION BETWEEN ATTITUDES AND PRICING PROCEDURES				
Pricing Procedures \ Attitudes	Fully Cooperative	Intermediate	Hostile	Total Number of Interviews
A. ("Stability")-----	57	62	6	125
B. ("Indirect Increases")-----	23	104	22	149
C. ("Direct Increases")-----	4	72	73	149
Total Number of Interviews--	84	238	101	423

Consequently, the pricing procedures were supposed to be strongly influenced by attitudes. These attitudes were at their turn found to be the outcome of factors as: (i) "past profit trends", (ii) "profit expectations", (iii) "price expectations", (iv) "long-range considerations", (v) "fairness of the regulations", (vi) "grievances about language and procedures", and (vii) "misconceptions". Katona showed that the last three of these factors were closely related to government actions and strongly conditioned by the "announcements of the price control agency" (*ibid.*, p. 171).

As the attitudes of businessmen were found to explain pricing procedures which had at their turn important effects in the relative positioning of the different firms, Katona concluded that the analysis of the motives and attitudes of businessmen was a worthwhile approach for studying economic phenomena²³. In order to explore the motives and attitudes of businessmen, the method of detailed interviews was found to be the appropriate tool of analysis. It was in this context that Katona produced the passages on which Machlup (1946) supported his claims against Lester's questionnaires (Section 5.2):

²² The attitudes were also classified by type of activity: "In considering the various commodity lines we find that men's wear, shoes, and drugs, which rank highest in attitude scale, were characterized by relatively stable prices; and groceries, liquor, and meat, which rank lowest in the attitude scale, by the most frequent price increases and noncompliance. Women's wear, furniture, and laundries occupy an intermediate position in both scales." (*ibid.*, p. 170)

²³ The last part of the monograph studied the "changes in the relative positions of different firms" (pp. 173-208). It was divided in five chapters that studied these changes as effects of: "informal rationing" (pp. 174-181), "the wholesale trade" (pp. 182-188), "small vs. large firms" (pp. 189-194), "chains vs. independents, inexpensive vs. expensive firms" (pp. 195-202), and "business initiative under price control" (pp. 203-208).

“only detailed interviews can probe into the motives behind business decisions. In our studies, for example, we found that individual businessmen belonging to the same field and operating under the same price regulations often acted very differently. Inquiries into the causes underlying the behavior of each respondent paved the way for an understanding of the differences – and, ultimately, for the determination of the factors responsible for the success or failure of price control.” (*ibid.*, p. 210)

In regards to the efficacy of the price control policies, the main conclusions of the monograph were focused on the effects the government announcements had on the formation of the attitudes of the economic agents. The announcements of the price control agency were found to be essential to gain the cooperation of the population, for the “understanding by both businessmen and consumers of the need for and the purposes of price control” (*ibid.*, p. 217) was among the main factors explaining the success of the price regulation plan:

“If such an understanding is achieved, and if the price regulations are equitable and do not interfere arbitrarily with production, distribution, or consumption, price control need not arouse hostility but, on the contrary, can enlist the cooperation of all concerned. Such cooperation is, in fact, essential to the success of price control, the intricate rules of which cannot be enforced by police power alone.” (*ibid.*)

According to Katona’s findings, government agencies should support the price control actions with announcements directed “to change business attitudes and to rally public opinion behind price control” (*ibid.*, p. 221). He showed that both the “failure to evoke full understanding of the functions of price control” and the businessmen’s confidence in its success were “largely responsible for occasional waves of hoarding and inventory accumulation and the resulting price increases” (*ibid.*).

5.4. Subjective quantification at the Survey Research Center

In 1946 Katona moved to the Bureau of Agricultural Economics in Washington, and in 1947 to the University of Michigan where he founded the Survey Research Center (SRC) together with Rensis Likert and Angus Campbell. Katona’s work in these institutions was largely devoted to the empirical analysis of the attitudes and motives of consumers (not

businessmen). This approach was justified as part of his theoretical program of economic psychology (1). As the economists of affluence, Katona promoted his program as an attempt to renew obsolete theories, in this case the Keynesian consumption function. In his *Psychological Analysis of Economic Behavior* (1951) Katona presented the full version of his theoretical project. It challenged the Keynesian “fundamental psychological law” in regards to consumer behavior (2), and explored economic accounts of business behavior (3).

(1) After the end of WWII, Katona became increasingly engaged in the development of his program of economic psychology. Unlike his earlier studies, the new project was mainly focused on consumer behavior and consisted in bringing psychology into economics in order to update Keynesian macroeconomic analysis²⁴:

“J. M. Keynes, in describing ‘psychological characteristics of human nature,’ did not borrow from psychologists but proposed, without their aid, what he called ‘a fundamental psychological law’ referring to the propensity to consume under the influence of changes in income. Keynes also ignored the work of psychologists in describing certain subjective factors that contribute to the formation of producers’ expectations.” (Katona, 1946, p. 45)

Like the economists involved in macroeconomic analysis, Katona was highly interested in expectations. However, unlike the most part of these economists, he thought expectations (which he considered a special kind of attitude) should not be “derived exclusively from current and past” data (*ibid.*) but studied by means of the analysis of other attitudinal data, as well as the motives of these attitudes²⁵. For Katona, the Keynesian consumption function

²⁴ This section is based on Katona’s work between 1946 and 1957. In this period, he developed the full version of his program of economic psychology out of the production and analysis of the SRC annual surveys of consumer finances as well as surveys sponsored by private foundations. Besides his 1951 volume: *Psychological Analysis of Economic Behavior*, this section is based on Katona and Likert (1946), and Katona (1946, 1947, 1949, and 1957).

²⁵ Katona’s analysis of expectations was also influenced by his earlier work on the psychology of learning. In psychology, he wrote, “the study of expectations forms part of the psychology of learning, since expectations are not innate or instinctive form of behavior but rather the result of experience” (Katona, 1946, p. 51). For Katona, the strongest expectations arose from the agents’ understanding of an economic situation and tended “to change infrequently, radically, and simultaneously” (*ibid.*, p. 54). In regards to economic policy, he thought it was possible to influence the consumer’s propensity to spend and save: “If it is true (a) that the volume of consumption and saving does not follow income in a mechanistic way but depends upon the prevailing expectations, and (b) that one’s past experience is not the only factor shaping expectations but expectations can be influenced, then it follows that the

was the result of making “economics without psychology” (*ibid.*). Instead of analyzing aggregate consumption as the effect of variations in aggregate incomes, he thought the analysis of consumer behavior should be studied by means of the method of “sample interview surveys”. This method was initiated by Rensis Likert in the Division of Program Surveys of the American Department of Agriculture and was used in the “first national survey on liquid assets” to collect financial data of individual economic units as well as data on “motives for, attitudes toward, and expectations of saving” (Katona and Likert, 1946, p. 197)²⁶. It was intended to explore the household’s use of the considerable amounts saved during WWII²⁷. Katona and Likert described the method used in these surveys in the following six points:

“(a) a representative, carefully stratified sample of ‘spending units’ is drawn; (b) specific questions are formulated, designed to secure the full and unbiased cooperation of respondents; (c) all respondents in the sample are asked these questions not only about their actions (e.g., about the amounts they saved), attitudes and expectations, but also about their reasons for those actions, attitudes, and expectations; (d) interviewers, after general training in interviewing techniques, are instructed on how to use this particular questionnaire and how to introduce the various parts of the interview so as to cover all of it systematically with all respondents, and to make them understand why the questions are asked; (e) emphasis is placed on building rapport with the respondent both in the design of the questionnaire and in the interviewing techniques employed; (f) coding and analysis

propensity to spend and save can be influenced. This can be done, without destroying the people’s free choice, by making consumers and business men understand why certain kinds of public and private policies lead to certain results [...]. It is one of the major conclusions of our study of psychology that such actions, the consequences of which are not understood or not trusted, will hardly ever arouse powerful expectations.” (*ibid.*, pp. 58-59)

²⁶ The “Survey of Liquid Asset Holdings, Spending and Saving” (1946) was the first survey of consumer finances conducted under the auspices of the Federal Reserve Board. It was undertaken by the Division of Program Surveys of the U.S. Department of Agriculture whose “senior officials” were Rensis Likert [...], Angus Campbell [...], and George Katona (Smithies *et al.*, 1955, p. 6). After the dissolution of the Division of Program Surveys, the Survey Research Center (SRC) was established under the direction of the same scholars and completed the first annual Survey of Consumer Finances (SCF) in 1947. This survey was also sponsored by the Federal Reserve Board and systematically (though only partly) published in the *Federal Reserve Bulletin*.

²⁷ “Like that of many other institutions, survey research at SRC and ISR developed as a result of the improvisations and special efforts made necessary by World War II and of the associations forged in the course of those efforts. Among those who worked in Washington during the war years with Rensis Likert were the psychologists Angus Campbell, Charles Cannel, Daniel Katz, and Dorwin Cartwright; the Gestalt psychologist turned economist George Katona; and the sampling statistician Leslie Kish” (House *et al.*, 2004, p. 8).

techniques are used which incorporate objective checks on the coding of all measures of attitude and behavior.” (*ibid.*, pp. 197-198)

These points show that Katona and Likert’s studies emphasized the role of the respondents’ understanding of the questionnaires. The results of the interviews showed that standard macroeconomics was accurate for analyzing economic phenomena in contexts of given attitudes and expectations. In these cases, knowledge about how “people acted in the past” was sufficient to understand their future actions (*ibid.*, p. 199). However, the analysis of the interviews also showed that people rapidly changed their behavior and that “changes in attitude precede[d] changes in action” (*ibid.*).

It is important to point out that even if the “information on intervening variables, motives, attitudes, etc.” was particularly useful in the context of Katona’s project, the attitudinal data were considered only as factors acting “between a given environmental situation and the resulting overt behavior” (*ibid.*):

“Analysis of attitudes and motives is required to find out whether in a given situation it is permissible to neglect the intervening variables. Possibly, the situations in which intervening variables may not be neglected are the most interesting ones, marking turning points in business cycles or crucial stages of inflation or deflation.” (Katona, 1947, p. 451)

Because the “economics without psychology” neglected the role of attitudinal variables in the determination of consumer behavior, it was useless to deal with turning points (*ibid.*, p. 452)²⁸. Like psychology: “the study of behavior” (*ibid.*, p. 455), Katona thought that economic psychology, “the study of economic behavior” should be an empirical discipline based on “information concerning attitudes, motives, plans, intentions and expectations”, as well as “micro-economic data on the distribution of income, savings, and liquid asset

²⁸ For Katona, the economics without psychology was based on “data on gross national product, national income, the expenditures and saving of all consumers in a given past period, or – in business economics – data on aggregate production, sales and profits of all business or of certain types of business [...]. These [were] global data or, if divided by the number of people or firms, averages” (Katona, 1947, p. 449). Katona thought that Keynesian economics was based on a “mechanistic view of human behavior” as it assumed that “the propensity to consume [was] a function of income alone” (Katona, 1947, pp. 453). Those who did not endorse this mechanistic view, relied on the “law of large numbers” (*ibid.*): “Some scholars seem to acknowledge that it is not realistic to treat human behavior as automatic. Individual people and individual firms act differently. Yet, for the economy as a whole this does not matter. It is argued that there is an inertia of large numbers; the individual differences cancel out. It is widely held that what one person will do is uncertain, but what thousands will do is not equally uncertain.” (Katona, 1947, pp. 453-454)

holdings” (*ibid.*, p. 456). With these data, the new discipline would be able to “determine the relation of attitudinal and financial data” (*ibid.*) and to supplement (not supplant) the analysis of aggregate data (*ibid.*, p. 459):

“the emphasis on attitudinal information should not imply that purely psychological theories of business cycles, inflation, deflation, etc., are in order. Psychological factors must be taken into account so as to understand these developments, but they are not autonomous [...]. If we oppose such formulations as ‘the propensity to save is a function of income’ or ‘inflation is a function of changes in money supply,’ we must not commit the mistake of arguing that propensity to save, or inflation, is a state of mind the origin of which is noneconomic [...]. Psychological factors and traditional economic factors are interwoven in one unified pattern and must be studied together to understand economic behavior.” (*ibid.*)

(2) In 1951, Katona published his *Psychological Analysis of Economic Behavior*. The first part of the volume was directed towards the Keynesian analysis of consumer behavior and criticized the mechanic character of the Keynesian consumption function based on the argument that it was “rigidly and passively tied to income” (Katona, 1951, p. 136)²⁹. Katona showed that the propensity to consume was not only a function of changes in economic aggregates, but could be also modified in the absence of income changes:

“the major problem confronting the analysis of the propensity to consume is the omission of the study of expectations. Keynes, who assigns great importance to the expectations of businessmen in shaping their policies, does not take expectations into account when he analyzes the factors influencing consumer behavior.” (*ibid.*, p. 141)

Katona’s empirical studies on spending and saving were entirely produced in the SRC and showed that there were “small but consistent” differences in saving behaviors of groups with different income expectations (*ibid.*, p. 157). This challenged the “widespread use” of equations as “ $C = fY$ or $S = fY$ ” (*ibid.*, p. 172):

²⁹ Katona’s *Psychological Analysis* was divided in five parts: “Problems and Tools” (pp. 3-59), “Consumer Behavior” (pp. 63-190), “Business Behavior” (pp. 193-253), “Economic Fluctuations” (257-298) and “Research Methods” (pp. 299-334). This section is focused on the second and third parts. The fourth part presented Katona’s earlier work (presented above), and the fifth and last part the “economic-psychological surveys” (pp. 301-334) discussed below. See Katona (1949) for an empirical analysis of Keynes’ “fundamental law” based on the Survey of Consumer Finances.

“As we have repeatedly stated, repetitiveness of behavior does occur, and it may be true that projecting past relationships will give a correct picture of future relationships. But whether or not that will be the case cannot be known in advance [...]. It is probable than in the past the preponderant use of the time-series approach, both in the theory and in statistical analysis, has done disservice to economic research by impeding the study of the ‘wanderings’ of the consumption function.” (*ibid.*, pp. 172-173)

It is very important to understand that Katona’s program was not designed as a forecasting device and that Katona’s analysis of consumer expectations was not intended as the production of “measures of what will happen” (*ibid.*, p. 174). Katona’s analysis was rather intended as a means “to obtain as complete an account as possible of the psychological field as it prevails at a given moment” (*ibid.*):

“answers to the question, ‘Do you expect to buy a new car between now and a year from now?’ do not enable us to predict the size of automobile sales during the next year. But by studying the time trend of the answers, we may, under certain circumstances, draw the conclusion that the market for new cars is in better, the same, or in worse shape than it was six or twelve months earlier.” (*ibid.*)³⁰

(3) In regards to the behavior of businessmen, Katona (1951) showed that their “motivations” and “investment” decisions, as well as their “output and price” policies, had received little attention by economists. Unlike the analysis of consumer behavior, Katona showed that the analysis of business behavior was not yet supported by quantitative analyses:

“the study of business motivation is at present in the qualitative, or case study, phase. Exact quantitative analysis on the frequency of different motivational patterns in different circumstances is not available.” (*ibid.*, p. 210)

According to Katona’s survey, the few empirical analyses available for business decision-making corresponded to the full-cost literature and the literature on uncertainty (namely F. Knight’s *Risk, Uncertainty and Profit* (1921) and A. Hart’s *Anticipations, Uncertainty and Dynamic Planning* (1940)). He also mentioned well-known elements of macroeconomics as the

³⁰ It is worth noting that Katona (1951) clearly stated that economic psychology did not rely on “basic material collected for practical or legal purposes” which were “by-products of administrative activity” (Katona, 1951, p. 301). He thought the psychological analysis of economic behavior should “be based on empirical studies initiated and guided by the aims of that analysis” (*ibid.*).

“acceleration principle” (*ibid.*, p. 241) and a special mention to both Keynes’ analysis of the “marginal efficiency of capital” (*ibid.*, p. 252) and his writings about “the uncontrollable and disobedient psychology of the business world” (Keynes in Katona, 1951, p. 252).

5.5. Shoemakers’ wives go barefoot...

It is an irony that coming from the author of a psychological theory of learning by understanding, Katona’s economic psychology was misunderstood by economists. In 1954, a consultant committee appointed by the Federal Reserve Board evaluated the quality of the data provided by the SRC. However, this report was focused on the predictive power of the attitudinal data rather than their role as composing parts of a broader project and failed to grasp the essence of Katona’s approach (1). This event led to a controversy between Katona and some of the members of the committee (2).

(1) In 1954, the Subcommittee on Economic Statistics of the Congress of the United States requested the Board of Governors of the Federal Reserve to explore the “adequacy” of “statistical information in the fields of savings, business inventories and business and consumer expectations” (request letters; Smithies *et al.*, 1955). In response to this request, the Board organized five task groups³¹. Of these, the Consultant Committee on Consumer Survey Statistics produced a report known as the “Smithies report”. Its objective was to provide advice about the “usefulness of consumer survey statistics in general” (Smithies *et al.*, 1955, p. 1). It evaluated the Survey of Consumer Finances (SCF) conducted jointly by the Federal Reserve Board and the SRC as well as “interim surveys” on expectations, intentions and attitudes conducted by the SRC under the auspices of private foundations (*ibid.*, p. 6).

³¹The five groups were: “Committee on Savings Statistics”, “Committee on Consumer Expectations”, “Committee on Inventory Statistics”, “Committee on General Business Expectations”, and “Committee on Plant and Equipment Expenditure Expectations”. The second of these became the “Committee on Consumer Survey Statistics” composed by Arthur Smithies (Chairman), Hazel Kyrk, Guy H. Orcutt, Harold C. Passer, Bert Seidman, Samuel Stoufer, James Tobin, and Vernon G. Lippitt (Secretary).

According to the Smithies report, The SCF produced three types of information³²:

“(1) The current economic and demographic status and recent history of consumer spending units [...]. (2) Expectations and attitudes of consumers with respect to their current and their future situation and to that of the economy as a whole [...]. (3) Intentions of consumers with respect to the future. Do households have specific intentions to buy houses, automobiles or other durable goods?” (*ibid.*, p. 8)

The committee suggested that the “relatively expensive” survey data should “supplement rather than replace the traditional kinds of information” (*ibid.*, p. 9)³³. This remark was consistent with Katona’s use of the data collected in the SCF. However, unlike Katona’s view of the data on attitudes, expectations and intentions, (which was the holistic view of a Gestalt psychologist) the report tested the individual predictive power of the different data by comparing them to “alternative” forecasting techniques. Among the different data collected in the SCF, the report was favorable in regards of the analysis of the predictive power of “buying intentions” but disapproving in regards to the predictive power of the “less specific attitudinal data” (*ibid.*, p. 37):

“Buying intentions, properly interpreted, appear to have predictive value [...]. Other attitudes are highly correlated with buying intentions, both over time and as among spending units; and there is so far no convincing evidence that they make an independent contribution to ability to predict, however interesting these attitudes may be for other purposes.” (*ibid.*, p. 38)³⁴

³² The report was divided in the following parts: “I. Introduction” (pp. 4-9), “II. Need for Consumer Survey Statistics” (pp. 9-15), “III. Accuracy of Survey Research Center Data” (pp. 19-34), “IV. Predictive Value of Attitudinal Data” (pp. 37-65), “V. Evaluation of the Technical Efficiency of Survey of Consumer” (pp. 66-78), and “VI. Possibilities of Improvement” (pp. 78-90). This and the following sections are based in the Smithies report and the discussion of it by Katona (1957) and Tobin (1959). Further information about the sample interview surveys, the mail questionnaire surveys and the reliability of survey data is available in Katona’s (1951) chapter on “economic-psychological surveys”. The SCF is also discussed in “Methods of the Survey of Consumer Finances”, *Federal Reserve Bulletin*, July, 1950.

³³ “Perhaps the most important question concerning such data is whether they meet needs which cannot be met by other statistics [...], consumer attitudinal data may contribute useful information about consumers (1) if their attitudes are different from what can be inferred from economic aggregates and (2) if these attitudes are related to consumer behavior.” (*ibid.*, p. 34)

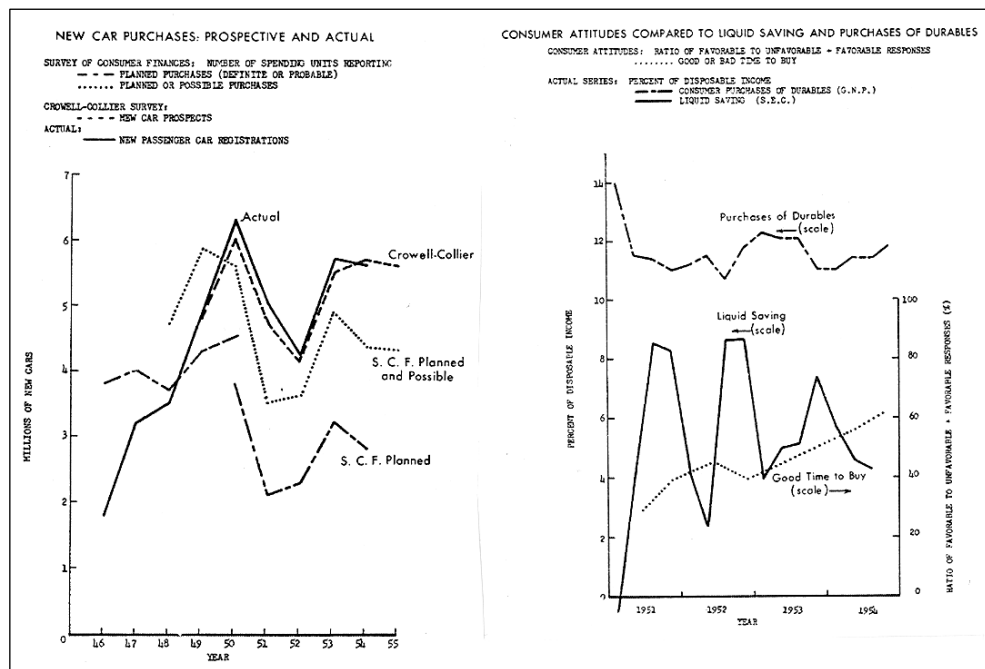
³⁴ In regards to the accuracy of the attitudinal data, the report stated that it had not been proved that “expectations and attitudes other than buying intentions, add[ed] to the predictive value of survey data” (Smithies *et al.*, p. 2). It showed, however, that these data were “easy

The committee's conclusions were the result of an examination comparing purchase intentions and actual purchases for "automobiles", "furniture and major household equipment", and "houses" (*ibid.*, pp. 46-52). According to the Smithies report, the buying intention data of the SCF were less accurate than those of surveys gathering specific buying intentions as the "Crowell-Collier automotive survey":

"a survey devoted entirely to one subject, as the Crowell-Collier survey is to automobiles, may be able to elicit more considered anticipations than a survey, such as the Survey of Consumer Finances, in which this is just one subject among many." (*ibid.*, p. 47)

Charts as the following supported the committee's analysis of the predictive power of buying intention data (left) vs. that of consumer attitudes (right).

Figure 5.3 (Smithies *et al.*, 1955, p. 47, 60)



An important part of the report consisted in analyzing this kind of charts. It showed that the buying intention data ("S.C.F.", left side) were accurate for predicting the direction of changes in purchases. The report was, however, less auspicious in regards of the attitudinal data ("Good Time to Buy", right side):

and inexpensive to obtain" and that the attitudinal questions were "valuable if only because they greatly help[ed] in establishing 'rapport' between interviewer and respondent" (*ibid.*).

“No clear picture emerges from these [right side] Figures. On the whole, these attitudinal series conform less well to ‘actual’ series than do purchase intentions. It would be difficult to say whether there is more conformity of favorable attitudes to subsequent durable goods expenditure or to subsequent liquid saving.” (*ibid.*, pp. 53-54)

Rather than tools for prediction, the report considered that attitudinal variables were useful for noneconomic purposes. It presented these variables as having “considerable descriptive interest in themselves” as measures of the “households’ assessments of their own well-being” (*ibid.*, p. 66). Despite this interest, however, the committee dismissed the analysis of attitudinal data as a matter for economic research:

“if spending units are to be interviewed for other purposes anyway, as in the case of the Surveys of Consumer Finances, the additional cost of attitudinal data is negligible. Indeed questions of this kind have considerable value just in arousing the cooperative interest of the respondent. Interim surveys, which collect solely attitudinal data, require more substantial justification.” (*ibid.*)

(2) The Smithies report was reviewed by George Katona (1957) who claimed that the committee had overlooked the “socio-psychological” character of his studies by comparing them to “more narrowly conceived economic studies” (Katona, 1957, p. 40)³⁵. He thought the report was excessively focused on the use of the attitudinal data as forecasting tools but did “not consider explicitly the basic problems of cross-disciplinary or behavioral research” (*ibid.*). They had overlooked the use of the data as a means to build up a new theory:

³⁵ Katona’s comments were exclusively focused on the scientific conclusions of the report. He seemed to agree with the fifth and sixth parts of the report which dealt with technical aspects of the surveys as well as the operative efficiency of the SRC. The annual budget for the SCF was \$150,000 for 3,000 interviews which was considered expensive by the committee. The problem of coordination between the Federal Reserve Board and the SRC staff was mentioned as an important disadvantage of the general planning of the survey: “The need for coordination between the ideas of the research staff of the Federal Reserve Board in Washington and the senior professionals of the Survey Research Center can and does result in considerable amount of time-consuming discussion. It seems to be agreed that much of the discussion and joint planning, both of the questionnaires and tables, results in a richer product than if either staff were operating independently. Nevertheless, working relationships between the two research staffs, each with highly competent and independently minded scholars, is not without difficulties” (Smithies *et al.*, 1955, p. 69). Besides the general planning, the report also evaluated the efficacy of “the questionnaire”, “the sample”, “the field work”, “the editing and coding”, “the tabulation”, “the preparation of the reports”, as well as the interim projects of the SRC. The most important of these was a study about “The Relation of Attitudes to Economic Actions” financed by the Ford Foundation at a cost of \$148,500 (*ibid.*, p. 77).

“The research work of the Survey Research Center on expectations is based on a psychological theory and is undertaken to develop further this theory [...]. Today in the United States consumer expenditures, and especially postponable expenditures are not rigidly tied to income. They are a function both of resources (‘ability to buy’) and of psychological factors (‘willingness to buy’). Enabling conditions set more or less flexible limits to the rate of spending; the use consumers make of this latitude depends on their attitudes.” (*ibid.*, p. 41)

For Katona, attitudes were “generalized viewpoints” that had an influence on the agents’ “perceptions, cognitions, and behavior” (*ibid.*). Even expectations, a “subgroup of the more general concept of attitudes”, were not considered as forecasting tools but as psychological variables in a program intended to understand the consumers’ current evaluations of their past, present and future situations (*ibid.*)³⁶:

“Expectations – intentions as well as other notions about the future – are current data which help to understand what is going on at the time when the expectations are held. Good diagnosis, of course, helps in making predictions. But forecasting remains a separate step, additional to and different from the measurement of prevailing expectations and intentions.” (*ibid.*)

Based on his training as a Gestalt psychologist, Katona criticized the atomistic viewpoint of the Smithies committee. He showed that the charts presented in the report were “based on the assumption that each individual attitude, taken in isolation, should have a specific relation, not changing over time, to action variables” (*ibid.*, p. 43):

“The basic tenet of Gestalt psychology in which our studies originated is [...] that a part or item may change its meaning and function according to the whole to which it belongs. Thus, it is not at all surprising that expecting prices to rise was at certain times a factor promoting and at other times a factor retarding consumer purchases [...]. Instead of testing the predictive value of each attitude separately, the relation of clusters of attitudes

³⁶ “when a consumer says to an interviewer that he expected to buy a car early next year or that he expects business conditions to improve during the next year – or when x percent of all consumers make such statements – this should not be viewed as a prediction of things to come. But if a sample survey discloses that a significant greater proportion of consumers expect to buy a car and are optimistic about the future than six or twelve months ago, we may conclude that consumer sentiment has changed in the direction of stimulating increased purchases. Such a conclusion represents useful information for those who wish to make predictions about trends in consumer demand, provided it is used in conjunction with other relevant information (especially about changes in consumer income, assets, and debts).” (Katona, 1957, p. 41)

to behavior should be studied [...]. The Smithies report distinguishes sharply between buying intentions which are acknowledged 'to have predictive value' and other expectations [...], however, the two are theoretically and practically so closely related that their separation is not justified." (*ibid.*)

5.6. ... And doctors' wives die young.

Katona's review of the Smithies report generated a reaction by James Tobin who insisted in evaluating the data in terms of their predictive power (1). The controversy between Katona and the Federal Reserve Board led to further discussions about the predictive role of the data collected by the SRC and other Federal programs (2).

(1) In 1959, James Tobin, a former member of the Smithies Committee, replied to Katona's (1957) paper and insisted in exploring whether "the answers to attitudinal and intentions questions provide[d] information of value in predicting the buying behavior of households" (Tobin, 1959, p. 1). Tobin compared the SRC data with information about actual purchases gathered in reinterviews, and insisted in making the separation between buying intentions and the other attitudinal data:

"The calculations reported in this note are designed to appraise the predictive value of certain consumer intentions and attitudes. The appraisal is made by comparing the intentions and attitudes expressed by individual households at the beginning of a year with their subsequent economic behavior." (*ibid.*)

It is worth noting that Tobin was absolutely focused on the predictive power of the SCF data and literally did not see what Katona's whole project was about:

"I do not see how the predictive value of these data can be adequately appraised without confronting the attitudes and intentions of individual households with the record of their subsequent behavior. But it is possible to interpret George Katona, the pioneer student and chief collector of consumer anticipations data, as challenging this point of view." (*ibid.*)³⁷

³⁷ An NBER conference about the quality of the anticipations data was held in 1957 (NBER, 1960). Tobin's paper was strongly based on Okun's contribution to the NBER conference:

The fact that Tobin considered Katona's position in the light of the predictive power of the analysis can be only attributable to a misunderstanding of Katona's program which was focused on understanding rather than predicting consumer behavior. Tobin mentioned the construction of an index by the SRC that he also considered as a forecasting tool:

"Katona holds that it is the Gestalt or cluster of attitudes, rather than attitudes taken singly that matters. Under this direction the Survey Research Center has constructed a time series index of attitudes and intentions. The relation over time between this index and actual expenditures on durable goods in relation to income is the main basis for the claim that attitudinal indicators have predictive value." (*ibid.*, p. 5)

In line with the Smithies report, Tobin concluded that while buying intentions had "predictive value", the other attitudinal questions did not (*ibid.*, p. 10). He strongly defended the Smithies committee's results by quoting the report's conclusion about the data on attitudes:

"These data have considerable descriptive interest in themselves [...]. Moreover, is spending units are to be interviewed for other purposes anyway, as in the case of the Surveys of Consumer Finances, the additional cost of attitudinal data is negligible. Indeed questions of this kind have considerable value just in arousing the cooperative interest of the respondent." (Smithies *et al.*, in Tobin, 1959, pp. 10-11)

By maintaining the committee's position, Tobin advanced a sort of "official" position towards the analysis of attitudinal data. This position strongly influenced the subsequent development of the Federal Programs producing subjective data on expectations.

(2) "I would not conclude without stressing the very considerable debt the profession owes George Katona and his colleagues at the Survey Research Center for their imaginative and pioneering work in the collection and interpretation of buying intentions and attitudinal data [...]. Thanks to the experience they are accumulating, we can investigate the questions which attitudes are the most important ones to investigate in periodic surveys and what is the best way to use these data in combination with other economic information." (Tobin, 1959, p. 11)

"Okun's calculations show that this claim is not solidly supported by the evidence. [...] its predictive success is due entirely to its buying-intentions components, more diffuse attitudinal indicators, make no net contribution." (*ibid.*, p. 5)

Despite Tobin’s words of acknowledgement for Katona and his colleagues at the SRC, the Federal Reserve Board interrupted most of its founding for the SCF in 1960 (Juster, 1964, p. 19; Juster, 2004). It sponsored instead the first “Quarterly Survey of Consumer Buying Intentions” (SCBI) conducted by the U.S. Census Bureau from 1963 onwards (McNeil, 1974, p. 2). Unlike the SCF, which proceeded by detailed interviews, the SCBI proceeded by mailed questionnaires and was entirely devoted to the collection of buying intentions. During the 1960s, Thomas Juster and James Byrnes working at the Census Bureau studied the accuracy of the SCBI data and proposed a new method to replace the “traditional” buying intention questionnaires. Instead of asking respondents to give yes/no answers about their buying intentions, they suggested a new procedure based on the elicitation of “subjective purchase probabilities”.

Figure 5.4 (McNeil, 1974, p. 3)

10	Absolutely certain to buy	10
9	Almost certain to buy	9
8	Much better than even chance	8
7	Somewhat better than even chance	7
6	Slightly better than even chance	6
5	About even chances (50-50)	5
4	Slightly less than even chance	4
3	Somewhat less than even chance	3
2	Much less than even chance	2
1	Almost no chance	1
0	Absolutely no chance	0

In his NBER monograph entitled *Anticipations and Purchases* (1964), Juster recognized the existence of two different approaches to the analysis of subjective data³⁸. On one hand, the holistic approach of Katona and E. Mueller (the SRC approach), and on the other, that of J. Tobin, A. Okun, L.

³⁸ According to Juster, the monograph was strongly influenced by “the staff reading committee” of the NBER composed by Milton Friedman, Jacob Mincer, Ruth Mack, and Philip Cagan and also by the “reading committee of the National Bureau’s Board of Directors: George B. Roberts, Paul A. Samuelson, and Theodore O. Yntema”. Its orientation owed “a great deal to Friedman” and was financially supported by “grants from the Relm Foundation, Ann Arbor, Michigan; Consumers Union of the U.S., Mount Vernon, N.Y.; and by other funds of the National Bureau” (Juster, 1964, p. xvii). The monograph was divided in seven chapters: “1. Introduction and Summary” (pp. 3-16), “2. An Empirical Analysis of Buying Intentions and Subsequent Purchases” (pp. 17-42), “3. Buying Intentions and Purchase Probability: I” (pp. 43-103), “4. Buying Intentions and Purchase Probability: II” (pp. 104-121), “5. Aggregate Intentions – Purchases Relationship” (pp. 122-139), “6. The Influence of Attitudes and Expectations on Purchases” (pp. 140-165), and “7. Multivariate Analysis of Aggregate Purchases and Buying Intentions” (pp. 166-221).

Klein, J. Lansing, S. Whitey and Juster himself (the NBER approach) which was rather focused on the predictive power of the analysis of expectations:

“One view is that consumer attitudes (thought of as generalized feelings of well-being reflecting relative optimism or pessimism) are fundamental determinants of spending and saving behavior and that both expectations (judgments about the course of events external to the household) and intentions (judgments about events internal to the household) are basically attitudes carrying a time dimension. [...]. An alternative viewpoint is that attitudes, expectations, and intentions should be taken at face value. That is to say, expectations reflect the household’s judgment about the future course of events external to the household; intentions, on the other hand, reflect tentative plans to undertake specified actions in the light of these judgments.” (Juster, 1964, pp. 140-141)

The approach defended by Juster and his associates took the attitudinal variables at “face value” and led to the analysis of probabilistic surveys. These surveys were conceived as a means to predict durable good demand efficiently:

“a reasonably good proxy for household purchase probability can be obtained from a survey of subjective purchase probabilities. The data indicate that a survey of buying intentions is simply a less efficient way of getting an estimate of purchase probabilities than a survey of explicit probabilities. Intentions seem to have no informational content that a probability survey does not also have, and the probability survey is able to extract information that is not obtainable from intentions surveys.” (Juster, 1966, pp. 660-661)

The main difference between the two approaches was that unlike Katona’s program of economic psychology, which was a theoretical attempt, the aim of Juster’s approach was to predict purchase rates directly:

“The objectives of a probability survey are, in principle, quite straightforward. An unbiased estimate of the future purchase rate is required, hence the survey should yield an estimate of mean probability which is on average equal to the observed purchase rate. While the distribution of probabilities is not known, there is a presumption that the true distribution is both continuous and relatively smooth.” (*ibid.*, p. 666)

Based on Juster’s methodology as well as the first results of the probabilistic surveys, the Bureau of Census decided to replace the SCBI and to introduce instead the “Survey of Consumer Buying Expectations” (SCBE) in 1966, turning from the analysis of “buying intentions” to that of “subjective purchase probabilities” (McNeil, 1974, p. 3).

However, according to McNeil's (1974) account, even if there appeared to be "legitimate reasons for optimism following the introduction of the Survey of Consumer Buying Expectations" (*ibid.*, p. 5), the quality of the results was insufficient to keep the Census Bureau funding the SCBE. During the early 1970s, "Census Bureau analysts began to express their concern over the performance of the CBE series on expected car purchases" (*ibid.*), and in October 1972, "data users from industry, government, universities, and private research organizations met with Census Bureau officials to discuss the evidence concerning the predictive value of the CBE" (*ibid.*). The final survey was conducted in April 1973 and its interruption marked the end of discussions about the analysis of subjective data on expectations. According to McNeil's account, the Census Bureau program "survived for 15 years because the early part of that period was marked by a high correlation between plans and purchases" (*ibid.*, p. 9). When the series of subjective data "lost the strong trend factors which had been present for much of the 1960s", it "became apparent that aggregate purchase plans were not a good predictor of aggregate purchase behavior", and the Federal programs collecting subjective data on expectations were discontinued (*ibid.*).

5.7. Conclusion

Sections 5.1 to 5.6 explored the main events mentioned in the current accounts of the history of the economic use of subjective data (Introduction, Section 0.2) in order to take this history one step further. The main character of the history presented in this chapter is George Katona, whose role in both the measurement without theory and the full-cost controversies has been largely overlooked by historians of economics. Katona's project was an attempt to develop a theoretical program of economic psychology (later labeled "behavioral economics"; Chapter 6) out of the analysis of detailed interviews; first with businessmen (1942-1946), and then with consumers (1946-1980). Katona's program was based on his psychological training as a psychologist of learning and teaching by understanding. According to this program, economic behaviors were dependent on the attitudes of both businessmen and consumers towards the prevailing economic situations.

The controversy on the measurement of attitudes, arising between the Federal Reserve and the SRC after the Smithies Committee's report on the SRC surveys seems to show that Katona's attempt to develop a "behavioral" theory of consumer behavior was rather misunderstood (Tobin, 1959), or overlooked (Juster, 1966) by economists engaged in the prediction of durable good purchases. Katona's attitudinal data were dismissed because of their lack of predictive power, and instead of founding Katona's program of economic psychology, the Federal Reserve sponsored surveys of economic expectations intended to predict aggregate demand by means of data of "buying intentions" and "purchase expectations". However, the programs promoted by the NBER and the Census Bureau, were discontinued during the 1970s because of their inaccurate predictions. The following chapter deals with what remains of Katona's economic psychology, as well as the current empirical studies of economic expectations.

Chapter 6. Subjective Quantification and Economic Policy: Is There a Use for the Happiness Data?

6.1. Introduction

The statement according to which one can “get an audience of economists to laugh out loud by proposing ironically to send out a questionnaire on some disputed economic point” (McCloskey, 1983, p. 514) could seem part of the past in view of the current use of subjective data in economics. Economists have intensified their studies of “job satisfaction” (*Labor Economics*), “self-assessed health” (*Health Economics*), “subjective expectations” (*Economics of Expectations*), “contingent valuations” (*Ecological and Environmental and Resource Economics*), and even results of “on-line quizzes” or “web-based surveys” (*Economic Education*)¹. However, it seems that much of the recent development of these subfields is the outcome of applied rather than theoretical research. The collection and analysis of subjective data is being mainly advanced as a support for private and public policies. This is the case of “job satisfaction” as a variable of interest for human resource management (Clark *et al.*, 2009), or of “contingent valuations” as tools for quantifying the social effects of private and public projects (Walton *et al.*, 2008). Another important use of subjective data is as proxies for objective variables in econometric models. This is the case of the use of “self-assessed health” as a proxy for health (Lokshin and Ravallion, 2008), or of “subjective retirement expectations” as a proxies for retirement (Haider and Stephens Jr., 2007).

This chapter focuses on the use of the economic analysis of subjective data in general and that of subjective well-being data in particular as “tools for government” (Desrosières, 2008a, p. 7). The first sections of the chapter present two of the main domains in which the use of subjective data has

¹ Recent examples as well as numerous references of these studies can be found respectively in Lévy-Garboua, Montmarquette and Simonnet (2007), Lokshin and Ravallion (2008), Schläpfer (2007), Manski (2004), and Steele (2008).

become intensive: the analysis of economic expectations (Section 6.2), and the use of contingent valuations (Section 6.3).

Section 6.2 is directly related to the history presented in Chapter 5. It deals, on one hand, with the production and analysis of the Index of Consumer Sentiment by the Survey Research Center of the University of Michigan and, on the other, with the development of the Survey of Economic Expectations conducted since 1993 by the University of Wisconsin Survey Center. It deals with the failure of George Katona's program of behavioral economics and maintains that this failure was partly due to the fact that the theoretical part of the program was overshadowed by the development of the Index of Consumer Sentiment, which is nowadays widely used as a forecasting tool by both government agencies and businessmen. In what concerns the Survey of Economic Expectations, it shows that the approach developed by Charles Manski, Jeff Dominitz and their associates at the University of Wisconsin is an attempt to renew with the analysis of expectations developed by Thomas Juster and his associates (Chapter 5). Its long term aim is to use subjective data on expectations as a means to predict economic aggregates.

Section 6.3 deals with the recent development of contingent valuations by ecological and environmental economists. This is perhaps the clearest case showing that the economic use of subjective data has been developed as a response to applied rather than theoretical problems. The economic use of contingent valuations is indeed a reaction to the inclusion of losses of "existence values" among the costs to be assumed by the parties involved in environmental damages and projects with high social impact.

The last two sections of the chapter are based on A. Desrosières (2000, 2003, 2008) history of statistics, according to which there are specific and distinctive configurations between "state, market and statistics" (Desrosières, 2003, p. 553). According to this approach, statistics are not only descriptions, but also tools for government, and thus the main question to ask is what are statistics used for. Section 6.4 maintains that while an important part of the economists involved in the analysis of subjective data are reacting to specific requirements and producing their own data (as is the case of environmental economists), economists of happiness are rather looking for possible uses of life satisfaction data produced, mainly, in household panel surveys. Similar arguments have been advanced in critical reviews of the subfield by economists who maintain that there is no evident use of happiness data as a

support for economic policy, and think economists should “leave the subjective states of human beings to sociologists and psychologists” (Tribe, 2008, p. 467).

Section 6.5 concludes the chapter and the dissertation by returning to the analysis of subjective well-being data by economists of happiness. It presents the Life Satisfaction Approach for valuing environmental goods, which is one of the most recent developments in the subfield. The Life Satisfaction Approach consists in using life satisfaction data as proxies for utility in order to evaluate monetary trade-offs between incomes and environmental variables. It is a direct attempt to give an economic use to happiness data, but creates a conflict with previous developments in the subfield for it considers life satisfaction data as measures of experienced (rather than decision) utilities (Chapter 2).

6.2. Adjusting the Telescope on Society

Despite the failure of George Katona’s attempt to make behavioral economics out of the analysis of detailed interviews, attitudinal data are still produced by the Survey Research Center and widely used as composing parts of the Index of Consumer Sentiment (1). In the meantime, scholars from the University of Wisconsin have renewed with Thomas Juster’s approach by eliciting probabilistic data in the Survey of Economic Expectations (2). The Index of Consumer Sentiment and the Survey of Economic Expectations are both produced and analyzed by economists involved in the prediction of economic aggregates as incomes, consumption or savings.

(1) Despite the negative consequences of the debates about the predictive power of “attitudes”, “buying intentions”, and “purchase probabilities” (Chapter 5), many of the questionnaire items introduced by Katona and his associates during the 1940s are still used by the Survey Research Center, especially those producing the attitudinal data. These data, which were initially answers to introductory questions in the Survey of Consumer Finances, started to be collected separately from 1952 onwards under the label of Survey of Consumer Attitudes with even greater periodicity than the

original survey. It is worth noting that while the measurement of purchase probabilities was discontinued during the 1970s (Chapter 5), the Survey of Consumer Attitudes was conducted in a regular quarterly basis from 1966 to 1977 and from 1978 onwards on a regular monthly basis (Juster, 2004, p. 122). The fact that the analysis of consumer attitudes was maintained suggests that at least part of Katona's program found support within the Survey Research Center².

During the 1960s, Katona developed his program of "behavioral economics" out of the analysis of attitudinal data, while, in the meantime, the Index of Consumer Sentiment produced by the Survey Research Center gained recognition as a forecasting tool. Consequent to the development of his program, Katona attempted to clarify the theoretical role played by the Index of Consumer Sentiment in particular, and the attitudinal data in general. In *The Mass Consumption Society* (1964), Katona distinguished "predicting" from "understanding" short-range demand fluctuations and stated that the predictions derived from the publication of the Index of Consumer Sentiment were just a part of the whole program. The purpose of the Survey of Consumer Attitudes was "not only to find out what [would] happen to discretionary demand, but also to find out *why*" (Katona, 1967, p. 13).

For Katona, the "major task of expectational economics" was the analysis of the "reasons for observed changes" in demand (*ibid.*). He maintained that policy makers as well as the public opinion needed not only the prospects, but also knowledge about the developments explaining "large or small changes in the one or the other direction" (*ibid.*). Katona's program consisted in the analysis of the "the actions of man or groups of men by applying the scientific method – observation, measurement, testing" (Katona, 1968, p. 147):

"Behavioral economics is concerned primarily with the process of decision making regarding spending, saving, investing, borrowing, pricing, etc., and thus supplements the analysis of interrelationships among results of behavior (amounts spent or saved, business investments, prices) which has been the traditional domain of economics." (*ibid.*)

² According to Curtin's (1982) account, whereas the data were originally obtained by means of detailed interviews designed by Katona and his associates, from 1976 onwards the dominant form of data collection has been the use of "random-digit-dialed telephone samples" (Curtin, 1982, p. 344).

In 1968, Katona opposed his strategy to that of the “nonbehavioral approach” defended by Machlup and Friedman³. Katona’s position consisted in stating that the behavioral approach had “resulted in outcomes different from the nonbehavioral approach” for the analysis of consumer behavior, and that it promised to “result in additional such outcomes, even regarding the behavior of the firm” (Katona, 1968, p. 149). As a response to Friedman’s (1953) defense of the “unrealism” of the psychological assumptions of economic theory, Katona suggested to modify the theory by making the assumptions more realistic. He maintained that the aim of behavioral economics was to construct “a theory of economic behavior” (*ibid.*):

“Traditional economic theory has not proved to be a complete success (many of its implications and predictions have not proved to be a correct); how can the theory be improved? [...] One possible way is by making the underlying assumptions more realistic.” (*ibid.*)

During the last part of his career (retired in 1972 and died in 1982), Katona was still engaged in supporting his theory of behavioral economics. In *Aspirations and Affluence* (1971), written together with B. Strumpel and E. Zahn, he summarized his “studies of economic behavior” which he related to the context of “the prosperous 25 years following World War II” (Katona, 1980, p. 14). He maintained that because affluent consumers had a relatively high degree of discretion in their economic behaviors, the importance of behavioral economics varied “in direct proportion to the affluence of the economy” (*ibid.*, p. 10). As noted above, Katona maintained that “discretionary expenditures” depended not only on the “ability to buy” (incomes), but also on the “willingness to buy” (attitudes) of affluent consumers (*ibid.*)⁴.

³ Katona’s (1968) paper was partly a reply to Machlup’s (1967) presidential address to the AEA which consisted in revisiting the full-cost controversy in order to clarify the marginalist position towards the theory of the firm (Section 5.2). Katona’s conception of behavioral research was the following: “behavioral research consists of (a) formulating hypotheses, (b) testing derivations from the hypotheses (“translations” of the hypotheses into operational terms) through observation and measurement, (c) revising the hypotheses accordingly, (d) testing derivations from the revised hypotheses, etc.” (Katona, 1968, p. 149).

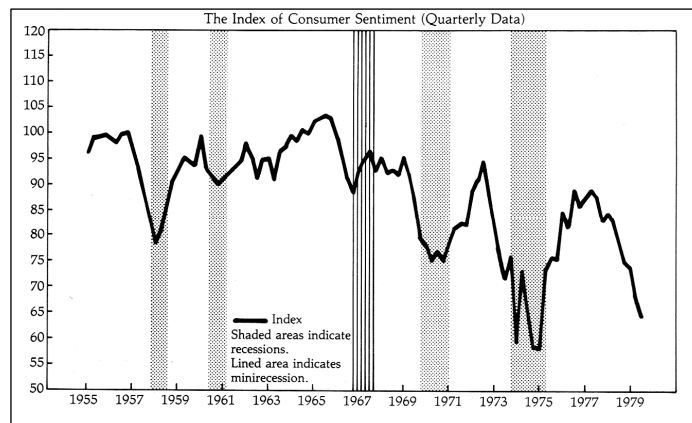
⁴ *Aspirations and Affluence* was clearly a contribution to the economics of affluence (Chapter 1). Like Scitovsky’s *Joyless Economy* (1976), *Aspirations and Affluence* was an “an attempt to compare economic processes in the United States with those in Western Europe” (*ibid.*). It was divided in four parts: (1) “Assumptions and Background” (pp. 1-38), (2) “Consumer Behavior” (pp. 39-118), (3) “Acquisition of Income” (pp. 119-164), and (4) “Conclusions” (pp. 165-201).

In 1980, Katona published his *Essays of Behavioral Economics* which still emphasized the difference between “understanding” and “predicting economic trends” (*ibid.*, p. 8):

“Behavioral economics collects data that explains the trends predicted by the measurement of attitudes and expectations. To say that at a given time expectations point toward an upswing (or downswing) is not enough. The reasons for the changes in expectations need to be known. Such information is regularly supplied by the survey data collected.” (*ibid.*)

For Katona, the Index of Consumer Sentiment was just part of the program of behavioral economics. It was suitable for predicting economic trends by means of the measurement of attitudes and expectations, but insufficient to develop a theoretical analysis of consumer behavior. Figure 6.1 reproduces Katona’s presentation of the Index of Consumer Sentiment showing that periods of economic recessions are systematically preceded by falls in the index.

Figure 6.1 (Katona, 1980, p. 10)



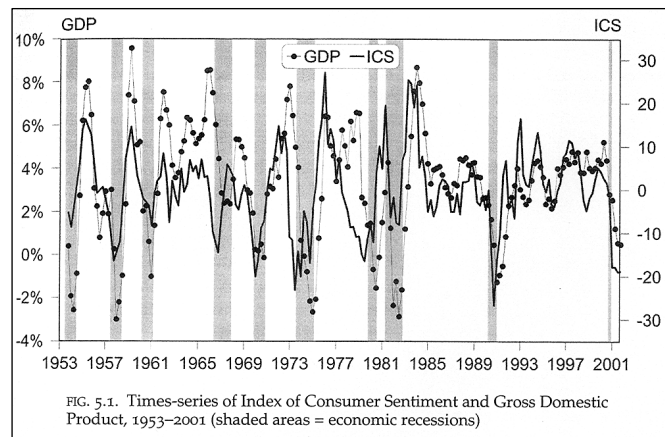
(2) Despite Katona’s insistence in promoting his behavioral program, it was the Index of Consumer Sentiment that concentrated the attention of both economists and businessmen (Curtin, 2004). In a recently published volume entitled *A Telescope on Society* (2004), scholars from the Institute for Social Research and the Survey Research Center of the University of Michigan have written their own history of the development of survey research during the twentieth century⁵. Among the different topics explored in the volume, there

⁵ *A Telescope on Society* is divided in five parts besides the introductory chapters: (1) “Political Behavior and Systems” (pp. 65- 117), (2) “The Behavioral Study of Economics” (pp. 119-193), (3) “From the Study of Organizations to the Study of Health” (pp. 195-270),

is an entire part focused on the history of “The Behavioral Study of Economics” (pp. 119-193). This part of the history is introduced by Thomas Juster and includes a chapter on “Psychology and Macroeconomics” (Curtin, 2004) and another on “The Panel Study of Income Dynamics” (Duncan *et al.*, 2004)⁶.

Curtin’s (2004) “Psychology and Macroeconomics” deals with the Economic Behavior Program developed by Katona and his associates at the Survey Research Center. It shows quite clearly that the main product of this program was not behavioral economics but the Index of Consumer Sentiment. Curtin (2004) maintains that the Index of Consumer Sentiment “is now one of the most closely watched indicators of future economic trends” (Curtin, 2004, p. 136), and highlights “its usefulness for understanding and forecasting changes in the national economy” (*ibid.*). Figure 6.2 shows Curtin’s (2004) representation of the Index.

Figure 6.2 (Curtin, 2004, p. 137)



(4) “Developments in the Study of Family and the Life Course” (pp. 271-354), and (5) “Surveying Subtleties and Complexities of Race” (pp. 355-454).

⁶ Duncan *et al.*’s (2004) chapter entitled “Evolution and Change in Family Income, Wealth, and Health: The Panel Study of Income Dynamics [PSID], 1968-2000 and Beyond”, deals with the history of the PSID led by James Morgan (a member of the Economic Behavior Program of the SRC). This interesting part of the history of the Survey Research Center is, however, only indirectly related to the history presented in this and the following chapters, for the PSID has been mainly focused on the analysis of objective outcomes: “The PSID began in 1968 as a project with a specific mission: to understand the changing fortunes of lower income families as part of the War on Poverty [...]. Through time, many of the initial attitudinal measures were curtailed, and the expansion of the initial behavioral content occurred. By continuing to select content that fits together – [...] – the PSID has become perhaps the most widely used research data set in the social sciences” (Duncan *et al.*, 2004, pp. 188-189).

However, Curtin (2004) clearly states that what “Katona and other behavioral scientists envisioned” was “quite different from what actually came to dominate economic theory” (i.e. the “rational expectations hypothesis”) (*ibid.*, p. 141). “Why should data spoil such a good story?” (Lovell, 1986 in Curtin, 2004, p. 141): this seems to be the question marking the history of the economic use of subjective data. Despite the old (Katona, 1951) and the new (Curtin, 2004) ambitions of scholars working at the Survey Research Center, the “success” of the subjective quantification programs has been on applied rather than theoretical grounds.

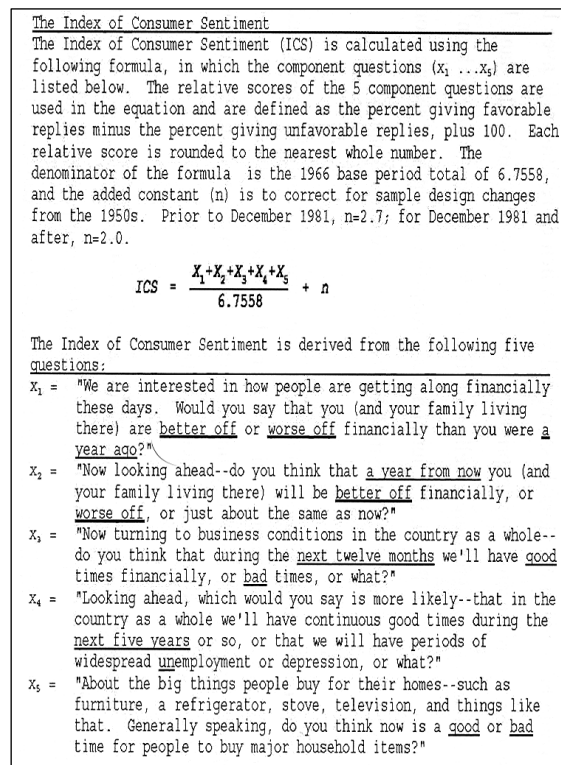
Like Curtin (2004), Dominitz and Manski (2003) show that while the Index of Consumer Sentiment, as well as other indices of consumer confidence, “are prominent in public discourse on the economy” (Dominitz and Manski, 2003, p. 1), the production and analysis of such subjective information has “little presence in modern economic research” (*ibid.*). They explain that the few studies devoted to the subject have “mainly sought to evaluate the predictive power” of the indices and that the analysis of economic expectations is virtually absent in “mainstream” economic journals (*ibid.*, p. 2)⁷.

Dominitz, Manski, and their associates involved in the development of the Survey of Economic Expectations agree with the Smithies Committee’s position concerning the attitudinal data collected by the Survey Research Center (Chapter 5). They believe that “the Smithies Committee was correct to recommend study of the micro foundations of consumer confidence indices” (*ibid.*, p. 4), and that “probabilistic questioning is feasible and yields richer information on consumer beliefs than is obtainable with traditional qualitative questions” (*ibid.*, p. 25).

Dominitz and Manski (2003) examine the “wording of the Michigan questions” and find “inherent weaknesses” in the construction of Index of Consumer Sentiment (*ibid.*). Figure 6.3 shows Dominitz and Manski’s (2003) description of the index including the five questions out of which the index is calculated.

⁷ “Notwithstanding their prominence in public discussions of the economy, the Michigan and Conference Board indices have little presence in modern economic research. Neither *consumer confidence* nor *consumer sentiment* appears in the *Journal of Economic Literature* Subject Index of Journal Articles. A search for the two terms in *EconLit* revealed 78 occurrences in the abstracts of articles and discussion papers published from 1969 through February 2003, but relatively few of these were in ‘mainstream’ economics journals.” (Dominitz and Manski, 2003, pp. 1-2)

Figure 6.3 (Dominitz and Manski, 2003, p. 28)



The Survey of Economic Expectations has been designed to bring back to economics the analysis of expectations. It consists of nationwide telephonic surveys conducted since 1993 by the Survey Center of the University of Wisconsin and studied by scholars such as Dominitz and Manski (1997a, b) or Manski and Straub (2000)⁸.

Manski (2004) shows that the first attempts to analyze economic expectations during the 1960s faced the economists' commitment to the idea that "empirical research on decision making should be based on choice data alone", and that a "quarter century passed before economists began to systematically collect and analyze probabilistic expectations data" (Manski, 2004, p. 1338)⁹. As Thomas Juster and his associates during the 1960s,

⁸ "From February-May 1993, we placed a set of probabilistic expectations questions concerning future income, earnings, and employment as a module in WISCON, an ongoing national computer-assisted telephone interview (CATI) survey conducted by the University of Wisconsin Survey Center at the University of Wisconsin-Madison. The WISCON core questions ask respondents about their labor market experiences, demographics, realized household incomes, and qualitative expectations. We refer to the edition of WISCON containing our expectations module as the survey of economic expectations SEE [...]. The SEE module follows the WISCON core questions." (Dominitz and Manski, 1997a, p. 858)

⁹ Manski (2004) gives a rich overview of the recent development of the analysis of economic expectations. His survey is structured as follows: "1. Introduction" (pp. 1329-1332), "2.

Manski (2004) claims that the “elicitation of subjective probability distributions should have compelling advantages relative to verbal questioning” (*ibid.*, p. 1339)¹⁰.

Besides the fact that, methodologically speaking, the Survey of Economic Expectations is strongly influenced by Juster’s approach, it is important to point out that it is intended to provide “probabilistic expectations” which present the “empirical evidence in the form sought by modern economic theory” (Dominitz and Manski, 1997a, p. 857)¹¹. This approach is thus consistent with the prevailing theories of economic expectations rather than an attempt to develop a new theory.

One of the long-term objectives of Manski and his associates is “to use expectations data to predict behavior” (*ibid.*). Figure 6.4 shows seven of the questions contained in the Survey of Economic Expectations (SEE). These questions explore “job insecurity” (first two questions), “income expectations” (third question), “social security expectations” (fourth to sixth questions), and “investment expectations” (seventh question).

Inference using choice data alone: two illustrations” (pp. 1332-1334), “3. Rational expectations assumptions” (p. 1334-1337), “4. Elicitation of expectations from survey respondents” (pp. 1337-1342), “5. A selection of findings” (pp. 1342-1360), “6. Evaluating the accuracy of elicited expectations” (pp. 1360-1365), “7. Using expectations data to predict choice behavior” (pp. 1365-1369), “8. Measuring ambiguity” (pp. 1369-1370), and “9. Looking ahead” (pp. 1370-1371).

¹⁰ In regards to the production of probabilistic data, Manski shows that “the major platforms for methodological exploration and substantive research include the Health and Retirement Study [of the SRC], the Bank of Italy’s Survey of Household Income and Wealth, the Survey of Economic Expectations, the Dutch VSB Panel Survey, and the 1997 cohort of the National Longitudinal Survey of Youth. Even the venerable Michigan Survey of Consumers now includes some probabilistic questions along with its traditional verbal questions.” (p. 1341-1342)

¹¹ As noted above (Introduction), for Dominitz and Manski, one of the main advantages of the SEE would be that “economists can readily utilize probabilistic expectations data, whereas qualitative expectations data are difficult to utilize” (*ibid.*).

Figure 6.4 (Manski, 2004, pp. 1344-1353)

SEE Job-Loss Question: I would like you to think about your employment prospects over the next 12 months. What do you think is the percent chance that you will lose your job during the next 12 months?

SEE Search-Outcome Question: If you were to lose your job during the next 12 months, what is the percent chance that the job you eventually find and accept would be at least as good as your current job, in terms of wages and benefits?

SEE Household Income Expectations Questions: What do you think is the percent chance (or what are the chances out of 100) that your total household income, before taxes, will be less than Y over the next 12 months?

SEE Social Security Eligibility Question: Politicians and the news media have been talking recently about the future of the Social Security retirement system, the federal program providing benefits to retired workers. The amount of benefits for which someone is eligible is currently determined by the person's retirement age and by earnings prior to retirement. There has been much discussion of changing the form of the Social Security system, so the future shape of the system is not certain. With this in mind, I would like you to think about what kind of Social Security retirement benefits will be available when you are older. In particular, think ahead to when you are about to turn 70 years old and suppose that you are not working at that time. What is the percent chance that you will be eligible to collect any Social Security retirement benefits at that time?

SEE Social Security Benefit Questions: Suppose you are eligible to collect Social Security benefits when you turn 70. Please think about how much money you would be eligible to collect each year. When considering the dollar value, please ignore the effects of inflation or cost-of-living increases. That is, please respond as if a dollar today is worth the same as a dollar when you turn 70. What do you think is the lowest amount of Social Security benefits, per year, that you would be eligible to receive? What do you think is the highest amount of Social Security benefits, per year, that you would be eligible to receive? What is the percent chance (or chances out of 100) that you would be eligible to receive over $\$(Y),000$ of Social Security benefits per year, when you turn 70?

Social Security Maximum Benefit Questions: Now think about Social Security benefits today. In particular, imagine a person who is now 70 years old. Suppose that this person retired from work at age 65 and began collecting benefits after working full time for 40 years. Suppose that, while working, this person had high enough income to be eligible for the maximum Social Security benefit that is currently paid. What is the percent chance that this person currently receives over $\$(Y),000$ of Social Security benefits per year?

SEE/Michigan Mutual-Fund Investment Question: The next question is about investing in the stock market. Please think about the type of mutual fund known as a diversified stock fund. This type of mutual fund holds stock in many different companies engaged in a wide variety of business activities. Suppose that tomorrow someone were to invest one thousand dollars in such a mutual fund. Please think about how much money this investment would be worth one year from now. What do you think is the percent chance that this one thousand dollar investment will increase in value in the year ahead, so that it is worth more than one thousand dollars one year from now?

Answers to the first two questions in Figure 6.4 suggest that subjective probabilities of job loss tend to decrease with schooling. The analysis of these subjective data is also supposed to shed light on the determination of wages, employment, consumption and savings (*ibid.*, p. 1344). Another use of the Survey of Economic Expectations has been the analysis of “income expectations” (question three in Fig. 6.4), which are supposed to be important determinants of consumption/saving decisions (*ibid.*, p. 1346). The analysis of “social security expectations” is aimed to explore “the impact of Social Security policy on labor supply” as well as the households’ decisions about

their “retirement savings” (*ibid.*, p. 1348). Finally, the empirical analysis of “investment expectations” is supposed to raise important questions on how the observed variation in expectations affects investment behavior (*ibid.*, p. 1354).

Despite the differences between the programs producing both the Survey of Economic Expectations and the attitudinal data contained in the Index of Consumer Sentiment, it is important to point out that both the Survey Research Center of the University of Michigan and the University of Wisconsin Survey Center are producing subjective data that are used to predict trends in aggregate data. They are focused on the empirical analysis of consumption, saving, investment and other macroeconomic variables which are useful as “tools for government” (Desrosières, 2008a, p. 7).

6.3. The rise of Contingent Valuations

Contingent valuations are currently used to produce “willingness-to-pay” data to calculate “existence” values of non-market resources. Unlike the traditional methods based on “preference revelations”, contingent valuations rely on the use of surveys producing subjective data (1). One of the main factors leading to the development of contingent valuations has been the need of measures of existence values as a support for public policies and projects (2)¹².

(1) The use of contingent valuations as a means for estimating values of nonmarket goods emerged as an alternative to methods based on revealed preferences. These methods, of which “Travel Cost” and “Hedonic Pricing” are the best known, are indirect as they are based on the idea that consumer choices between market goods can be used to provide information about their preferences for non-market goods.

¹² This section is intended as just an overview of the history of contingent valuations and is focused on its development as a means to “keep pace with policy needs” (Smith, 1993, p. 2). It is mainly based on Smith (1993), Arrow *et al.* (1993) as well as a debate in the *Journal of Economic Perspectives* (1994) which consisted in three papers. The first, by Paul R. Portney, gave an overview of the development of the contingent valuation method and was followed by one case for (Hanemann 1994), and another against (Diamond and Hausman 1994) the use of contingent valuations.

The travel cost method was proposed by Harold Hotelling in 1947 as a means for quantifying the social benefits of national parks. Hotelling's model was based on the idea that the cost of traveling to a recreation site (including the opportunity costs) was an implicit (indirect) price paid by the visitors (Smith, 1993)¹³. By measuring the travel costs for visitors coming from different distances, the method was supposed to give information "comparable to that provided by market transactions" (Smith, 1993, p. 3). The same year (1947), S. V. Ciriacy-Wantrup published "Capital Returns from Soil-Conservation Practices" in the *Journal of Farm Economics* and introduced the idea of asking individuals how much money they were "willing to pay for successive additional quantities of a collective extra-market good" (Ciriacy-Wantrup, 1947, p. 1189). There was, however, no survey in Ciriacy-Wantrup's paper.

In 1963, Robert Davis conducted the first survey in a dissertation entitled *The Value of Outdoor Recreation: An Economic Study of the Maine Woods*, and four years later, in "Conservation Reconsidered" (1967), John Krutilla introduced the distinction between "use" and "existence" values. Krutilla suggested that there was a demand (an "option demand") for natural resources not only among the persons "active in a market for the object" but also among those who placed a value on the "mere existence of biological and/or geomorphological variety". He identified a benefit related to the "widespread distribution" of such varieties (Krutilla, 1967, p. 781). The concept of "existence" value would turn to be fundamental for the subsequent development of contingent valuations.

(2) During the 1970s, (a quarter century after Ciriacy-Wantrup's paper) the first "concerted applications of the method" emerged slowly (Smith, 1993, p. 8; Portney, 1994), and only became intensive during the 1980s. This was mainly a consequence of the "Comprehensive Environmental Response,

¹³ "In 1947, Harold Hotelling proposed the first indirect method for measuring the demand for a non-marketed commodity. His letter, responding to a request by the director of the National Park Service for methods that might be used to measure recreation benefits, introduced the travel cost recreation demand method." (Smith and Karou, 1990, p. 267). According to Smith (1993), the travel cost method is the "most straightforward of the indirect methods" (Smith, 1993, p. 3). He also provides an account of the indirect methods of "hedonic price functions" and "household production function". Something more about these methods is presented in the last section of this chapter.

Compensation and Liability Act” (CERCLA) passed in 1980¹⁴. The CERCLA gave government agencies the right to sue for damages to natural resources (Portney, 1994, p. 7), and the regulations issued under the CERCLA included existence values:

“In regulations promulgated by the Department of the Interior in 1986 under the Comprehensive Environmental Response, Compensation, and Liability Act [...] passive-use values were included among the losses for which trustees could recover. The inclusion of passive-use values was recently upheld by the D. C. Court of Appeals, as long as they could be reliably measured.” (Arrow *et al.*, 1993, p. 1, references omitted)

In 1990, as a consequence of an important oil spill in Alaska (1989), the U. S. Congress passed the Oil Pollution Act, and the U.S. National Oceanic and Atmospheric Administration (NOAA) was designed to issue the regulations establishing the procedures necessary to assess the oil pollution damages¹⁵. In order to issue the regulations, the NOAA asked for assistance to a panel formed by Kenneth Arrow, Robert Solow, Paul R. Portney, Edward E. Leamer, Roy Radner and Howard Schuman (the NOAA panel)¹⁶.

The NOAA’s commitment to the inclusion of existence values was such that the panel “was *not* asked its opinion on the legitimacy” of such values but “to

¹⁴ According to Smith (1993): “Beginning in 1981 [...] and somewhat later in Western Europe, interest has increased in considering estimated values for environmental resources as part of public investment, management, and regulatory decisions. Today demands for this type of valuation information come from diverse sources for different reasons. The courts need valuation information for assessing natural resource damages, public utility commissions use it in environmental costing, and trade negotiators want to evaluate whether environmental policies are being used as trade barriers” (Smith, 1993, p. 1). According to Hanemann (1994), the emergence of contingent valuations was the outcome of their use by “government agencies and the World Bank for assessing a variety of investments” (Hanemann, 1994, p. 21). He mentioned “1600 studies and papers from over 40 countries on many topics, including transportation, sanitation, health, the arts and education, as well as the environment” (*ibid.*).

¹⁵ According to Portney (1994), “two federal laws and one very unfortunate accident” (*ibid.*, p. 6) were the main factors explaining the “heated” controversy raised by contingent valuations. This refers to the CERCLA (1980) and the regulations issued under this Act by the Department of the Interior (1986), the *Exxon Valdez* spill in 1989 (11 million gallons of crude oil spilled in Alaska), and the Oil Pollution Act (1990). It was also during the 1980s that a first conference on the subject was held and that a reference volume on the contingent valuation method was published by Mitchell and Carson (1989): *Using Surveys to Value Public Goods: The Contingent Valuation Method*, Washington DC, Resources for the Future.

¹⁶ The *Report of the NOAA Panel on Contingent Valuation* (1993) consisted in six parts: “I. Introduction” (pp. 1-6), “II. Criticisms of the Contingent Valuation Method” (pp. 6-17), “III. Key Issues in the Design of Contingent Valuation Instruments” (pp. 17-29), “IV. Survey Guidelines” (pp. 29-38), “V. Recommendations for Future Research” (pp. 38-41), and “VI. Conclusions and Recommendations” (pp. 41-45).

confine its attention solely to the potential reliability of the contingent valuation method” (Portney, 1994, p. 8). As there were “no indirect methods through which market data [could] provide at least some clues as to lost values” (Arrow *et al.*, p. 3), and no methodological alternative to contingent valuations, the NOAA panel had a great responsibility. In promulgating the regulations under the Oil Pollution Act, the NOAA would take a definitive position on whether contingent valuations were “capable of providing reliable information” (*ibid.*).

According to Portney (a member of the NOAA panel), the panel’s conclusions ended up upsetting both promoters and detractors of contingent valuations. On one hand, the guidelines proposed would have made “a number of proponents of the contingent valuation method quite unhappy” because of the emphasis given to the use of (costly) personal interviews rather than mailed questionnaires (Portney, 1994, p. 9). On the other, the panel concluded that, if based on the guidelines suggested in the report, contingent valuations could “produce estimates reliable enough to be the starting point of a judicial process of damage assessment” (Arrow *et al.*, p. 43). Despite the many problems indicated in the report, it was the inexistence of “other methods capable of providing information about existence values” (*ibid.*, p. 7) that led the panel to conclude favorably in regards to the use of contingent valuations¹⁷:

“By establishing a series of hurdles for contingent valuation studies to meet, the panel hoped to elevate considerably the quality of future studies and thereby increase the likelihood that these studies would produce estimates that could be relied on for policy purposes.” (Portney, 1994, p. 10)

The reaction against the use of contingent valuations was spirited. Diamond and Hausman (1994) showed that contingent valuations were “not consistent with economic theory” (Diamond and Hausman, 1994, p. 46) and claimed that there were strong pieces of evidence showing that contingent valuations did not “measure the preferences they attempt[ed] to measure” (*ibid.*)¹⁸. They

¹⁷ In producing the report, the NOAA panel observed that the “dramatic increase in the number of academic papers and presentations” based on contingent valuations consisted in estimations of lost passive-use values for litigations “arising from state and federal statutes designed to protect natural resources” (*ibid.*, pp. 4-5).

¹⁸ The NOAA panel had indeed reported six important problems arising in contingent valuations: (i) Inconsistency with Rational Choice, (ii) Implausibility of Responses, (iii) Absence of a Meaningful Budget Constraint, (iv) Information Provision and Acceptance, (v)

considered that contingent valuations did not have “much information to contribute to informed policy-making” (*ibid.*) and thought that government agencies were responsible for such a mistake:

“Our conclusion is often challenged by the common Washington fallacy that even if stated willingness-to-pay is inaccurate, it should be used because no alternative estimate exists for public policy purposes. Put more crudely, one hears the argument that ‘some number is better than no number.’” (*ibid.*, p. 59)

For Diamond and Hausman (1994), contingent valuations had more the character of “an opinion poll” about general concerns regarding the environment than they were a “measure of preferences about specific projects” (*ibid.*). Based on these arguments, they concluded that policy makers were likely to do better by noting the public concern about the quality of the environment, and using expert opinions instead of contingent valuations to “evaluate specific projects”, in order to take economic decisions such as the design of incentives (*ibid.*).

The important thing to take out of this episode is that despite the numerous shortcomings mentioned in the literature, the contingent valuation method is currently established as a technique of subjective quantification. An EconLit search for the past decade (1999-2009) gives 330 references for journal articles containing “contingent valuation” in the title against 58 containing “hedonic price”, and 49 containing “travel cost” (252 contain either “happiness”, “life satisfaction” or “well-being”, Chapter 2). Portney’s (1994) conclusion about the historical development of the method was clear in regards to this issue:

“Whether the economic profession likes it or not, it seems inevitable to me that contingent valuation methods are going to play a role in public policy formulation [...]. Surely, it is better for economists to be involved at all stages of the debate about the contingent valuation method, than to stand by while others dictate the way this tool will be used.” (Portney, 1994, p. 16)

Extent of the Market, and (vi) “Warm Glow” Effects. Kahneman and Sugden’s (2005) paper on the accuracy of CVs is presented in the last section of this chapter.

6.4. A lesson on “what-for-ology”

The development of the analysis of attitudes and expectations (Section 6.2) and the use of contingent valuations (Section 6.3) show that much of the analysis of subjective data has made its way into economics as a support for economic policies rather than as a means to build up or test theories. Alain Desrosières’ (2000, 2003, 2008) approach to the history of statistics shows that there are close ties between statistics, public policies, and historically dated ways of thinking the connections between society, the market, and the economy. This approach suggests that an important question to ask concerning the economic analysis of subjective data is “what is it used for?” It is on this question that the reminder of the dissertation is based (1). The second part of this section comes back to the analysis of the economics of happiness itself. It presents the critical evaluations of it made by Hamermesh (2004) and Tribe (2008). These two reviewers claim that the uselessness of the analysis of life satisfaction data for economic purposes is one of the apparent weaknesses of the subfield (2).

(1) In a recent session of the *Cercle d’Épistémologie Économique* (January, 2009), Alain Desrosières presented his history of the relation between “state, market and statistics”. He showed that rather than studied as “tools for government” (Desrosières, 2008a, p. 7), statistics had been historically perceived just as a “technical tool providing empirical validation for economic research and its political extensions” (Desrosières, 2003, p. 553). Desrosières’ approach consists in showing that there are “modes of statistical descriptions specific to various historical configurations of state and market” (*ibid.*), and his reconstruction is based on “five typical historical configurations”: “Direct Intervention” (*L’Etat Ingénieur*), “The Liberal State”, “The Welfare State”, “The Keynesian State”, and “The New Liberal State” (*ibid.*, p. 554). Figure 6.5 presents these configurations based on both Desrosières’ publications (2000, 2003, 2008), and his presentation at the *Cercle d’Épistémologie*.

Figure 6.5 (source: Desrosières, 2000, 2003, 2008, 2009)¹⁹

	Ways of thinking the society and the economy	Policies	Statistics and models
L'État Ingénieur Production and People (17 th century onwards)	Technical rather than market point of view: Colbert's royal installations, <i>Polytechniciens</i> , centralized planning (USSR), Cold War planning (US space program).	-Optimization (standardization) -Technical management -Long term planning	Comparable to the information needed by the general of an army: Quantities produced and consumed, demographic variables, input-output (Leontief).
The Liberal State Exchange and Prices (18 th century onwards)	Pure theory of the market: Physiocrats, one big market, free trade.	-Against Corn Laws -Against unions -Antitrust legislation	Statistics about markets: International commerce, industrial concentration, statistics for buyers and sellers.
The Welfare State Protecting Workers (late 19 th century onwards)	The market of labor is a special market: Improving the working class, improving the wage levels.	-"Eugenicist" biometrics -Insurance for unemployment -Insurance for work accidents	Labor statistics: Representative samplings, wages, employment figures, unemployment rates, cost of living indices, probabilistic measurement for insurances.
The Keynesian State Decomposing Global Demand (1940s onwards)	The market generates its own crises and must be regulated: Keynes, centralized regulation of macroeconomic equilibrium.	-Keynesian policies	Statistics about macroeconomic equilibrium: Global supply, global demand, national accounting tables, macroeconomic modelling (Tinbergen).
The New Liberal State Polycentrism and Incentives (1990s onwards)	Maximum possible liberty left to the more local levels of society: Incentives rather than regulation, because of rational expectations interventionist policies are undermined, no actor is outside the game (certainly not the state), short term, finance, numerous and unconnected structures of negotiation and decision	-New public management -National accounting as accounting for management -Evidence-based policy -Bioethics -Child abuse -Drug addiction -Prevention of AIDS -Protection of cultural minorities -Gender equality -Quality of consumer goods -Safety of domestic and industrial environments -Fiscal incentives -CO2 markets -Incentives -Empowerment rather than assistance	-Fast statistics -SWB data -Opinion surveys -Local statistics -Indexes -Benchmarking -Micro-data -Panel data -Microeconometrics -Random testing

The first important thing to point out is that Desrosières' five configurations are "typical" in the sense that they coexist and are seldom observed in their pure forms. For the first four configurations, Desrosières presents straightforward relationships between the statistics produced (3rd column), and both the way of thinking the society (1st column), and the policies sharing their time and place (2nd column). This is the case of the input-output analysis of Leontief and the policies of technical management under *L'Etat Ingénieur* (1st row). Under this configuration, statistics of quantities produced and

¹⁹ This figure is based on both Desrosières' (2008a, p. 56) representation and his presentation at the *Cercle d'Épistémologie Économique* (organized in Paris by professors Annie L. Cot and Jérôme Lallement) which was mainly focused on "The New Liberal State" configuration. This is the reason why the Figure emphasizes the last row of the table.

consumed, and the quantification of demographic variables are used as tools for strategic as well as long term planning.

Another interesting example, which is related to the analysis of subjective data on attitudes and expectations, is the production of statistics of global supply and demand supporting Keynesian macroeconomic policies (4th row). According to Desrosières' approach, these variables, as well as the production of indices such as the Index of Consumer Sentiment, correspond to tools used for regulating macroeconomic aggregates.

Concerning the statistics produced under "The New Liberal State" (5th row), and especially when asked about the development of the economics of happiness, Desrosières replied that the important question to ask was "what are they used for?" (*à quoi ça sert?* in French). According to Desrosières' historical account, the New Liberal State configuration is marked by the use of statistics for the design of economic incentives, as well as the comparison of performances of both private and public actors (benchmarking). The use of contingent valuations in order to evaluate the costs of environmental damages is an example of the use of subjective data as tools for designing incentives. The use of average happiness data for comparisons between countries is also an example of the use of statistics as a tool for government under the New Liberal State.

There are at least two important points concerning the relationship between the development of the economics of happiness and Desrosières' "New Liberal State" configuration. First, that this configuration is affluent in the production of survey data produced by both private and public actors at both general and local levels of society (the State is no longer the main responsible of the production of statistics). Second, that the databases produced are available for scholars working in different disciplines. This means that the collection and subsequent analysis of the data are not as closely tied to each other as under the other four configurations. Unlike the psychologists' studies of subjective well-being data, the economists' subfield is not directly connected to the production of the data. Most of economists of happiness are not collecting life satisfaction data but rather using information available in household panel databases as "The British Household Panel Survey" or the "German Socio-Economic Panel Study". This means that the economists of happiness' data are being produced regardless their potential as tools for economic government.

(2) Recent reviews by economists about recent developments in the economics of happiness suggest that the lack of economic use for the analysis of life satisfaction data is one of the apparent weaknesses of the subfield. Daniel Hamermesh's paper entitled "Subjective Outcomes in Economics" (2004), deals with the "upsurge in the use and analysis" of subjective data by economists (Hamermesh, 2004, p. 2). His comparative analysis of the use of data on "life satisfaction", "job satisfaction", "subjective health outcomes", "expectations", and "time stress", is precisely based on the idea that economists are running after subjective well-being data rather than producing them in order to solve economic problems:

"A relevant question is why economists are looking at these expressions of behaviour now. I think it is partly due to the fact that we are seeking things to do with the large amounts of data that we have suddenly discovered – the Mt. Everest phenomenon: If it's there, we must climb it." (*ibid.*)

Hamermesh shows that unlike subjective measures of health, which are mainly used as proxies for "objective health", there is no "apparent link" between life satisfaction data and "any underlying measure of utility"²⁰. Based on psychological studies showing that life satisfaction "adjusts to expectations about living standards" (*ibid.*, p. 3), Hamermesh would expect economists to stay away from the use of such data as proxies for utility. Such an analysis, he believes, would invalidate any policy designed to raise living standards.

Hamermesh maintains that subjective well-being data are far from "measures that maintain the properties that [economists] desire for utility functions" (*ibid.*, p. 3). He claims that in order to justify the economists' venture to the analysis of happiness it is necessary to bring the analysis down to the framework of economic theory. Based on the hundreds of empirical studies made by psychologists, Hamermesh claims that "running fancy regressions on this subject is not new", except for economists (*ibid.*, p. 4):

"Our ability to push buttons in STATA, SAS, TSP, or whatever, is not unique: psychologists and sociologists are perfectly capable of doing that. Our strength, I would argue, and the thing that should underlie whether we do anything in this area, is the extent to which we can bring economic theory to bear on the specifications that we try to formulate." (*ibid.*, p. 4)

²⁰ "Many of the people who work in this area try to equate this happiness measure with utility [...]. I find most of the analysis by economists on this issue to be rather silly." (Hamermesh, 2004, p. 2)

For Hamermesh, economists “need to be careful to base the research on [their] comparative advantage” (*ibid.*, p. 11). Following this strategy, they would avoid duplicating “what other social scientists have already done many times over” (*ibid.*)²¹.

As shown above (Chapter 2), one of the recent strategies followed by economists of happiness has been precisely to make subjective well-being data fit the standard framework of utility functions (Clark *et al.*, 2008). However, there is a problem with this strategy as it finally leads economists to run regressions explaining subjective well-being data (as a proxy for utility) by means of economic and demographic variables. This approach is very close to what psychologists have been doing for over fifty years.

In “Happiness: What’s the Use?” K. Tribe (2008) reviews two recent economic books: Layard’s *Happiness: Lessons from a New Science* (2005), and Bruni and Porta’s *Economics and Happiness* (2005). Besides sharing Hamermesh’s belief that economists should “leave the subjective states of human beings to sociologists and psychologists” (Tribe, 2008, p. 467), Tribe’s review suggests that the “(re)discovery of ‘happiness’ by mainstream economists remains partial and unconvincing” (*ibid.*, p. 461), because of these economists’ lack of position about the economic significance of their findings. Unlike economists of happiness, he claims, welfare economists were “quite clear about what economics was *for*” (*ibid.*, p. 464)²².

6.5. Finding a use for the happiness data: a conclusion

This final section concludes the chapter by coming back to the economic analysis of happiness data. It explores part of the empirical studies introduced

²¹ “On the empirical side, there are books and articles surveying hundreds of empirical studies of life satisfaction by psychologists. If you examine this literature, you find every specification that economists have used has also been included in at least several psychological studies.” (Hamermesh, 2004, pp. 2-3)

²² Tribe’s criticism is supported by Pigou’s (1920) words about the usefulness of his discipline: “The complicated analyses which economists endeavour to carry through are not mere gymnastic. They are instruments for the bettering of human life. The misery and squalor that surrounds us, the dying fire of hope in many millions of European homes, the injurious luxury of some wealthy families, the terrible uncertainty overshadowing many families of the poor – these evils are too plain to be ignored.” (Pigou, 1920 in Tribe, 2008, p. 464)

in Chapter 2: the analysis of the correlates of happiness (1), and the Life Satisfaction Approach for valuing public goods (2). It maintains that instead of producing subjective data as tools for government (Desrosières, 2008), economists of happiness are rather trying to find economic uses to data produced in general surveys. This is the case of the Life Satisfaction Approach, the development of which contradicts, however, part of the previous developments in the subfield.

(1) As shown in Chapter 2 (Fig. 2.5) much of the references animating the recent development of the economics of happiness consist in empirical analyses of the correlates of life satisfaction data²³. An important part of this literature seems to be a consequence of the increasing availability of the data and is thus subject to Hamermesh's (2004) critical remarks: there seems to be nothing in this literature than cannot be (or has not been) done by psychologists or sociologists. As noted by Clark *et al.* (2008), the economic analysis of subjective well-being data is largely the outcome of the availability of panel data, namely in "the British Household Panel Survey (BHPS), the German Socio-Economic Panel Study (GSOEP), and the Russian Longitudinal Monitoring Survey (RLMS)" (Clark *et al.*, 2008, p. 99)²⁴. Economists of happiness are being able to use micro data collected in panel studies to explore not only the effects of economic variables on subjective well-being, but also those of virtually every variable involved in the surveys, especially fixed traits:

²³ This is the case of the following references listed in the Appendix: "The Economics of Happiness 1999-2009": Frey and Stutzer (2000b); Dutt (2001); Namazie and Sanfey (2001); Hartley-Brewer (2002); Bjornskov (2003); Hartog and Oosterbeek (2003); Moller and Devey (2003); Blanchflower and Oswald (2004); Graham, Eggers and Sukhtankar (2004); Greene and Yoon (2004); Soydemir, Bastida and Gonzalez (2004); Chan, Miller and Tcha (2005); Dockery (2005); Mookerjee and Beron (2005); Rehdanz and Maddison (2005); Andren and Martinsson (2006); Bridges (2006); Easterlin (2006); Lelkes (2006); Dynan and Ravina, Enrichetta (2007); Bjornskov, Dreher and Fischer (2007); Oreopoulos (2007); Schimmack and Lucas (2007); Schwarze and Harpfer (2007); Abdallah, Thompson and Marks (2008); Blanchflower and Oswald (2008); Brereton Clinch, and Ferreira (2008); Kalyuzhnova and Kambhampati (2008); Luechinger, Meier, and Stutzer (2008); Thompson (2008); Welsch (2008); Welsch and Bonn (2008); Cattaneo, Galiani, Gertler, Martinez and Titiunik (2009); and Kassenboehmer and Haisken-DeNew (2009).

²⁴ These panel surveys present additional data to those available in general surveys as the World Values Survey, R. Veenhoven's World Database of Happiness, the Survey Research Center, the U.S. National Opinion Research Center, the Eurobarometer Survey, or the Satisfaction with Life Scale introduced by psychologist Ed Diener and his associates (Frey and Stutzer, 2002a, pp. 26-28).

“These panel studies allow researchers to track individuals’ income and happiness over long periods (now over 20 years in the case of the GSOEP) and to control for individual fixed traits, the latter having been shown to be crucial for the empirical modeling of subjective well-being.” (*ibid.*)

Surveys by Frey and Stutzer (2002b), and DiTella and MacCulloch (2006) suggest that instead of producing subjective data to guide or support economic policies, economists of happiness are looking for possible uses of already available data. Frey and Stutzer’s “What Can Economists Learn from Happiness Research” (2002b) is largely devoted to the analysis of the correlations of both income and unemployment with subjective well-being, and is intended to show that happiness research can “usefully inform economic policy decisions” (Frey and Stutzer, 2002b, p. 402)²⁵. However, it shows that noneconomic variables have the largest effects on life satisfaction data:

“One finding is the consistently large influence of nonfinancial variables on self-reported satisfaction. This does not mean that economic factors such as income, employment, and price stability are unimportant, but does suggest that the recent interest in issues such as good governance and social capital is well founded.” (*ibid.*, p. 403)

The large influence of “nonfinancial variables” would explain why economists of happiness are interested in broad social subjects rather than specific economic problems. It suggests that rather than producing data as tools for government, these economists are rather involved in the analysis of already available life satisfaction data and drawing conclusions about whatever variable may have a large influence on these kinds of facts.

In regards to the policy implications of the analysis of life satisfaction data, Frey and Stutzer’s arguments evidence that there is no straightforward application for the economists of happiness’ findings. Their strategy would rather consist in convincing “political entrepreneurs and citizens” about the importance of the information contained in the results of their studies:

“the insights from empirical analyses should serve mainly as an information on favorable economic and institutional conditions. If they are considered to be convincing by political

²⁵ Frey and Stutzer’s (2002b) paper consists in: “1. Why Study Happiness?” (pp. 402-403), “2. Happiness and Utility” (pp. 403-408), “3. Effects of Income on Happiness” (pp. 408-419), “4. Effects of Unemployment on Happiness” (pp. 419-422), “5. Effects of Inflation and Happiness” (p. 422), “6. Institutional Effects on Happiness” (pp. 422-426), and “7. Summary and Implications” (pp. 426-431).

entrepreneurs and citizens, they are taken up and are proposed in the political process.”
(*ibid.*, p. 427-428)

Di Tella and MacCulloch’s “Some Uses of Happiness Data in Economics” (2006) presents the analysis of happiness as an empirical application of marginal utility theory and shows that the “*potential uses* of happiness data in political economy are vast” (*ibid.*, p. 39, emphasis added)²⁶. However, these “potential uses” are also conceived as means to inform policy makers rather than as direct supports for economic policies²⁷:

“The patterns observed in the empirical measures of welfare and happiness deserve to play at least some role in the evaluation of what social goals to emphasize, what macroeconomic tradeoffs are acceptable and what public policies are pursued.” (*ibid.*, p. 44)

(2) The lack of a direct use of happiness data seems to have conditioned the strategy followed within the subfield. Besides taking subjective well-being data as proxies for utility (Clark *et al.*, 2008), the recent development of the subfield is marked by the emergence of the Life Satisfaction Approach for valuing public goods. The Life Satisfaction Approach consists in using the analysis of subjective well-being data as an alternative to “traditional” valuation methods such as contingent valuations, travel costs, or hedonic prices (Section 6.3). It was introduced by Heinz Welsch in a paper entitled “Environmental Valuation based on Happiness Surveys” (2002). In this seminal paper Welsch showed that it was possible to examine how life satisfaction data varied “with prosperity and environmental quality” (Welsch, 2002, p. 474). Welsch (2002) proceeded by analyzing correlations between the variables listed in Figure 6.6, and concluded that the use of happiness data made possible both the “direct estimation of preferences over prosperity and

²⁶ Di Tella and MacCulloch’s (2006) paper is divided in : (1) “Economic Growth Without Happiness” (pp. 26-35), (2) “Using Happiness Data to Evaluate Policy” (pp. 35-37), (3) “The Inflation-Unemployment Tradeoff” (pp. 37-39), (4) “Political Economy” (pp. 39-43), and (5) “Conclusions” (pp. 43-44).

²⁷ According to Di Tella and MacCulloch (2006), unlike traditional welfare economists, economists of happiness “simply compare measures of welfare, and what causes changes in welfare, under different scenarios” (Di Tella and MacCulloch, 2006, p. 44). They maintain that economists of happiness ultimately take the view that “happiness scores measure true internal utility with some noise” and that the noise in the available data is low enough to “make empirical research productive” (*ibid.*, p. 28). They show that economists “who believe that welfare can be measured to some extent by happiness surveys have an easier time” than economists who analyze “what people do” (*ibid.*, p. 44).

pollution” as well as the calculation of “the subjective monetary valuation of changes in nitrogen dioxide pollution” (*ibid.*, p. 478)²⁸.

Figure 6.6 (Welsch, 2002, p. 476)

List of Variables	
Variable Name	Description
<i>Happiness</i>	Average self-reported well-being
<i>Income</i>	GNP per capita
<i>Sulphur</i>	Urban sulphur dioxide pollution
<i>Nitrogen</i>	Urban nitrogen dioxide pollution
<i>Particles</i>	Urban total suspended particulate pollution
<i>Phosphorus</i>	Phosphorus pollution in water bodies
<i>Solids</i>	Suspended solids in water bodies
<i>Freedom</i>	Civil rights and liberties
<i>Rationality</i>	Scientific rationality

According to Welsch’s (2002) conclusions, the use of subjective well-being data is more suitable than contingent valuations, hedonic pricings or travel cost methods “for deriving the valuation of changes in aggregate levels of air or water pollution” (*ibid.*).

Welsch’s approach has been followed by an increasing number of studies²⁹. Of these, van Praag and Baarsma’s (2005) is worth mention for, unlike most of the other studies, these authors produced their own happiness data in an attempt to evaluate the effects of airport noise in Amsterdam³⁰. Their approach consisted in producing “an equation explaining happiness as a function of income, noise and other variables” by means of which they estimated the “change of income necessary to compensate for a specified change in noise” (van Praag and Baarsma, 2005, p. 225). Like Welsch, van Praag and Baarsma (2005) emphasized the advantages of the Life Satisfaction Approach in comparison to that of hedonic pricing or contingent valuations:

²⁸ The measurement of the first seven variables was quite conventional. Freedom was an index ranging from 1 to 7 taken from “Freedom House (2000)”; and scientific rationality was taken from the ESI database and measured “by the number of scientists and engineers per thousand population” (Welsch, 2002, p. 490).

²⁹ See van Praag and Baarsma (2005), Welsch (2006, 2007a, b), and Ferrer-i-Carbonell and Gowdy (2007). Interestingly, there has been a very recent upsurge of the use of the Life Satisfaction Approach with a substantial amount of references for 2009. See Carrol *et al.* (2009), Frey *et al.* (2009), Luechinger (2009), Luechinger and Raschky (2009), MacKerron and Mourato (2009), Welsch (2009), and Welsch and Kuhling (2009).

³⁰ The Leyden School’s approach (Chapter 1) is, however, an exception in the economics of happiness. The studies carried by van Praag and his associates since the 1970s have been largely based on own collections of subjective well-being data rather than the use of life satisfaction data produced in general surveys.

“The advantage of this extended model, compared to traditional hedonic price analysis, is that it does not assume equilibrium on the housing market as a starting point [...].The advantage of satisfaction questions compared to the CVM [contingent valuation method] approach is that the respondent is not aware or made to believe that his or her responses may have any influence on decisions or compensations in which he or she has an interest. Hence, strategic response behaviour is highly unlikely in our study.” (*ibid.*, p. 244)³¹

Welsch’s “Implications of Happiness Research for Environmental Economics” (2009) gives a comprehensive survey of the current state of the Life Satisfaction Approach. The approach is introduced in terms of utility functions and consists in using life satisfaction data as a means to calculate trade-offs between incomes and environmental variables:

“Using happiness data for environmental valuation constitutes a novel and potentially effective approach to assessing the monetary value of environmental amenities. By correlating people’s reported subjective well-being with both, environmental amenities and income, it permits to identify the utility function over environmental quality and income directly and to estimate the implied utility-constant trade-off between them.” (Welsch, 2009, p. 2739)

For Welsch, the “remarkably simple” idea supporting the Life Satisfaction Approach is that the individuals’ happiness can be modeled “as a function of their incomes and the prevailing environmental conditions, controlling for demographic and other relevant circumstances” (*ibid.*, p. 2740). It is clear from this explanation that the Life Satisfaction Approach uses life satisfaction data as a measure of utility, and that it is based on the assumption that happiness is positively correlated with income. Consequently, the Life

³¹ Luechinger and Raschky (2009) advance the comparison between these methods in the following terms: “In economics, essentially two avenues have been pursued to elicit people’s preferences for public goods: *revealed preference* methods on the one hand and *stated preference* methods on the other hand. The former are based on actual behavior and utilize complementary and substitutive relationships between public and various marketed goods to infer the value attributed to public goods from market transactions in private goods. A primary example is the hedonic method (HM). In the case of stated preference methods, individuals are directly asked to value the public good in question; the most prevalent method is the contingent valuation method (CVM). Both avenues have their weaknesses. Revealed preference methods are based on stringent assumptions and crucial elements are inherently difficult to measure. The hypothetical nature of CVM surveys, on the other hand, may entail unreliable results and strategic behavior [...]. [The LSA] is not based on observed behavior, the underlying assumptions are less restrictive and non-use values can – to some extent – be measured. Furthermore, individuals are not asked to value the public good directly, but to evaluate their general life satisfaction. This is presumably a cognitively less demanding task, does not evoke answers considered desirable by the persons asked, and there is no reason to expect strategic behavior.” (Luechinger and Raschky, 2009, p. 621)

Satisfaction Approach leaves no space for hedonic adaptations or social comparisons. This marks a strong contrast with the earlier developments in the subfield (Chapter 2).

Another interesting thing to point out is that the economists involved in the development of the Life Satisfaction Approach present the life-satisfaction data as measures of experienced utilities. In Welsh's terms:

“The estimated relationship is interpreted as an experienced utility function and used to calculate the trade-off people would be willing to make between income and environmental conditions [...]” (Welsch, 2009, p. 2740)³²

The Life Satisfaction Approach seems to overlook the hedonic psychologists' distinction between immediate and recalled evaluations of subjective well-being. Kahneman and Sugden's (2005) “Experienced Utility as a Standard of Policy Evaluation” is a clear statement of the psychologist's position. It deals with the use of the Life Satisfaction Approach as conducted by van Praag and Baarsma (2005) and, supporting the main findings of hedonic psychologists, shows that life satisfaction data should not be considered as measures of experienced utilities. They present the Life Satisfaction Approach in the following terms:

“This strategy takes an individual's reported happiness or subjective well-being as a measure of his current experienced utility. We suggest that this is not a reliable measure. One problem is that people don't really know how happy they are. In order to answer these kinds of question, they perform all sorts of computations that have nothing to do with experienced utility. Further, general questions about satisfaction with your life as a whole, or with major aspects of your life such as your job or your marriage, are liable to prompt normative responses [...]” (Kahneman and Sugden, 2005, p. 174)

Kahneman and Sugden maintain that the “statistical difficulty of identifying the relationship between overall satisfaction and specific causal factors” undermines the use of life satisfaction data as tools to “guide public policy” (Kahneman and Sugden, 2005, p. 174). This is, indeed, one of the main

³² In Luechinger and Raschky's (2009) words: “A growing literature in economics demonstrates that reported subjective well-being can serve as an empirically adequate and valid approximation for individually experienced welfare or utility. Hence, it is an obvious and straightforward strategy to directly evaluate public goods in utility terms. The partial correlations between the public good and life satisfaction and between income and life satisfaction capture the marginal utility of the public good and the marginal utility of income, respectively. This allows for estimating the trade-off ratio between income and natural hazards.” (Luechinger and Raschky, 2009, p. 621)

reasons why psychologists have developed “Experience Sampling” and “Day Reconstruction” methods for measuring “objective happiness” as presented in Chapter 2.

However, despite Kahneman and Sugden’s (2005) warnings, as well as hundreds of references by hedonic psychologists, defenders of the Life Satisfaction Approach such as Frey, Luechinger, and Stutzer (2009) still claim that life satisfaction data can serve as “adequate and valid” approximations for “experienced utility”:

“Due to extensive work by numerous psychologists spanning many decades, the great progress made in the measurement of experienced utility has gained recognition. Thus, reported subjective well-being can serve as an empirically adequate and valid approximation for individually experienced welfare. Hence, it is an obvious and straightforward strategy to directly evaluate public goods in experienced utility terms.”
(Frey *et al.*, 2009, p. 319)

Besides the fact that this is *not* what psychologists say, it is interesting to point out that these statements also contradict these economists of happiness’ own previous claims about the relationship between life satisfaction measures and measures of experienced utility or “objective happiness” (Frey and Stutzer, 2002b, p. 30; Chapter 2).

Does anything go with happiness?

Conclusion to Part III: Subjective Quantification in Economics

Part III was intended as a first step into the history of the economic use of subjective data and began by exploring the main events presented in the current versions of this history. Instead of taking the economists’ skepticism of the use of subjective data as the effect of “unfounded preconceptions” (Introduction), Chapter 5 explored the measurement without theory and the full-cost controversies, and maintained that George Katona’s attempt to develop a program of “economic psychology” had an influence in these two episodes that has been overlooked in the available literature on these subjects. The chapter then proceeded to the historical analysis of Katona’s program which was marked by the debates on the use of subjective data on

expectations happening between the Federal Reserve, the Survey Research Center, and the NBER. It showed that the analysis of subjective data developed by Katona under the labels of “economic psychology” first and then “behavioral economics” failed as it implied the development of a new economic theory rather than the use of the data as forecasting tools.

The first sections of Chapter 6 maintained that Katona’s program of behavioral economics was overshadowed by the production and analysis of the Index of Consumer Sentiment which is constructed out of the attitudinal data collected by the Survey Research Center. This index is nowadays widely used by both government agencies and businessmen as a means to predict trends in consumer behavior which are used to anticipate future levels of aggregate variables. The rest of the chapter showed that the analysis of subjective data on expectations as well as the use of contingent valuations have been both produced and used by economists as tools for government in order to deal with problems such as the prediction of macroeconomic aggregates or the monetary evaluation of environmental damages. It presented A. Desrosières’ approach to the history of statistics and concluded that the lack of economic application for the analysis of happiness data emerges as an apparent weakness of the subfield. It maintained that the development of the Life Satisfaction Approach for valuing environmental goods can be explained as a reaction to this issue.

GENERAL CONCLUSION

The Life-Satisfaction Approach for valuing environmental goods is the most recent development in the economics of happiness. It is based on the use of happiness data as proxies for utility and seems to contradict much of the early developments in the subfield. As we have seen, there have been different uses of happiness data by economists during the years that are covered in this dissertation (1974-2009). During the early development of the subfield, the happiness paradox was used as a piece of evidence to support a discourse about the effects of affluence on both consumer behavior and welfare. Social comparisons, adaptations and aspirations were advanced as theoretical principles and used to counter the basic postulates of consumer choice theory.

With the turn of the century, economists like Easterlin (2002) turned their sights towards the development of hedonic psychology. Rather than criticizing economic theories of consumer behavior and welfare, economists of happiness began to demonstrate that demographic variables had important effects on subjective well-being, as in the case of unemployment. The happiness paradox argument also experienced a dramatic change. Introduced by Easterlin in 1974, the paradox first showed that rich countries were not happier than poor ones and that they did not become happier as they became richer. Recent developments in the subfield are marked by new interpretations of the data. Money supposedly buys happiness, and a new strategy has been adopted by economists of happiness to explain this phenomenon. Following the “new” behavioral economists, economists of happiness present the empirical evidence as complements to choice theory.

The dissertation began by presenting two normative reconstructions of the history of the economics of happiness and a standard reconstruction about the

history of the relationship between psychology and economics. It concludes by revisiting these three accounts.

(1) Is happiness “back”; is it “a revolution in economics”?

The history of happiness *in* economics promoted by Bruni and Porta (2005, 2007) presents the economics of happiness as a rediscovery of happiness echoing the classic analysis of the wealth-happiness nexus. The “Return to Happiness in Economics” (Bruni and Porta, 2005, p. 1) is supposed to be marked by the contributions of Richard Easterlin (1974), and Tibor Scitovsky (1976). However, according to Bruni and Porta, the return of happiness to economics is limited as it involves a “hedonic” rather than a “eudaimonic” conception of happiness (Introduction). These reconstructions are focused on the history of the concept of happiness.

Chapter 1 of the dissertation showed that rather than rediscovering classical studies of happiness, Easterlin (1974) and Scitovsky (1976) were involved in a broader movement which consisted in the analysis of the welfare effects of affluence. It maintained that the economic analysis of affluence was not only meant to contest economic theories, but also to be used as a support for advancing policy recommendations with regards to the formation of consumers’ preferences. Chapter 2 presented the recent development of the economics of happiness and showed that the use of subjective well-being data has not been uniform during the short history of the subfield. As noted above, the current strategy of economists involved in the analysis of happiness consists in maintaining that money does buy happiness. They indeed analyze tradeoffs between income and happiness to evaluate changes in third variables in utility functions. Rather than being “back” and echoing the analysis of classical economists, the economists’ happiness (i.e. life satisfaction data) has been used as an alternative to the moment-based measures of hedonic psychologists. Based on the elements presented in Part I of the dissertation it seems safe to claim that recent economists of happiness are far from being revolutionary. The history of the economics of happiness presented in Part I of the dissertation was then used as a viewpoint to explore both the history of economics and experimental psychology (Part II), and the history of the economic use of subjective data (Part III).

(2) Is psychology “back”?

Part II of the dissertation explored the history of the relationship between economics and experimental psychology. Instead of analyzing the history of the two disciplines from the viewpoint of mainstream economics and asking whether psychology has been “in” or “out” of it, it explored this history from the viewpoint of the historical development of the economics of happiness, claiming that there are always psychological assumptions involved in economic accounts of behavior. Beginning with the analysis of the “economic psychologies” of Jevons and Edgeworth, Part II claimed that there is a history of ambiguous relationships between economics and psychology. It showed that these marginalists made a peculiar use of the experimental analysis of physiological psychologists, and that “behaviorist mainstream economics”, as developed by ordinalist economists, was not really behaviorist, for it still involved mentalism, rejecting theories of behavior control. Similarly, the mainstream economists’ reception of von Neumann and Morgenstern’s expected utility theory was ambivalent: it dismissed the “cyborg” content of von Neumann’s information-processing approach and was based instead on assumptions of rationality as consistency *à la* Nash.

Part II concluded that “new” behavioral economists adopt a similar strategy. Their program is based on standard rationality assumptions, and is complemented with insights from both cognitive psychology and psychological accounts of perception such as Adaptation-Level Theory. Concerning the long history of the relationship between economics and psychology, Part II showed that there is more than one “psychology” involved in it. The history of psychology as being “in” or “out” of economics overlooks the complexity of the relationship between the two disciplines.

(3) Is there more than an “unfounded skepticism” towards the use of subjective data?

Finally, Part III of the dissertation explored the history of the economic use of subjective data and introduced each of the main elements advanced in the current literature as an explanation of the subject. Chapter 5 presented George Katona’s attempt to develop a program of behavioral economics out of the analysis of subjective data gathered by means of detailed interviews. It showed that Katona’s theoretical program failed, as it was judged in terms of

the predictive power of its components rather than as a means for furthering the economic analysis of consumer behavior. Chapter 6 showed that the analysis of subjective data has been rejected as a means to test economic theories or develop theoretical research. It has rather been developed as part of applied programs such as the construction of the Index of Consumer Sentiment by the Survey Research Center of the University of Michigan, or the Survey of Economic Expectations conducted at the University of Wisconsin Survey Center. It maintained that the lack of straightforward applications to the economic analysis of happiness data is one of the apparent weaknesses of the economics of happiness. This partially explains the recent move attempted by economists who advance the Life Satisfaction Approach as a means for valuing environmental goods. The history of the economic use of survey data presented in Part III of the dissertation was developed from the viewpoint of the historical development of the economics of happiness.

Appendix: The Economics of Happiness 1999-2009

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Résumé : Étude des relations historiques entre analyse économique et analyse psychologique du point de vue de « l'économie du bonheur »

INTRODUCTION

Section 0.1. Objets et méthodologie de la thèse

Cette thèse traite de l'histoire des relations entre l'analyse économique et l'analyse psychologique du point de vue de « l'économie du bonheur » (*the economics of happiness*). Elle comporte trois parties. La première (Partie I. L'Économie du bonheur) distingue deux périodes du développement historique de la sous-discipline : depuis la première analyse économique des données de bonheur (1974) jusqu'à la fin du 20^e siècle et, ensuite, depuis 1999. Les deux autres parties prennent cette histoire comme point de départ pour l'analyse de deux questions : l'histoire des relations entre l'analyse économique et la psychologie expérimentale (Partie II. Analyse économique et psychologie expérimentale), et l'histoire de l'usage économique des données subjectives (Partie III. L'Analyse économique des données subjectives).

Section 0.2. La thèse par rapport à l'histoire de la pensée économique

La thèse s'inscrit dans trois types d'analyses historiques : l'histoire du concept de bonheur dans l'analyse économique (Section 0.2.1), l'histoire des fondements psychologiques de l'analyse économique (Section 0.2.2) et l'histoire de l'usage économique des données subjectives (Section 0.2.3).

0.2.1. L'histoire du concept de bonheur dans la pensée économique

L'« histoire du concept de bonheur dans l'analyse économique » (*happiness in economics*) est présentée dans la thèse à partir des publications de Luigino Bruni et Pier-Luigi Porta (Bruni (2004a, 2004b, 2006), Bruni et Porta (2007)). Celles-ci portent davantage sur l'histoire longue de la notion de bonheur dans

l'analyse économique, que sur le développement récent de l'économie du bonheur. L'objectif principal de ces auteurs est de clarifier le sens de la notion de bonheur tel qu'elle se présente dans l'histoire de l'analyse économique. La thèse présente leurs travaux en reprenant le point de vue de Bruni (2004a, b) qui affirme qu'il y a plusieurs histoires du bonheur dans l'économie.

La première histoire est celle des approches économiques classiques (Bruni, 2004a). D'un côté Adam Smith [1723-1790] et la tradition anglaise, centrent leurs recherches sur les causes de la richesse des nations. De l'autre, les traditions française et italienne ont pour objet la recherche de la « félicité publique » (*felicità pubblica* ou *public happiness*). Cette deuxième approche est présentée principalement à travers l'œuvre du prêtre napolitain Antonio Genovesi [1713-1769] (*ibid.*, pp. 25-28). Comme « Smith le philosophe » (l'auteur de la *Théorie des Sentiments Moraux*), Genovesi aurait professé la philosophie morale en s'intéressant à la valeur des relations interpersonnelles. Mais à la différence de « Smith l'économiste », l'avantage principal de la vie en société n'aurait pas résidé, pour Genovesi, dans la production de biens matériels, mais dans la jouissance des relations sociales. Toutefois, tant Smith que Genovesi, auraient tous deux produit des théories en continuité avec la notion de bonheur de la tradition aristotélicienne : l'*eudaimonia*.

Or, d'après Bruni, certains des principaux auteurs de l'économie classique anglaise (notamment Smith) se seraient détachés de l'analyse du bonheur pour se consacrer à l'analyse de la richesse (Bruni, 2004a, p. 23). La *Richesse des Nations* serait d'ailleurs porteuse d'un changement radical de l'objet de l'analyse anglaise :

« L'économie politique de Smith (et l'ensemble du paradigme classique), aurait pu devenir quelque chose de complètement différent : au lieu d'être définie comme la science de la richesse, l'économie politique aurait pu être définie comme l'étude de comment et sous quelles conditions les richesses peuvent être transformées en bonheur. » (*ibid.*, p. 32)

La seconde histoire du bonheur dans l'économie est présentée à partir de l'interprétation des travaux de Jeremy Bentham [1748-1832] et de l'influence de ces travaux sur l'économie politique proposée par Bruni (2004a, p. 35). Cette histoire du bonheur se traduit dans l'analyse économique par l'abandon de ce que Bruni appelle le problème de la transformation des richesses en bonheur. Selon Bruni, à la différence des classiques latins, la notion de bonheur retenue par Bentham est purement identifiée aux plaisirs et aux

peines. Il s'agit, ainsi, d'un véritable point de rupture dans l'histoire du bonheur dans l'analyse économique. Ce sont, ensuite, Jevons [1835-1882] et Edgeworth [1845-1926] qui, en suivant les idées de Bentham, transplantent la philosophie utilitariste/hédoniste à l'analyse économique :

« Le réductionnisme du bonheur-*eudaimonia* à l'utilité-plaisir est le vrai point de rupture dans l'histoire du bonheur dans la science économique. La distinction entre les conditions matérielles et le bonheur a été perdue. » (*ibid.*, p. 36)

D'après Bruni, l'approche hédoniste aurait été ensuite abandonnée en raison du tournant initié par W. Pareto [1848-1923], ce qui marque le point de rupture entre l'économie et la psychologie :

« Du point de vue de l'économie du comportement, le « tournant de Pareto » dans la théorie du choix rationnel est le point où la science économique a pris le mauvais chemin. Après avoir commencé avec une théorie psychologique et ayant constaté que cette théorie manquait d'appui, les économistes (selon cette école) auraient dû rechercher de meilleures bases psychologiques [...] au lieu de changer l'interprétation de ces bases. » (*ibid.*, p. 38)

Alors que Jevons et Edgeworth voulaient fonder l'économie sur la psychologie expérimentale de l'époque, Pareto, dans sa reformulation, a abandonné toute recherche expérimentale sur les fondements psychologiques du comportement humain :

« On peut commencer à partir des courbes d'indifférence, qui sont un résultat direct de l'expérience. » (Pareto, cité par Bruni, 2004a, p. 38)

Dans l'histoire reconstruite par Bruni, le concept de bonheur des économistes du bonheur s'inscrit dans une continuité avec l'approche hédoniste mais est en rupture avec la notion de bonheur classique, héritière de l'*eudaimonia* des traditions antiques :

« L'analyse historique montre que la discussion contemporaine sur le bonheur est un retour à la ligne classique (le bonheur n'est pas la richesse), mais le bonheur demeure individualiste et hédoniste : l'*eudaimonia* et la *publica felicità* sont les grands absents ici. » (Bruni, 2004a, p. 40)

L'histoire du concept de bonheur devient normative (*ibid.*). Bruni, Porta et leurs disciples préconisent de rétablir certains éléments de l'analyse du bonheur classique dans l'analyse contemporaine du bonheur en économie.

0.2.2. *L'histoire des fondements psychologiques de la pensée économique*

L'histoire du bonheur dans la pensée économique (0.2.1) est fortement influencée par l'analyse historique des relations entre l'économie et la psychologie. La section 0.2.2 de la thèse introduit l'histoire « officielle » de ces relations selon laquelle l'analyse économique est imprégnée de psychologie jusqu'au tournant ordinaliste, puis que cette dernière s'éloigne des préoccupations des économistes pour revenir avec le développement récent de l'économie comportementale (Hands, 2009). Cette analyse considère que le tournant ordinaliste dans la théorie du choix rationnel a été un mouvement béhavioriste :

« Au moment où les économistes essayaient de libérer l'analyse économique d'éléments psychologiques, un mouvement béhavioriste est apparu. Ce mouvement a contribué au remplacement de l'ancienne théorie de l'utilité cardinale par la nouvelle notion des préférences ordinales. » (Lewin, 1996, pp. 1294-1295, et beaucoup d'autres économistes et historiens de la pensée économique)

La Partie II de la thèse étudie l'histoire des relations entre l'analyse économique et la psychologie expérimentale (notamment le béhaviorisme) et se positionne contre l'analyse dominante des relations entre ces deux disciplines.

0.2.3. *L'histoire de l'analyse économique des données subjectives*

À la différence de l'histoire du bonheur dans l'analyse économique de Bruni et Porta (Section 0.2.1), l'histoire reconstruite par Easterlin (2004) aborde le développement de l'économie du bonheur à partir du problème de la quantification dans l'analyse économique¹. Easterlin (2004) montre que l'analyse des données de bien-être subjectif a été exclue de l'analyse économique alors que la théorie contient explicitement des références à ce concept (Easterlin, 2004, pp. 21-22).

D'après l'analyse d'Easterlin (2004), ou encore celles de McCloskey (1983) et de Manski (2004), c'est la connaissance superficielle de débats célèbres sur les « coûts totaux » (*the full-cost controversy*) ou l'analyse d'anticipations économiques (*the economic expectations controversy*) qui sont à la base du

¹ C'est aussi le cas pour d'autres économistes investis dans l'analyse des données subjectives comme Hanemann (1994) et Manski (2004).

« scepticisme non fondé » des économistes à l'égard des données subjectives. La troisième partie de la thèse, qui prend ces débats pour point de départ, souligne la différence entre l'analyse des données subjectives en tant qu'« outils scientifiques » et en tant qu'« outils de gouvernement ». Elle enrichit ainsi l'analyse historique de l'usage économique de ce type de données.

Sections 0.3 et 0.4. Plan de la thèse

La structure de ce résumé reprend le plan de la thèse en trois parties, chacune composée de deux chapitres. La première partie porte sur le développement historique de l'économie du bonheur et s'oppose aux reconstructions proposées par Bruni et Porta (section 0.2.2). Les deux autres parties prennent l'histoire développée dans la première partie comme point de départ pour l'analyse de deux autres histoires : l'histoire des liens entre analyse économique et psychologie expérimentale (Partie II), et l'histoire de l'usage économique des données subjectives (Partie III).

La deuxième partie de la thèse s'appuie sur l'histoire de l'économie du bonheur pour étudier l'histoire des relations entre l'analyse économique et la psychologie expérimentale. Elle compare deux visions alternatives du comportement du consommateur : le consommateur comme agent rationnel qui répond à ses préférences, et le consommateur influencé par son entourage socio-économique ainsi que les pratiques des producteurs.

La troisième partie de la thèse étudie l'histoire de l'usage économique de données subjectives. Elle est centrée sur le développement croissant d'analyses économiques de données d'anticipations et d'évaluations contingentes et montre que ces analyses ont été adoptées comme outils de gouvernement, plutôt que comme outils scientifiques. Cette évolution explique les derniers développements de l'économie du bonheur qui consistent à utiliser des données de bien-être subjectif comme instruments d'évaluation de politiques environnementales.

PARTIE I : L'ÉCONOMIE DU BONHEUR

La première partie de la thèse comporte deux chapitres : le premier, (Chapitre 1) porte sur le développement initial de l'économie du bonheur (1974-1999), et le second (Chapitre 2) sur son développement récent (1999-2009).

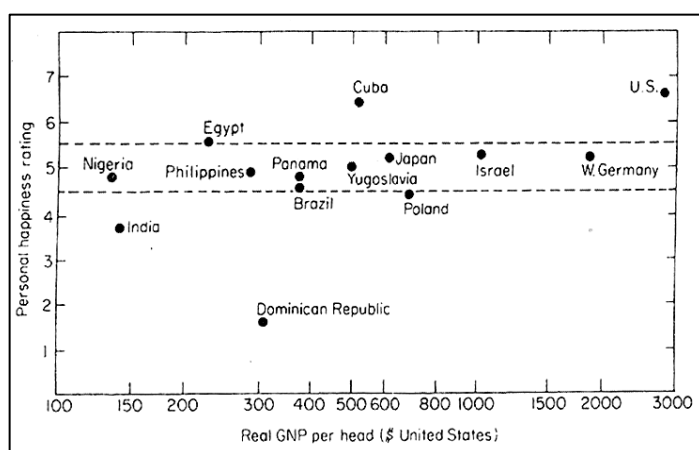
Chapitre 1 : Du bien-être au bonheur (1974-1999)

La publication, par Richard Easterlin, de l'article « *Does Economic Growth Improve the Human Lot ?* » (1974), ouvre la voie à l'analyse économique des données de bonheur. Ensuite, et ce jusqu'à la fin du 20^e siècle, l'économie du bonheur se développe face aux difficultés de la théorie du choix rationnel, ainsi que de l'économie du bien-être à rendre compte des effets de l'abondance économique sur le bien-être des consommateurs (notamment aux États-Unis).

Sections 1.1 et 1.2. La découverte des données de bonheur : Easterlin (1974)

La première analyse économique des données de bonheur (Easterlin, 1974) examine, tout d'abord, un des postulats majeurs de l'économie du bien-être, à savoir, l'effet du développement économique sur le bien-être social. C'est lors de cette présentation qu'Easterlin fait émerger le paradoxe du bonheur (devenu récemment le paradoxe d'Easterlin). Les résultats paradoxaux sont au nombre de trois. Le premier montre des corrélations positives entre les données individuelles du bien-être subjectif et les données de revenu (Easterlin, 1974, pp. 99-104). Les deux autres présentent, au contraire, des faibles corrélations entre les données du bien-être subjectif et celles du revenu par pays (moyennes), ainsi que des faibles corrélations pour les séries temporelles des données du bien-être subjectif et du revenu pour les États-Unis (Easterlin, 1974, p. 111). Easterlin fournit la représentation suivante de l'analyse des corrélations du bien-être subjectif et du revenu par pays.

Figure 1.1 (Easterlin, 1974, p. 106)



Dans la Figure 1.1, les limites tracées par les lignes pointillées, sont censées montrer l'absence de rapport entre les données du bien-être subjectif et les données du PNB.

Quant à l'interprétation de ces « faits », Easterlin part des thèses de J. Duesenberry (1949) et de son concept de « revenu relatif » pour expliquer que la satisfaction d'un individu à l'égard de son niveau de consommation n'est pas une fonction du niveau absolu de sa dépense, mais du rapport entre sa dépense et celle des autres individus (Easterlin, 1974, p. 112). L'auteur soutient cette thèse grâce aux enquêtes provenant d'autres disciplines. Il cite ainsi *The Patterns of Human Concerns* (1965) du psychologue Hadley Cantril pour montrer que les normes de consommation varient directement avec le niveau de développement économique des pays. D'après Easterlin, les standards de comparaison s'ajustent à la hausse au fur et à mesure que les revenus augmentent. Cet ajustement éliminerait l'effet de la hausse du revenu sur le sentiment de bien-être des individus (Easterlin, 1974, p. 116). Tout en critiquant l'hypothèse de la stabilité des préférences, Easterlin se demande s'il n'est pas temps pour les économistes de se tourner davantage vers l'analyse psychologique (Easterlin, 1974, pp. 117-118).

Section 1.3. Psychologie du bien-être : Scitovsky (1976)

Deux ans plus tard (1976) paraît *The Joyless Economy* de Tibor Scitovsky, ouvrage destiné à tisser des liens entre l'analyse psychologique et l'analyse économique afin de proposer une approche alternative à la question du bien-

être économique. Tout comme Easterlin, Scitovsky revient sur l'hypothèse de la stabilité des préférences.

D'après Scitovsky, l'analyse économique repose sur l'idée qu'un agent se trouve dans une situation optimale lorsqu'il égalise la satisfaction marginale de ses différents besoins, bien qu'ils restent insatisfaits en niveau absolu (Scitovsky, 1976, p. 65). À l'analyse économique, Scitovsky oppose la vision psychologique du comportement stimulus-réponse, qui est le résultat d'analyses expérimentales menées en grande partie sur des animaux. D'après ces recherches, la satisfaction des besoins serait intermittente mais complète. Scitovsky considère le point de satiété comme un état de confort incompatible avec le sentiment de plaisir. En effet, il s'agit pour l'auteur d'un état caractérisé par l'absence de plaisirs et de peines. L'individu se trouverait ainsi confronté à un choix entre confort et plaisir qui est absent de la théorie économique du choix rationnel.

Le but de Scitovsky étant d'enrichir le cadre d'analyse de la théorie économique, il propose de rendre compte du comportement des agents économiques de manière aussi riche que l'approche psychologique le permet (*ibid.* p. 78). Or, malgré le caractère psychologique de la théorie du comportement de Scitovsky, son objectif reste de proposer des principes de politique économique. Les principes suggérés par Scitovsky ne visent pourtant pas la disponibilité des ressources mais plutôt les préférences des agents économiques, car, pour lui, le consommateur américain favorise de façon disproportionnée la dépense en biens de confort. Le problème de *L'Économie sans Joie* est ainsi un problème d'allocation des ressources, conséquence directe des structures de préférence des consommateurs américains.

Section 1.4. L'abondance économique et l'obsolescence de l'économie du bien-être

Les publications d'Easterlin (1974) et de Scitovsky (1976) sont à présent largement reconnues comme les points de départ de l'économie du bonheur. La section 1.4 de la thèse montre que, loin d'être des projets de recherche isolés (que des auteurs comme Bruni et Porta (2007) relient à l'analyse du bonheur classique), ces travaux font partie d'un mouvement bien plus vaste : « l'économie de l'abondance » (*the economics of affluence*).

La problématique de l'abondance a été introduite et développée par J. K. Galbraith dans *L'Ère de l'Opulence (The Affluent Society)*, 1958). D'après l'auteur, l'analyse économique devient obsolète suite au développement économique des sociétés occidentales (Galbraith, 1958, p. 3). Pour ce type de sociétés, écrit Galbraith, il est incorrecte de maintenir des recommandations de politique économique conçues pour affronter des problèmes d'insuffisance des ressources (Galbraith, 1958, p. 4). Or, malgré la renommée de l'ouvrage de Galbraith et de la littérature sur l'abondance économique, l'analyse historique de l'économie du bonheur (notamment celle de Bruni et Porta, 2007) retient principalement les contributions de Fred Hirsch (1977) et de Robert Frank (1985, 1989, 1997).

L'ouvrage de Hirsch introduit le concept de biens positionnels dans l'analyse économique. Avec l'abondance économique, la consommation des sociétés occidentales s'étend à des biens satisfaisant des besoins de positionnement social (Hirsch, 1977, p. 2). Ce phénomène ébranle la capacité de la croissance économique à résoudre le problème de satisfaction des besoins (d'où le titre de son ouvrage: *The Social Limits to Growth*). Dans *Choosing the Right Pond* (1985), Robert Frank prend pour point de départ la problématique des biens positionnels de Hirsch (Frank, 1985, p. 7). De là il passe à l'analyse de l'influence des contextes de référence (*frames of reference*) sur le sentiment de bien-être des consommateurs. Frank montre qu'il vaut mieux être positionné haut dans une économie modeste que bas dans une société riche. Quelques années plus tard, dans son article « *Frames of Reference and the Quality of Life* » (1989), Frank appuie son discours avec des éléments d'analyse psychologique. Tout comme Scitovsky (1976), Frank analyse le bien-être économique à partir des processus physiologiques (stimulus-réponse) d'adaptation des consommateurs à des contextes variables. De même, Frank (1989, p. 80) fait référence à la théorie psychologique du niveau d'adaptation (Chapitre 4).

Section 1.5. Économie du bonheur et politique économique : Layard (1980)

Alors que les économistes du bien-être postulaient des principes de politique économique visant à l'augmentation de la disponibilité des ressources, le discours sur l'abondance économique montre qu'il n'y a pas forcément de causalité directe entre disponibilité des ressources et satisfaction des besoins.

Le discours issu de l'analyse des données de bonheur se retrouve dans les recommandations des premiers économistes du bonheur, notamment celles de Layard (1980, p. 737), présentées dans la section 1.5 de la thèse.

L'analyse de Layard (1980) est construite avec des outils d'analyse économique traditionnelle, à travers, notamment, du maniement de fonctions d'utilité. D'après Layard, les politiques économiques devraient se focaliser sur les fonctions d'utilité des consommateurs de manière à corriger deux types de problèmes : la quête de positionnement social (*the pursuit of status*), et le rôle des anticipations dans la constitution des préférences (*the role of expected income and expected status*). Les interventions proposées par Layard commencent, en premier lieu, par l'imposition (Layard, 1980, p. 738). Par la suite, Layard envisage des changements plus profonds, notamment, dans l'organisation institutionnelle de la société (Layard, 1980, p. 742). Afin d'encourager des comportements économiques favorables au bien-être social, Layard propose également des interventions directes sur la structure des préférences individuelles (*changing human nature*) (Layard, 1980, p. 745).

En ce qui concerne le rôle des anticipations dans la formation des préférences, les suggestions de Layard restent insolites, notamment à l'égard des anticipations de revenu (Layard, 1980, p. 745). Étant donné l'effet négatif de ces anticipations sur le bien-être, Layard propose des politiques visant à les réduire. Enfin, parce que l'effet des pertes est plus fort que celui des gains sur le sentiment de bien-être, il se positionne contre toute redistribution injustifiée. Layard est, par conséquent, favorable au maintien des positionnements relatifs, tout changement étant nocif pour le sentiment de bonheur de la communauté dans son ensemble (Layard, 1980, p. 748).

Section 1.6. Conclusion : l'économie du bonheur (1974-1999)

Le chapitre 1 de la thèse se conclue en montrant que l'analyse économique des données de bonheur de la première période (1974-1999) est discrète par rapport au développement récent de la sous-discipline. Il présente les principaux traits des premières analyses à travers d'un débat sur l'économie du bonheur, paru dans *The Economic Journal* sous le titre *Controversy : Economics and Happiness*. Ce débat est composé de trois articles : Oswald (1997), Frank (1997) et Ng (1997).

Le premier de ces articles (Oswald, 1997) se concentre sur l'importance de l'élargissement de la base de données utilisée couramment par l'analyse économique. L'article relève de l'analyse de données subjectives et recommande fortement l'incorporation des données du bonheur aux programmes gouvernementaux de quantification (Oswald, 1997, p. 1815). Oswald présente les résultats les plus remarquables de l'analyse des données de la période (pp. 1818-1825) et en avance deux principales découvertes, à savoir, le faible impact des variations de revenu sur les données du bonheur (le paradoxe du bonheur), et le remarquable effet négatif du chômage sur le sentiment de bien-être des individus.

La deuxième contribution (Frank, 1997) est centrée sur les implications du paradoxe du bonheur en termes des politiques économiques. Le problème relève, d'après Frank, de la méconnaissance de la part des consommateurs des phénomènes d'adaptation (Frank, 1997, p. 1839). Le caractère relatif de la consommation mettrait en conflit les objectifs individuels et sociaux. La correction de ce dysfonctionnement du marché passerait par l'établissement d'une taxe à la consommation, ainsi que des mesures (proches de celles proposées par Layard (1980)) visant à agir sur les préférences des consommateurs (Frank, 1997, pp. 1841-1842).

Enfin, l'article de Ng (1997) présente le problème du point de vue d'un économiste du bien-être. Pour Ng, l'économie du bien-être ne parvient pas à aborder l'analyse du bonheur car elle évite l'usage des comparaisons interpersonnelles d'utilités cardinales. Ces comparaisons auraient été bannies de la nouvelle économie du bien-être alors qu'elles sont indispensables pour évaluer le bien-être social (Ng, 1997, p. 1855). Ng propose donc l'usage des données de bonheur en tant qu'utilités cardinales.

Chapitre 2 : L'économie du bonheur (1999-2009)

Le chapitre 2 de la thèse montre que l'économie du bonheur s'est renouvelée depuis la fin du 20^e siècle. Elle a pris de l'ampleur se positionnant non pas par rapport à l'économie du bien-être ou la théorie du choix rationnel, mais par rapport à la psychologie du bonheur (*hedonic psychology*). C'est dans ce contexte que Richard Easterlin publie *Happiness in Economics* (2002), recueil des principales contributions à l'économie du bonheur, dans lequel il annonce également sa position contre l'absence d'analyses économiques de données subjectives (Section 0.2.3).

Sections 2.1 et 2.2. La psychologie du bonheur (*hedonic psychology*)

Depuis la fin du 20^e siècle, l'économie du bonheur se construit par rapport à l'approche des psychologues, qui est le résultat d'études initiées dans les années 1930¹. Ces travaux portent dans une grande mesure sur la production de données de bien-être subjectif et sont présentés dans la thèse à travers des bilans de la discipline faits par Wilson (1967), Diener (1984), et Diener *et al.* (1999).

En 1967, W. Wilson fait le point sur plus de trente ans d'étude de données de bien-être subjectif (1930-1967), qui suggèrent que « le bien-être subjectif peut être déterminé de manière fiable » (Wilson, 1967, p. 294). Les recherches présentées par Wilson correspondent pour la plupart à l'analyse de corrélations entre évaluations subjectives de bien-être (des données de bien-être subjectif) et des évaluations alternatives visant à évaluer la fiabilité des méthodes de mesure subjectives.

Les années suivantes (1967-1984) ont été très prolifiques du côté de la psychologie du bonheur avec « plus de 700 études publiées » (Diener, 1984, p. 542). Or, bien que la plupart des conclusions de Wilson (1967) soient restées valides, Diener (1984) souligne des résultats qui montrent des faibles corrélations entre les données de bien-être subjectif et les variables

¹ Le premier article cité par Wilson est celui de Watson (1930), puis l'institutionnalisation de la psychologie du bonheur se fait en 1999 par l'ouvrage collectif *Well-Being : The Foundations of Hedonic Psychology* édité par D. Kahneman, E. Diener et N. Schwarz. Cet ouvrage fondateur définit la psychologie du bonheur comme l'analyse des facteurs qui déterminent le sentiment de bien-être humain (Kahneman, Diener et Schwarz, 1999, p. ix).

démographiques. L'analyse psychologique du bonheur se serait ensuite tournée vers l'analyse d'autres facteurs capables d'expliquer le bien-être subjectif (notamment l'analyse des traits de personnalité).

Le développement plus récent de la discipline (jusqu'en 1999, date de son institutionnalisation), est présentée dans la thèse à travers de l'article de Diener *et al.* (1999). Cet article insiste sur l'effet limité des variables démographiques sur les données de bien-être subjectif et montre que les nouvelles études désagrègent le concept de bien-être subjectif en plusieurs entités indépendantes (Diener *et al.*, 1999, pp. 276). Il montre aussi qu'un des objectifs prioritaires de la nouvelle discipline est de développer des mesures immédiates d'évaluation d'expériences hédoniques.

Section 2.3. Le bonheur des psychologues (au delà des données d'enquête)

La section 2.3 montre que dans « *Objective Happiness* », Kahneman (1999) présente une nouvelle approche basée sur l'évaluation immédiate du bonheur qui s'oppose aux méthodes de mesure « purement subjectives » (Kahneman, 1999a, p. 5). D'après des études menées au sein de l'économie comportementale, l'évaluation rétrospective (ou utilité de décision) correspond à des prédictions faites à partir de seulement deux choses: l'évaluation immédiate maximale d'une expérience (*peak*), et l'évaluation immédiate de la fin de celle-ci (*end*)². Les évaluations rétrospectives auraient ainsi pour principal défaut la négligence de la durée, c'est-à-dire le fait que la durée d'une expérience n'a pas d'effet sur son évaluation rétrospective (Kahneman, 1999a, p. 19). D'après ces résultats, l'évaluation rétrospective est un mauvais indicateur du sentiment de bonheur ressenti par l'individu ou « utilité totale » (Kahneman, 1999a, p. 20).

L'imperfection des données d'enquête aurait incité les psychologues du bonheur à réfléchir à de nouveaux outils de mesure du bonheur. Particulièrement gênés par les biais de rétrospection, ces nouvelles mesures ont pour objectif d'isoler la composante immédiate des évaluations. Deux

² Il s'agit de l'article de Kahneman, Wakker et Sarin (1997). Dans le cadre de l'économie comportementale (*behavioral economics*), les auteurs montrent que l'utilité effectivement ressentie par un individu (*total utility*) diffère systématiquement de son évaluation rétrospective (*remembered utility*). Étant donné que les choix des agents économiques sont influencés plus par leurs rétrospections que par l'utilité effectivement ressentie, ces auteurs font la distinction entre cette dernière (*decision utility*) et l'utilité totale.

méthodes sont proposées: l'enquête immédiate (*experience sampling method* : Kahneman *et al.*, 2004a), et l'enquête par reconstruction (*day reconstruction method* : Kahneman *et al.*, 2004b). Au lieu d'estimer le bien-être social par des mesures générales et rétrospectives de bien-être (*top-down approach*), les psychologues du bonheur proposent donc des mesures faites à partir d'évaluations immédiates (*bottom-up approach*).

Section 2.4. Le bonheur des économistes (les données de satisfaction)

Le développement de nouvelles mesures de bien-être subjectif dans l'objectif de corriger les biais de rétrospection des données ne constitue pas le centre d'intérêt des économistes du bonheur. Tout en s'écartant des discussions psychologiques, leur approche relève de l'analyse de données d'enquête. Ils évitent, par ailleurs, les discussions philosophiques et même techniques au sujet du concept du bonheur (Frey et Stutzer, 2002b, p. 4).

D'après Bruno Frey et Alois Stutzer (2002b), l'interprétation économique des données de bonheur diffère de celle des psychologues. L'analyse économique s'intéresse aux processus cognitifs qui affectent les évaluations rétrospectives. Ces économistes reconnaissent le fait que ces processus cognitifs rendent ces évaluations moins précises (Frey et Stutzer, 2002b, p. 6), mais cette imprécision ne leur pose pas de problème. Intéressés par l'impact des facteurs sociaux sur le sentiment de bien-être, ils profitent des biais cognitifs des données de satisfaction. D'après Frey et Stutzer, le bonheur des économistes ne se veut pas une variable objective, ni l'évaluation d'une activité en particulier. Il est mesuré par des données de caractère général qui demandent aux individus d'évaluer l'ensemble de leur vie (*ibid.*, p. 30)³.

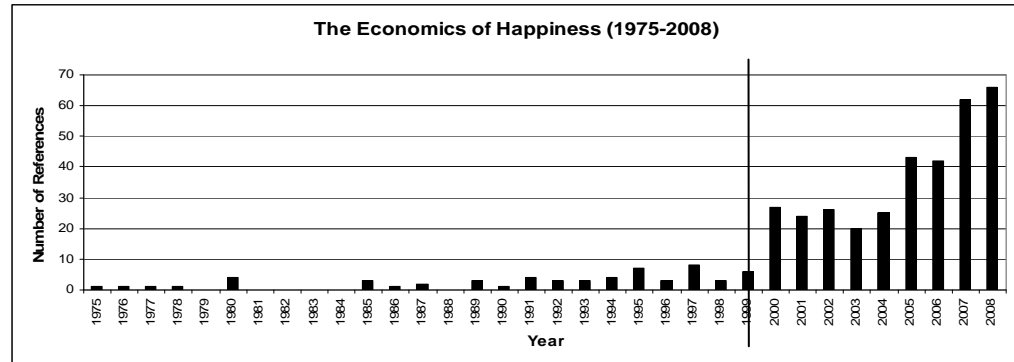
Section 2.5. L'économie du bonheur en chiffres

La prolifération d'analyses économiques du bonheur est un fait mis en avant dans la plupart des publications récentes. D'après Clark, Frijters et Shields

³ Ils critiquent d'ailleurs le concept de bien-être objectif (*objective happiness*) de Kahneman (1999a), qu'ils qualifient de normatif. En accord avec la position de Frey et Stutzer (2002b, p. 6), Helliwell (2006) pense qu'il est plus pertinent d'étudier des données de satisfaction (données d'enquête) que de procéder comme les psychologues le font (par des mesures « objectives »).

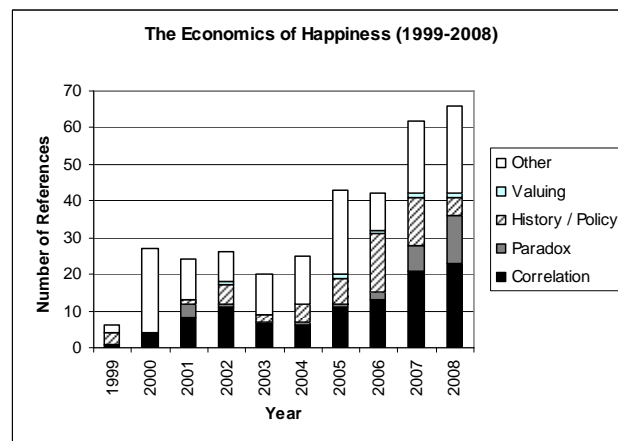
(2009), l'évolution de cette littérature est exponentielle. Une analyse similaire à celle de Clark *et al.* (2009) montre l'évolution suivante.

Figure 2.2 (source: EconLit)



Contrairement au faible nombre de publications de la première période (1974-1999), le nombre de publications économiques récentes est significatif. L'analyse détaillée du contenu des publications récentes (1999-2008) montre qu'elles sont consacrées principalement à quatre types d'analyse : « l'analyse des corrélations » (*Correlation*), « l'analyse du paradoxe du bonheur » (*Paradox*), « l'analyse historique et politique de l'économie du bonheur » (*History/Policy*), et « l'approche de la satisfaction de vie pour l'évaluation de biens environnementaux » (*Valuing*).

Figure 2.5 (source: EconLit)



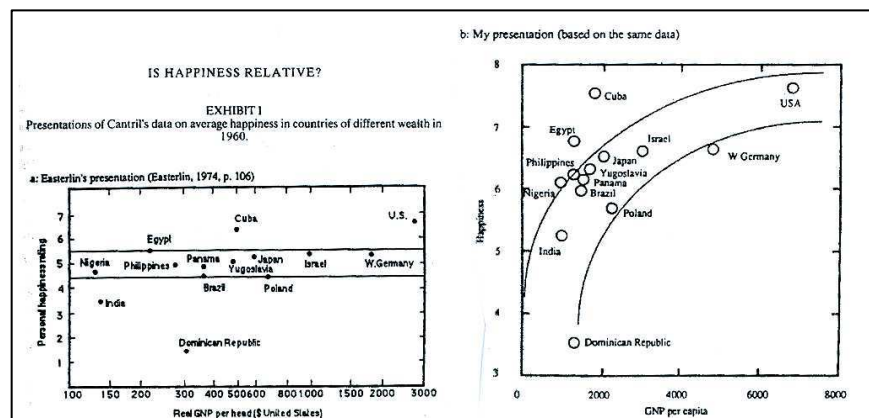
L'analyse du paradoxe et des implications politiques de l'économie du bonheur sont présentées dans les deux dernières sections du chapitre 2 de la thèse. L'analyse des corrélations et de l'approche de la satisfaction de vie sont abordées au chapitre 6.

Section 2.6. Débats sur le paradoxe du bonheur

Les discussions autour du paradoxe du bonheur (tel qu'avancé par Easterlin en 1974 et mis à jour en 1995) ont été fondamentales pour le développement récent de la sous-discipline. Les effets de l'adaptation hédonique et des comparaisons sociales ont été des sujets de controverse, notamment entre Richard Easterlin et le sociologue Ruut Veenhoven.

Dans son article « *Is happiness relative?* » Veenhoven (1991) s'attaque directement au paradoxe d'Easterlin ainsi qu'à l'ensemble des théories du bonheur relatif (Veenhoven, 1991, p. 1). Il aborde l'analyse des données étudiées par Easterlin (1974) (Fig. 2.6, côté gauche), en fournit une nouvelle représentation (côté droit), et affirme qu'il y a une corrélation positive (mais décroissante) entre revenus et bonheur.

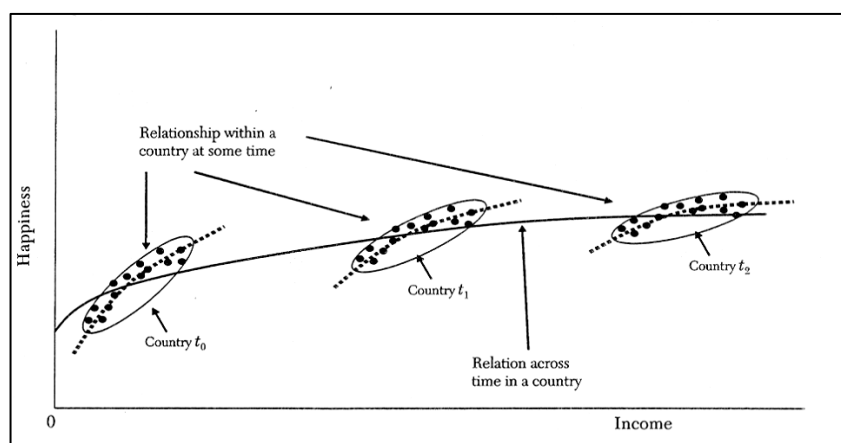
Figure 2.6 (Veenhoven 1991, p. 11)



Une dizaine d'années plus tard, et suite à l'émergence de l'économie du bonheur, Veenhoven (2003) revient sur la validité du paradoxe du bonheur et manifeste sa préoccupation à l'égard des implications du paradoxe en termes de politique économique (Hagerty et Veenhoven, 2003, pp. 2-3). Dans « *Feeding the Illusion of Growth and Happiness* » (2005b), Easterlin répond avec beaucoup d'ironie. Il constate que malgré le développement croissant de la sous-discipline, le programme de recherche construit autour de son paradoxe est toujours loin d'être accepté par la communauté économique.

Et effectivement, d'après la synthèse des travaux récents proposée par Clark *et al.* (2009), il y aurait un consensus parmi les « nouveaux » économistes du bonheur quant à l'effet positif du revenu sur les données de bonheur (Figure 2.8) ainsi qu'un fort questionnement du paradoxe d'Easterlin (1974, 1995).

Figure 2.8 (Clark *et al.*, 2009, p. 101)



La section 2.6 conclut et montre, à travers l'étude de Clark *et al.* (2009), que le développement récent de l'économie du bonheur est marqué par l'analyse empirique de fonctions d'utilité dans laquelle les données de bonheur sont utilisées en tant que mesures approximatives du concept d'utilité. Elle montre aussi que la sous-discipline est à présent avancée comme un complément plutôt qu'une alternative à l'analyse des préférences révélées.

Section 2.7. Conclusion du Chapitre 2 : le message

La section 2.7 conclut le chapitre en contrastant l'analyse récente de Richard Layard (*Happiness: Lessons from a New Science*, 2005) avec celle de son article de 1980 présenté dans la Section 1.5. Elle montre que son message a profondément changé, sa nouvelle analyse étant basée sur l'étude de données de bonheur plutôt que sur des principes analytiques (notamment le paradoxe du bonheur). Elle décrit également le travail de Layard (2005) qui présente l'analyse de données de bonheur comme approche complémentaire à l'analyse économique des préférences révélées.

L'économie du bonheur contemporaine aboutit donc à se présenter comme étant complémentaire aux courants dominants d'analyse économique, plutôt que comme une alternative de changement radical par rapport à ceux-ci.

LA PAUSE DE CINQ MINUTES

Le passage de la première partie de la thèse aux deux suivantes implique un fort changement de perspective (de l'analyse de l'économie du bonheur à l'analyse des relations entre l'économie et la psychologie). Ce changement de perspective est signalé, dans le corps du document, par le passage du premier au second mouvement de la seconde symphonie de Mahler avec sa célèbre pause de cinq minutes (p. 105).

PARTIE II : ANALYSE ECONOMIQUE ET PSYCHOLOGIE EXPERIMENTALE

La deuxième partie de la thèse traite de l'histoire des relations entre l'analyse économique et la psychologie expérimentale et comporte deux chapitres. Le chapitre 3 montre que l'analyse du comportement des consommateurs des premiers économistes du bonheur était influencée par des théories psychologiques de contrôle du comportement. Il trace l'histoire de ces théories en partant des origines du béhaviorisme et montre qu'il y a une longue histoire de rapports entre ces analyses psychologiques et l'analyse économique. Le chapitre 4 analyse l'histoire des fondements psychologiques de la théorie du choix rationnel sur la même période. Mécanique, consistante, et ensuite comportementale, cette théorie est présentée dans sa relation avec une autre partie de l'histoire de la psychologie.

Chapitre 3 : Analyse économique et psychologie béhavioriste

Section 3.1. Introduction

Outre l'aspect excessivement formel de l'économie du bien-être, c'est le principe de la souveraineté du consommateur qui a le plus perturbé les premiers économistes du bonheur (1974-1999). Ce serait dû au fait que la théorie du choix rationnel traite du comportement de consommateurs rationnels et optimisateurs, n'étant empêchés d'atteindre leurs buts que par le manque de moyens, qu'elle reste concentrée sur des problèmes de disponibilité de ressources. Au contraire, l'approche des premiers économistes du bonheur se focalise sur le processus de formation des préférences des consommateurs, ce qu'elle fait à partir d'analyses de contrôle du comportement. L'origine de ce type d'analyse se trouve à la naissance de la psychologie expérimentale américaine¹.

¹ Les Sections 3.1 au 3.3 font référence aux travaux d'histoire de l'analyse psychologique de Boring (1950), Madden (1965), Daston (1978), Ash (1980), Herman (1995), Mills (1992, 1998), Capshew (1993, 1999), Rutherford (2003, 2006), Mandler (2007), et Igo (2008).

Section 3.2. Les origines de la psychologie expérimentale américaine

La première partie du chapitre 3 porte sur l'origine de la psychologie expérimentale américaine. Elle commence en mettant en perspective l'influence des travaux expérimentaux de Wilhelm Wundt (Mandler, 2007, p. 59) ainsi que celle de l'approche naturaliste britannique (basée sur le principe de la continuité entre l'espèce humaine et les espèces animales). Mais elle montre aussi qu'il y a un consensus parmi les historiens de l'analyse psychologique autour de l'idée que c'est la société américaine (plutôt que les programmes de recherche européens) qui a le plus fortement influencé le développement de la psychologie expérimentale depuis les années 1880 (Mills, 1998, p. 2).

L'analyse comparative, comme le béhaviorisme, ont été des courants dominants dans l'analyse psychologique dès années 1880 aux années 1920 (Mills, 1998). Le béhaviorisme était étroitement lié au mouvement progressiste, de même que son déclin à partir des années 1920 (Mills, 1998, p. 55). Il a été avancé, notamment par John B. Watson, pour qui l'objectif de l'analyse psychologique était "la prédiction et le contrôle du comportement" (Watson, 1913, p. 158).

Section 3.3. Sur « l'institutionnalisme béhavioriste »

La section 3.3 de la thèse montre qu'il y a bien eu une contrepartie dans l'analyse économique à la psychologie expérimentale américaine du début du 20^e siècle ainsi qu'au béhaviorisme de Watson. Cette contrepartie a été initiée par Thorstein Veblen [1857-1929] et Wesley C. Mitchell [1874-1948] (1), et développée davantage dans le programme des institutionnalistes américains de l'entre-deux-guerres (2).

(1) L'institutionnalisme développé par Veblen et Mitchell pendant les deux premières décennies du 20^e siècle était étroitement lié aux théories d'instincts avancées par les psychologues fonctionnalistes, ainsi qu'à l'idéal progressiste du contrôle scientifique du comportement humain. Ces économistes ont essayé d'appliquer les sciences sociales à l'analyse de l'« art de la consommation ». La thèse présente « *The Backward Art of Spending Money* » de Mitchell (1912) qui renoue avec l'approche initiée par Veblen dans *La*

Théorie de la Classe de Loisir (1899) en montrant qu'au contraire de l'« art de la production » (qui est soutenu par le progrès des sciences de la nature), l'« art de la consommation des ménages » (*the art of housekeeping*) manque de support scientifique (Mitchell, 1912, p. 3).

(2) Après la première guerre mondiale, des institutionnalistes tels que W. C. Mitchell, L. K. Frank et M. A. Copeland ont développé un programme d'« institutionnalisme béhavioriste ». Ce programme était encore plus concerné par des réformes sociales progressistes que celui des premiers institutionnalistes (Asso et Fiorito, 2004b, M. Rutherford, 2000, Mills, 1998).

L'approche quantitative de Mitchell était une tentative de répliquer les travaux des béhavioristes en avançant une « conception objective et un traitement quantitatif du comportement humain » (Mitchell, 1925, p. 6). D'après lui, la mesure statistique était « indispensable pour transformer la quête aveugle du bonheur social en un processus d'expérimentation intelligent » (*ibid.*). Ce processus devait être supporté par l'analyse quantitative du comportement de groupes, ce qui consistait principalement à contrôler l'environnement de ces comportements (Mitchell, 1925, pp. 8-9).

Lawrence K. Frank [1890-1968], quant à lui, allait encore plus loin en ce qui concerne le contrôle du comportement. Sa théorie des cycles (1923) consistait à expliquer les déséquilibres économiques par le contraste entre le développement rapide des techniques de production et le caractère fixe des « uses et coutumes (institutions) des hommes constituant l'économie monétaire de production » (Frank, 1923, p. 639). Frank affirmait qu'au contraire des changements techniques, l'être humain agissait de manière récurrente en raison de la fixité de ses habitudes de comportement. Il proposait donc l'application de « technologies de comportement » afin de « mettre à jour » les comportements économiques et maîtriser ainsi les cycles d'affaires (Frank, 1924a, p. 24).

Enfin, Morris A. Copeland [1895-1989] était également favorable à l'analyse scientifique des habitudes et il était engagé dans l'analyse des effets des techniques de publicité sur les préférences des consommateurs. Il considérait ces techniques comme des pratiques « éducatives et informatives » s'opposant au principe économique de la souveraineté du consommateur (Copeland, dans Cherington et Copeland, 1925, p. 38).

La section 3.3 se conclue en montrant qu'alors que l'approche défendue par Mitchell, Frank et Copeland concordait avec le « mouvement de réforme libérale progressiste en Amérique » (M. Rutherford, 2000a, p. 298) ces deux mouvements ont décliné ensemble à partir de la fin des années 1920. Elle montre aussi que l'institutionnalisme béhavioriste a été fortement critiqué en raison de son discours sur le contrôle du comportement. L'exemple le plus clair de cette réaction est l'analyse de Frank Knight (1925), qui était dirigée non pas contre la validité scientifique de l'approche, mais contre le « sens » de la conception institutionnaliste de la société.

Section 3.4. B. F. Skinner : du béhaviorisme à la philosophie sociale

La section 3.4 de la thèse montre que depuis la fin des années 1930 et jusqu'aux années 1970, il y a eu un renouvellement du béhaviorisme, caractérisé par la production de théories psychologiques fortement sophistiquées (Mills, 1998, p. 4). Le nouveau béhaviorisme a été animé notamment par les contributions de B. F. Skinner, qui a appliqué des résultats obtenus dans l'analyse expérimentale animale à l'analyse de sujets assez controversés tels que le soin des enfants, l'éducation, et la philosophie sociale.

Le comportement, pour Skinner, était conçu comme le résultat de chaînes répétées de stimulus, réponses, et renforts, ou, dans les termes de Skinner, comme le résultat de « l'histoire des renforts passés » rencontrés par un organisme dans son interaction avec l'environnement. Cette histoire était censée modeler le comportement des individus et une partie importante du programme de Skinner a consisté à montrer comment « des comportements apparemment commandés par des processus cognitifs pouvaient être modelés dans des boîtes d'expérimentation animale [boîtes de Skinner] » (Mills, 1998, p. 124).

Après la deuxième guerre mondiale, Skinner est devenu de plus en plus partisan de l'application sociale de ses résultats. Entre la fin des années 1940 et les années 1970 il a montré qu'il « était possible de développer des technologies sociales pour former des êtres humains » (Mills, 1998, p. 149). C'est par ce biais, pensait Skinner, « que la science devait servir à promouvoir le bien social » (*ibid.*). Il a prouvé que des comportements indésirables pouvaient être corrigés par des « renforts positifs » plutôt que par des mesures coercitives (Skinner, 1976, p. x).

Cependant, l'analyse de Skinner, comme d'autres programmes scientifiques du contrôle du comportement, ont été fortement contestées et socialement contrôlés dans les années 1970 (A. Rutherford, 2006). Les réactions les plus critiques ont suivi la publication de *Beyond Freedom and Dignity* de Skinner (1971), un volume de philosophie sociale qui appliquait ses résultats scientifiques à l'analyse de sujets sociaux. D'après A. Rutherford (2006), la réaction sociale contre le béhaviorisme et ses applications représentent un épisode clairement illustratif des « processus par lesquels l'analyse psychologique et ses produits » sont « formés, réglés, et modifiés par la société » (*ibid.*, p. 218). Il est aussi intéressant de constater que le projet de Skinner a été contesté socialement et non pas au sujet de sa validité scientifique (A. Rutherford, 2000, pp. 385-386).

Section 3.5. Ordinalisme vs béhaviorisme

La section 3.5 s'oppose à l'interprétation dominante de la « révolution ordinaliste » en tant que mouvement béhavioriste dans l'analyse économique. Elle montre que la conception du comportement du consommateur des ordinalistes était radicalement opposée aux approches du contrôle du comportement des béhavioristes, et suggère que l'approche ordinaliste a été confondue avec le béhaviorisme en raison de l'usage de méthodologies opérationnalistes tant par Samuelson que par Skinner.

Il est certain que l'opérationnalisme était une référence méthodologique pour ces deux auteurs (Boring, 1950, Mills, 1992, 1998, Hands, 2004). Cependant, les différentes manières dont ces auteurs ont manipulé l'approche de Bridgman expliquent la différence radicale entre l'approche béhavioriste et celle qui est à la base de la théorie des préférences révélées.

L'approche de Samuelson consistait à (conceptuellement) faire varier des prix de paniers de biens afin que « les préférences des consommateurs puissent être 'révélées' et rendues opérationnellement significatives par 'l'opération' de révélation de préférences » (Hands, 2004, p. 957). Ceci indique que le but de Samuelson était de révéler des phénomènes « internes » aux consommateurs, ce qui prouve qu'il y avait du mentalisme dans le propos de Samuelson. Plutôt « qu'éliminer des notions intentionnelles telles que les préférences et l'utilité », la théorie de Samuelson a fini par « légitimer précisément ces mêmes concepts » (Hands, 2004, p. 962).

Parmi les behavioristes, c'est Skinner qui a « appliqué l'opérationnalisme de la manière la plus radicale à l'analyse psychologique » (Mills, 1992, p. 76). À l'opposé de l'approche des préférences révélées, le programme de Skinner a libéré l'analyse du comportement des « états mentaux » en définissant les causes (*drives*) du comportement « en termes d'opérations effectuées par l'expérimentateur » (Mills, 1992, p. 76). Les causes du comportement des behavioristes étaient donc « externes » aux organismes étudiés (« avoir faim », par exemple, était défini comme « une réduction du poids corporel de l'animal jusqu'à 80% de son poids en conditions de libre alimentation » (*ibid.*)).

La comparaison des approches de Skinner et de Samuelson montre que le programme ordinaliste n'était pas behavioriste, si l'on respecte la définition donnée par les psychologues. Le projet de Samuelson était loin de l'approche de Skinner qui consistait à analyser le comportement en tant qu'effet d'histoires des renforts passés.

Section 3.6. Le rejet de la pensée de Galbraith et de l'analyse économique de l'abondance

La section 3.6 de la thèse renoue avec l'analyse économique de l'abondance introduite au Chapitre 1. Elle montre que l'approche défendue par Galbraith a partagé la scène avec la philosophie sociale de Skinner entre la fin des années 1950 et les années 1970. Tout comme Skinner, les économistes de l'abondance ont pensé au comportement du consommateur en tant que sujet à des contrôles. Mais à la différence de Skinner, ces économistes ont fait face à des discours de « liberté et dignité » déguisés en tant qu'arguments scientifiques.

Dans *Le Nouvel Etat Industriel*, Galbraith (1967) introduit le concept de « filière inversée » (*revised sequence*) qu'il avance comme étant un complément aux « effets de dépendance » présentés dans *L'Ere de l'Opulence* (1958). Le concept de filière inversée était dirigé contre l'acceptation du principe de la souveraineté du consommateur et basé sur l'idée que les firmes avaient la faculté de contrôler leurs marchés, comme les préférences des consommateurs (Galbraith, 1967, p. 211). *The Costs of Economic Growth* d'E. Mishan (1967), était également un ouvrage critique à l'égard du « mythe de la souveraineté du consommateur ». Comme Galbraith, Mishan montrait

que les choix ouverts aux consommateurs étaient contrôlés par leurs environnements et qu'il y avait des restrictions institutionnelles importantes réduisant l'ensemble du « choix effectif » (*ibid.*, p. 152).

La section 3.6 conclut le chapitre en présentant *The Age of Uncertainty* (un projet de Galbraith contenant à la fois une série audiovisuelle et un livre) de manière parallèle au succès du *best-seller Walden two*, de B. F. Skinner (1976-1977). Elle montre que Skinner comme Galbraith étaient à ce moment identifiés par le grand public américain comme des que personnalités scientifiques s'intéressant aux problèmes des sociétés d'abondance comme, par exemple, la surconsommation, la pollution, et la menace de l'holocauste nucléaire. C'est à ce moment précis que M. Friedman produit la critique la plus virulente contre l'analyse économique de l'abondance.

Dans ses discours intitulés *From Galbraith to Economic Freedom* (1977), Friedman présente sa propre critique à l'analyse de Galbraith qui a été accompagnée par celle de ses partisans². Or, contrairement aux réactions contre le béhaviorisme (section 3.4), les affirmations de Friedman ont été présentées en tant qu'arguments scientifiques. Il déclarait qu'il n'avait « pas de preuve » (*ibid.*, p. 23) justifiant l'affirmation de Galbraith quant à la capacité de contrôle des « entreprises technostrutturées » (*ibid.*, p. 24), et il se demandait si l'analyse de Galbraith était « scientifique ou missionnaire » (*ibid.*, pp. 25-29).

L'orthodoxie économique a certainement réussi à exclure l'analyse économique de l'abondance du cœur de la discipline, et le chapitre 3 tire deux conclusions de cet événement. Premièrement, qu'il y a un fort contraste entre les réactions de contrôle social rencontrées par les béhavioristes comme Skinner, et celles rencontrées par l'économie de l'abondance qui ont été présentées sous forme d'évidences scientifiques. Deuxièmement, qu'il n'est pas surprenant que ce programme n'ait pas parvenu à faire partie du courant dominant de la discipline, car il était plus lié au béhaviorisme qu'aux fondements psychologiques de la théorie du choix rationnel. Ce qu'illustre le

² Ces discussions ont eu lieu en 1976, l'année où Friedman reçoit le prix de la banque de Suède en mémoire d'Alfred Nobel. Elles ont été publiées en 1977 et accompagnées d'extraits des critiques à la théorie de Galbraith par G. Stigler, H. Demsetz, R. Solow, G. C. Allen, J. Jewkes, et F. McFadzean. L'ajout de ces extraits était justifié par l'affirmation que « les écrits du Professeur Galbraith [avaient] été mieux publicités que ceux de ses adversaires » (*ibid.*, p. 6). En 1980 Friedman produit *Free to Choose*, une série audiovisuelle de dix épisodes en réponse à *The Age of Uncertainty*.

mieux le contraste entre ces deux approches économiques est précisément la défense par Samuelson du principe de la souveraineté du consommateur. Pour Samuelson, les biens produits sont déterminés par les « voix en dollars des consommateurs » (Samuelson, 1970, p. 40) et pour la plupart de ses lecteurs ces consommateurs sont des « rois souverains ».

Chapitre 4 : Les fondements psychologiques de l'analyse économique

Section 4.1. Introduction

Alors que le chapitre 3 de la thèse met en évidence l'absence d'approches de contrôle du comportement dans l'analyse économique, le chapitre 4 traite d'une autre partie de l'histoire de la relation entre économie et psychologie. Il prend en compte l'évolution historique des fondements psychologiques de la théorie du choix rationnel et offre un complément historique à l'histoire développée dans le chapitre 3.

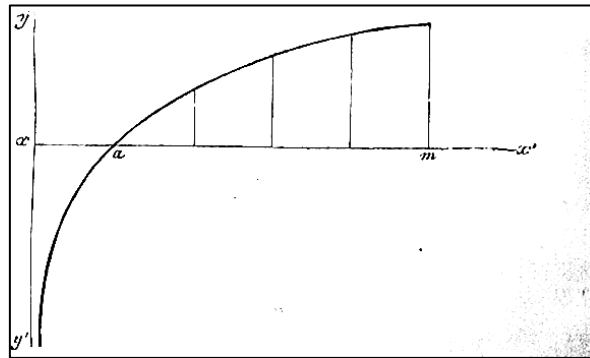
En ce qui concerne l'histoire de l'économie du bonheur, ce chapitre montre que, contrairement aux premiers travaux (1974-1999), le développement récent de la sous-discipline (1999-2009) est fondé sur une psychologie qui, bien qu'incompatible avec le béhaviorisme, est cohérente avec les principes psychologiques de base de l'analyse économique contemporaine.

Section 4.2. De la psychophysique de Fechner à la psychologie expérimentale de Wundt

La section 4.2 introduit l'analyse psychophysique comme méthode de mesure de l'intensité d'une sensation à partir du contrôle de la mesure du stimulus qui en est à l'origine. Gustav T. Fechner [1801-1887] a amené ce programme à maturité à travers la généralisation de ces résultats (associés à ceux d'E. H. Weber) sous la forme d'une « loi psychophysique fondamentale ».

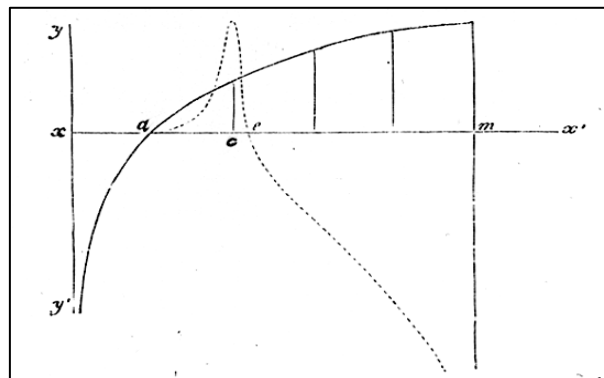
L'expression mathématique de la loi de Fechner est présentée graphiquement dans la thèse. La courbe dans la Figure 4.1 correspond à la présentation de la loi de Fechner par W. Wundt (1874) et représente l'accroissement de l'intensité d'une sensation en fonction d'accroissements arithmétiques de l'intensité d'une stimulation. « α » correspond au « seuil d'excitation », et « m », à la « hauteur d'excitation » de la sensation.

Figure 4.1 (Wundt, 1874, p. 406)



D'après l'analyse de Wundt, une sensation est aussi caractérisée par son « ton » (ou son « sentiment sensoriel »), ce qui renvoie aux sentiments de plaisir et de peine. Or, tout comme l'intensité de la sensation, le sentiment sensoriel est représentée par Wundt comme une variable continue par rapport à l'intensité de la stimulation. Aux deux points identifiés dans la Figure 4.1, s'ajoutent les points « c » et « ρ », qui correspondent respectivement au point de plaisir maximum et au point d'indifférence dans la Figure 4.3 : « en présence d'une certaine énergie modérée de la sensation, le ton de sentiment atteint le maximum de sa valeur positive et passe alors, en traversant un point d'indifférence, à des valeurs négatives, qui croissent toujours davantage » (Wundt, 1874, p. 532).

Figure 4.3 (Wundt, 1874, p. 529)



Section 4.3. Analogies économiques à partir de la loi psychophysique fondamentale

La section 4.3 de la thèse montre que les dernières décennies du 19^e siècle furent des années de développement mathématique du côté des sciences sociales et de recours à l'approche psychophysique. Cette sorte de fascination pour la loi de Fechner que l'on observe, non seulement chez certains économistes de l'époque mais aussi dans d'autres domaines scientifiques, est présentée par Feuerhahn (2005) dans les termes suivants.

« On a peine aujourd'hui à se représenter l'engouement que suscita cette perspective parmi les scientifiques de l'époque, les physiologues et psychologues d'abord, et jusqu'aux chercheurs en sciences sociales ensuite qui étaient tous en quête de garanties de scientificité. Nombreux furent ceux qui, d'une manière ou d'une autre tentèrent de ramener leur objet d'étude à une relation mathématique analogue à celle découverte par Fechner entre le stimulus physique et la sensation, et de garantir de la sorte à leur pratique le statut de science. » (Feuerhahn, 2005, p. 783)

En économie, certaines grandes analogies ont été avancées par W. S. Jevons (1871) et F. Y. Edgeworth (1877, 1881). Celles de Jevons ont été moins influencées par l'analyse des psychophysiciens allemands que par l'approche des psychophysiologues britanniques, notamment William Carpenter, Henry Maudsley, et Thomas Laycock (cf. Maas, 2005, p. 154, Chaigneau, 2002). Jevons a appliqué ces théories à l'analyse des sujets économiques, principalement à travers les *Natural Elements of Political Economy* (1855) de Richard Jennings¹.

La thèse montre ensuite que la *Theory of Political Economy* de Jevons (1871) était aussi un moyen de « recadrer les 'lois des besoins humains' en termes mécaniques et, par là, d'appliquer les méthodes des sciences physiques » à l'analyse économique (Maas, 2005, p. 154). A première vue on pourrait penser que l'approche de Jevons était en accord avec le naturalisme moniste des psychophysiologues britanniques, ce qui n'est pas tout à fait le cas. A la différence des psychologues, Jevons a développé sa propre théorie du comportement qui a été, en effet, identifiée à une « mécanique des intérêts individuels » par des institutionnalistes américains tels que Veblen et Mitchell (Mitchell, 1910, p. 109). Cette approche était définitivement plus proche de la

¹ Voir Stigler (1950), Zafirovski (2001), Chaigneau (2002), Maas (2005), Bruni et Sugden (2007) et leurs références.

psychologie philosophique (et non pas scientifique) de J. S. Mill que de l'approche psychophysique britannique ou encore que de la psychologie expérimentale de Wundt.

La section se conclue en montrant que l'approche d'Edgeworth était similaire à celle de Jevons. Elle consistait à emprunter divers éléments à la psychologie expérimentale de Wundt et à les fusionner en un unique axiome à partir duquel Edgeworth a développé sa *Psychique Mathématique (Mathematical Psychics)* de manière déductive. Il est important de souligner que tant Jevons qu'Edgeworth ont développé des théories qu'ils considéraient comme des parties composantes de l'analyse psychologique. Pour reprendre les termes de la deuxième partie de la thèse, il s'agit des premières « psychologies économiques ».

Section 4.4. Deux réactions : Weber (1908) et Mitchell (1910)

La section 4.4 de la thèse montre que les psychologies économiques de Jevons, d'Edgeworth et des auteurs qui s'en sont inspirés, ont conduit à deux types de réactions : la critique dualiste de Max Weber (1908) (1), et la critique naturaliste-moniste des institutionnalistes américains (2). Alors que Weber pensait que l'analyse économique devait développer sa propre approche en isolant les motifs du comportement économique des autres motifs du comportement (une approche d'« idéaux-types »), les institutionnalistes défendaient une approche naturaliste consacrée à l'étude du comportement humain dans toute sa complexité.

(1) Dans son article sur la « Théorie de l'Utilité Marginale et la Loi Psychophysique Fondamentale » (1908), M. Weber a fortement contesté l'idée d'utiliser la loi de Fechner comme base pour la théorie de l'utilité. Selon Weber, il n'était pas correct de considérer l'analyse économique comme une application de l'analyse psychophysique car les deux approches étaient opposées dans leur essence même.

Weber pensait que l'analyse économique n'était pas (et ne devrait pas être) fondée sur une loi de la nature, puisque son caractère était celui d'une construction théorique fondée sur l'idée que l'individu économique agit « en termes strictement 'rationnels' » (Weber, 1908, p. 31). Pour Weber, le but de

L'analyse économique n'était pas d'expliquer des comportements concrets, mais d'offrir des « approximations » à ceux-ci en supposant des actions purement rationnelles. A la différence de la psychologie, l'analyse économique devait procéder selon ses propres termes par la production de « séries d'événements conceptuellement construits » ou « idéaux-types » (*ibid.*, p. 34). Même si ces « puretés idéales » n'étaient qu'abstractions de la « réalité historique » (*ibid.*), Weber pensait que la théorie de l'utilité marginale était utile en tant qu'elle était une partie composante de la science de l'« histoire économique » qui devait être construite à partir d'une somme d'analyses « typiques » (*ibid.*).

(2) La réaction institutionnaliste à la « mécanique des intérêts individuels » est nettement différente de celle de Weber. Des auteurs institutionnalistes, comme Veblen (1909), Mitchell (1910) et Clark (1918), pensaient que l'analyse économique du comportement humain ne devait pas être déduite de « quelques principes sur la nature humaine » (Mitchell, 1910, p. 97), mais construite à partir de l'analyse du comportement de « ceux qui étaient spécialisés dans ce domaine » (Clark, 1918, p. 4). Ces auteurs institutionnalistes pensaient que les relations de cause à effet explorées par les psychologues fonctionnalistes étaient justifiées (Veblen, 1909, p. 625), et qu'à la différence du raisonnement « déductif, aprioriste et subjectif » de l'analyse marginaliste, l'approche psychologique avait un « caractère et une force objective, impersonnelle et matérielle » (*ibid.*). Inspiré par l'*Introduction à la Psychologie Sociale* de William McDougall (1909), Mitchell pensait l'approche psychologique comme une « science positive du comportement » qui allait bientôt devenir une « histoire naturelle de l'esprit » (Mitchell, 1910, P. 100). À la différence de la mécanique des intérêts individuels (et de la conception Weberienne de l'analyse économique), ces institutionnalistes voulaient étudier les comportements économiques dans toute leur complexité (*ibid.*, pp. 109-110).

Ces institutionnalistes critiquaient la spécialisation suggérée par Weber, et cherchaient à montrer que la théorie de l'utilité marginale s'adressait, au mieux, à la moitié du problème économique : « l'art de la production » (*the art of making money*, selon les termes de W. C. Mitchell). Elle n'avait strictement rien à dire sur « l'art de la consommation » (*ibid.*, p. 200). Ils critiquaient aussi fortement le fait que les théoriciens du choix se focalisaient

sur un sujet d'analyse qui « n'était pas suffisamment large comme pour exiger l'énergie d'une compagnie entière de travailleurs scientifiques » (*ibid.*, p. 202).

Section 4.5. Deux autres tentatives d'élargissement de la psychologie économique

La section 4.5 présente deux autres théories économiques des phénomènes d'adaptation et d'apprentissage : l'introduction d'une perspective dynamique dans la théorie de l'équilibre générale, et l'introduction dans la théorie de l'analyse psychologique des processus d'information (les sciences cognitives).

Le premier de ces deux cas est abordé en présentant brièvement une partie de l'ouvrage *Modelling Rational Agents* de Nicola Giocoli (2003), qui montre que les tentatives d'analyse dynamique ont été principalement impulsées par E. Lindahl, G. Myrdal, O. Morgenstern, T. Hutchinson, et J. Hicks dans les années 1920 et 1930. Ces auteurs ont essayé de modéliser le « fonctionnement des systèmes économiques en dehors de l'équilibre » en introduisant la « révision des anticipations » dans l'analyse, ce qui impliquait l'inclusion d'« agents économiques dotés d'habilités limitées d'information et de prévision » (*ibid.*, p. 144).

Le second de ces cas est introduit en présentant une partie de « l'histoire de l'analyse économique du point de vue des cyber-sciences » (« *history of economics from the cyborg point of view* ») de Philip Mirowski (Mirowski, 2002, p. 29) qui montre que la réaction des économistes orthodoxes au livre de von Neumann et Morgenstern (1944) sur la *Théorie des Jeux et Comportement Economique*, consistait à rejeter l'analyse des processus d'information de von Neumann (*ibid.*, p. 270), tout en gardant des parties de la théorie qui n'étaient pas conçues comme des « analyses sérieuses du traitement cognitif de l'information » (ce qui était le cas du principe de « l'utilité espérée de von Neumann-Morgenstern » (*ibid.*, p. 280)).

La section se conclue en montrant que ces deux tentatives de dépassement de la conception standard du comportement économique ont conduit au développement d'une notion formelle de l'agent économique (l'analyse

d'ensembles de préférences) plutôt qu'à l'analyse de comportements économiques concrets d'adaptation et d'apprentissage.

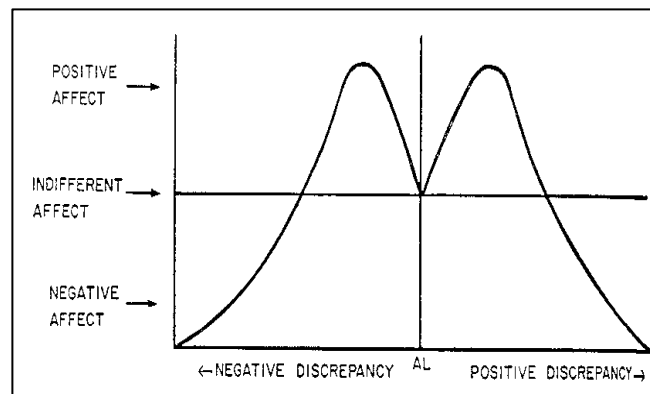
Sections 4.6 et 4.7. De la psychophysique au principe de l'« adaptation hédonique »

Les sections 4.6 et 4.7 montrent qu'alors que l'analyse des phénomènes d'adaptation et d'apprentissage n'ont pas réussi à transformer les fondements psychologiques de l'analyse économique, différentes approches à l'analyse de ces sujets ont été développées par des psychologues expérimentaux. À la différence des économistes, ces psychologues se sont appuyés sur des tests pour reformuler la loi de Fechner et ont prolongé cette approche à l'analyse de nouveaux phénomènes. Ces prolongements ont été principalement présentés entre les années 1950 et les années 1960 et synthétisés par Harry Helson sous forme d'une *Théorie du Niveau d'Adaptation* (1964). Cette théorie a été consacrée premièrement à l'analyse de la perception et ensuite à celle de l'affectivité, de la motivation et des comportements interpersonnels. L'ouvrage de Helson a unifié et présenté près de trois décennies de recherches sous forme d'une « approche expérimentale et systématique au comportement ».

La section 4.6 traite des origines de la théorie du niveau d'adaptation dans les recherches de Helson sur les sensations visuelles. Les premiers travaux de Helson conduits entre 1929 et 1938 relevaient de l'analyse psychophysique et étaient consacrés à l'analyse de la perception des couleurs – et plus spécifiquement à celle du phénomène de « l'adaptation chromatique » (Helson et Judd, 1932). La section montre ensuite la manière dont l'analyse de Helson a été prolongée et appliquée, notamment, à l'analyse de l'affectivité, de la motivation, et du comportement (Appley, 1971).

En ce qui concerne le comportement, la théorie du niveau d'adaptation part du principe selon lequel l'affectivité est l'un de principaux facteurs de motivation d'un organisme (Helson, 1964, p. 328). La théorie de la motivation de McClelland et Clark est présentée par Helson au travers d'une double représentation (d'une représentation en miroir) de la courbe de Wundt. Elle montre que l'affectivité (qui explique la plupart des motifs du comportement) dépend des différences entre l'intensité d'un stimulus et le niveau d'adaptation (AL dans la Figure 4.5) de l'organisme.

Figure 4.5 (Helson, 1964, p. 382)



Cette théorie a été appliquée par certains psychologues aussi bien à l'analyse de l'apprentissage qu'à la formation d'anticipations, d'aspirations ou à d'autres « normes internes » censées expliquer le comportement humain (Helson, 1971, pp. 15-16).

Mais à la différence des analyses psychologiques présentées dans le chapitre 3, le programme de Helson avait pour but principal de comprendre (plutôt que de contrôler) les comportements. Ce point a été perçu comme une insuffisance de la théorie, et en 1971, lors d'un colloque tenu en l'honneur de la théorie du niveau d'adaptation, les principaux contributeurs à ce programme de recherche ont discuté les applications « politiques économiques et sociales » de la théorie (*ibid.*, p. 16). La section 4.7 présente la contribution de Philip Brickman et Donald T. Campbell : « *Hedonic Relativism and Planning the Good Society* » (1971), qui était supposée transformer les principes développés en laboratoire en pratiques politiques concrètes favorisant le bonheur humain. Quelques années plus tard, en 1978, P. Brickman, D. Coates et R. Janoff-Bulman (1978) ont publié le premier test empirique de la théorie du niveau d'adaptation du bonheur et ont corroboré le principe du « bonheur relatif » prévu par théorie. Ces deux contributions sont devenues des pièces fondamentales de l'histoire de l'analyse des données de bonheur.

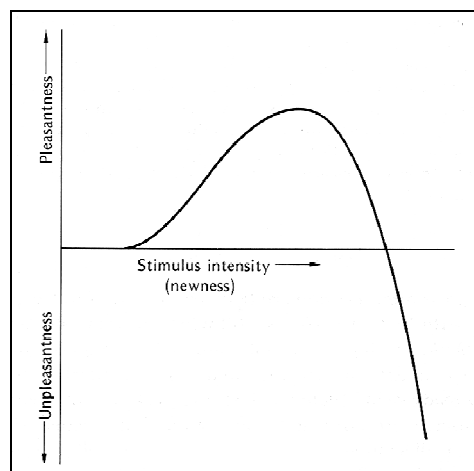
Section 4.8. De la théorie du niveau d'adaptation à la nouvelle économie comportementale

La section 4.8 montre que la théorie du niveau d'adaptation a été appliquée à l'analyse économique d'au moins deux manières différentes. Elle a été employée par Scitovsky (1976) dans *L'Economie sans Joie*, et dans la

« théorie des perspectives » (*prospect theory*) de Kahneman et de Tversky (1979).

La thèse reprend d'abord la partie analytique de *L'Economie sans Joie*, introduite au chapitre 1, et montre que c'est « l'analyse psychologique de la motivation » – « cette partie du corpus de la psychologie physiologique qui est la plus approprié pour établir, prolonger, et corriger, si nécessaire, la théorie économique du comportement du consommateur » (Scitovsky, 1976, p. xii) – qui a été à la base de la théorie de Scitovsky. Elle introduit l'analyse de Scitovsky sur « la nouveauté » qui suggère que pour procurer du plaisir, la consommation de biens exige des capacités d'adaptation à différents degrés de nouveauté.

Figure 4.6 (Scitovsky, 1976, p. 35)



D'après Scitovsky, la théorie du choix rationnel ne reconnaît pas le « besoin de nouveauté et de variété » des consommateurs, ainsi que le « nécessité des aptitudes à la consommation » (*consumption skills*) pour l'adéquation de celle-ci à des degrés de nouveauté divers (*ibid.*, p. 150). Il conclut que le système d'éducation américain produit des consommateurs insuffisamment aptes.

La section 4.8 se conclue en montrant que la théorie du niveau d'adaptation a été également mobilisée dans la « théorie des perspectives » de Kahneman et Tversky (1979). D'après ces auteurs, les agents ne sont pas concernés par des niveaux absolus de richesse ou de bien-être lorsqu'ils choisissent en situation d'incertitude, mais plutôt par les gains ou les pertes qui sont relatifs à leurs niveaux de référence. À la différence de la théorie de l'utilité espérée, la théorie des perspectives définit les gains et les pertes par rapport à des

niveaux d'adaptation (Kahneman et Tversky, 1979, p. 274) ce qui renvoie explicitement à la théorie de Helson (*ibid.*, p. 277). Toutefois, plutôt que d'emprunter la totalité de la théorie du niveau d'adaptation, Kahneman et Tversky ont recours à des parties spécifiques de celle-ci et avancent leur propre théorie du comportement sous la forme d'une « théorie des choix » (*ibid.*, p. 284).

Section 4.9. Conclusion

Dans la dernière section du chapitre 4 on montre qu'alors que l'analyse du comportement proposée par les économistes de l'abondance n'a pas réussi à intégrer l'analyse économique, l'approche de Kahneman et Tversky (1979) a, elle, conduit au développement de la nouvelle économie comportementale, qui constitue aujourd'hui le cœur de la discipline. On y analyse certains comptes rendus récents du développement de l'économie comportementale qui identifient des différences importantes entre « l'ancienne » et la « nouvelle » approche (Sent, 2004). Ainsi, Sent (2004) analyse le programme de Simon, qui proposait une approche alternative à l'analyse économique standard. Or, cette « ancienne » approche « n'a jamais vraiment réussi » alors que les « nouveaux » économistes comportementaux sont « les nouvelles vedettes de la discipline » (*ibid.*, p. 750).

Il est intéressant de noter que la théorie du niveau d'adaptation paraît soutenir les principales « déviations » de la théorie développée par les économistes comportementaux (cf. Rabin, 1998, p. 13). La théorie du niveau d'adaptation explique l'« effet de dépendance », l'« aversion aux pertes », la « diminution de la sensibilité » ainsi que d'autres concepts clés pour comprendre la portée de la nouvelle économie comportementale. La dernière section du chapitre 4 insiste sur le fait que la théorie du niveau d'adaptation est utilisée pour expliquer les déviations par rapport à la théorie des choix, plutôt que les éléments qui composent son « noyau dur ». Elle montre que la nouvelle économie comportementale emprunte ses fondements à « l'analyse comportementale des décisions » (*behavioral decision research*), champ né de « l'application de l'analyse économique à des problèmes psychologiques » qui a été « ajusté et réintroduit » dans l'analyse économique par Kahneman et Tversky (Heukelom, 2009, p. 2). La nouvelle économie comportementale est donc bien une « psychologie économique ». Et si la prise en compte de

déviations a « élargi la portée » de la théorie du choix du consommateur, il existe également des « limites à cet élargissement » (*ibid.*, p. 133), notamment sur la question de l'analyse de la formation des préférences. Ainsi, la nouvelle économie comportementale maintient-elle un principe de préférences exogènes (*ibid.*, p. 135), ce qui s'oppose radicalement, tant à l'analyse du contrôle du comportement des économistes de l'abondance des années 1960 et 1970, qu'à celle des premiers économistes du bonheur (1974-1999).

PARTIE III : L'ANALYSE ECONOMIQUE DES DONNEES SUBJECTIVES

La troisième partie de la thèse est composée de deux chapitres : le premier (chapitre 5) porte sur l'analyse économique des données subjectives en tant qu'outils scientifiques, et le deuxième (chapitre 6) sur l'usage des données subjectives en tant qu'outils de gouvernement. Elle se conclue en appliquant cette distinction à l'analyse économique des données de bonheur.

Chapitre 5 : Données subjectives et théorie économique

Le chapitre 5 de la thèse traite de l'histoire de la quantification subjective et la *théorie économique*, et se fonde sur la tentative de George Katona de développer une « psychologie économique » ou « économie comportementale » à partir de l'analyse d'enquêtes explorant les motifs et attitudes d'hommes d'affaires et des consommateurs.

Sections 5.1 et 5.2. Introduction et controverses sur l'usage économique de données subjectives

Le chapitre commence par passer en revue deux controverses célèbres dans l'histoire de l'analyse économique : la « mesure sans théorie » et les « coûts totaux ». Il présente ensuite le programme de Katona et montre que l'influence du psychologue dans ces débats a été largement négligée dans l'analyse historique dominante de ces événements. La section 5.2 de la thèse souligne que ces événements ont été lus en tant que parties composantes de l'analyse économique dominante, telles que l'histoire de l'économétrie et de la théorie de l'entreprise, plutôt que par rapport à l'histoire de l'analyse des données subjectives.

Sections 5.3 et 5.4. Sur le programme d'économie psychologique de Katona

La section 5.3 présente les premières tentatives d'analyse économique de Katona, lorsque son centre d'intérêt passe de l'analyse psychologique de « l'apprentissage et l'enseignement » (*Organizing and Memorizing*, 1940), à l'analyse de l'inflation en temps de guerre (*War without Inflation*, 1942), et ensuite à sa monographie produite au sein de la *Cowles Commission* (*Price Control and Business*, 1945).

Les travaux psychologiques de Katona se situent dans la tradition de la psychologie de la forme (*Gestaltpsychologie*). Ils traitent de l'analyse de l'apprentissage et l'enseignement, et affirment qu'il est mieux « d'apprendre par la compréhension » des processus d'information que par la mémorisation d'associations spécifiques. Dans *War without Inflation* (1942) Katona a appliqué cette approche à l'analyse économique. Il étudie le phénomène d'inflation en temps de guerre ainsi que l'effet du contrôle des prix dans la structure de production de l'économie de guerre américaine et affirme que l'inflation n'est pas « l'effet automatique de facteurs économiques » (Katona, 1942, p. 4), mais qu'elle est aussi influencée par des facteurs psychologiques.

La thèse montre que, outre les politiques d'impôt et de fixation des prix, Katona affirmait qu'il était nécessaire de faire comprendre au public l'objectif de ces mesures. En d'autres termes, qu'il était nécessaire d'ajuster le cadre de référence du public pour adapter ses comportements économiques aux objectifs de l'économie de guerre. Katona montrait que, afin d'obtenir l'entière coopération du public, les annonces de gouvernement devaient être fondées sur la connaissance des attitudes de la population. Les études psychologiques de Katona étaient censées recueillir ce type d'information (*ibid.*, 172).

Dans *Price Control and Business* (1945) Katona a étudié les « actions d'hommes d'affaires américains confrontés aux contrôles de prix ainsi qu'à d'autres mesures appliquées en temps de guerre » (Katona, 1945, p. 2). Son projet était une alternative à l'analyse quantitative du NBER qui consistait à analyser des agrégats économiques, et son but était de recueillir des données utiles à la planification de la guerre, ainsi que de tester la légitimité de l'usage de données d'enquête au tant qu'« outils d'analyse économique » (*ibid.*, p. 5). La méthode employée par Katona consistait à analyser les attitudes d'hommes d'affaires et d'expliquer ensuite leurs procédures de détermination de prix.

Comme ces procédures avaient à leur tour des effets significatifs sur le positionnement relatif des firmes, Katona conclut que l'analyse des attitudes était une approche valide pour l'étude de certains phénomènes économiques.

La section 5.4 traite des études de Katona après son installation au Bureau d'Économie Agricole à Washington (1946), puis à l'Université de Michigan (1947-1972), où il a participé (avec Rensis Likert et Angus Campbell) à l'établissement du Centre d'Étude d'Enquêtes (*Survey Research Center*, SRC). Elle montre que le travail de Katona dans ces institutions a été en grande partie consacré à l'analyse des attitudes des consommateurs et qu'il a défendu son programme en tant que tentative de dépassement de la théorie keynésienne de la consommation.

Pour Katona (1946), la fonction de consommation keynésienne était le résultat d'une « analyse économique sans psychologie ». Au lieu d'analyser la consommation comme l'effet de variations de revenu (via la propension marginale à consommer), il pensait que le comportement du consommateur devait être étudié par la méthode d'enquêtes initiée par Rensis Likert à la Division des enquêtes du ministère américain de l'agriculture. Cette méthode avait pour objet d'analyser les motifs, les attitudes et les projets d'investissement des ménages américains suite aux forts montants d'épargne cumulés pendant la seconde guerre mondiale (Katona et Likert, 1946, p. 197).

L'analyse des données subjectives a montré que les ménages changeaient rapidement de comportement et que les « changements d'attitude précédaient les changements d'action » (*ibid.*). Puisque « l'économie sans psychologie » négligeait le rôle des attitudes dans la détermination du comportement des consommateurs, elle était incapable d'expliquer des changements soudains.

La section se conclue en présentant l'*Analyse Psychologique du Comportement Économique* de Katona (1951) et montre que son programme n'était pas censé produire des outils de prévision d'agrégats macroéconomiques. Les données d'anticipations produites par Katona étaient des mesures d'attitudes plutôt que des « mesures de comportements futurs » (*ibid.*, p. 174) et le but principal de son analyse était de fournir « une explication aussi complète que possible du champ psychologique dominant à un moment donné » (*ibid.*).

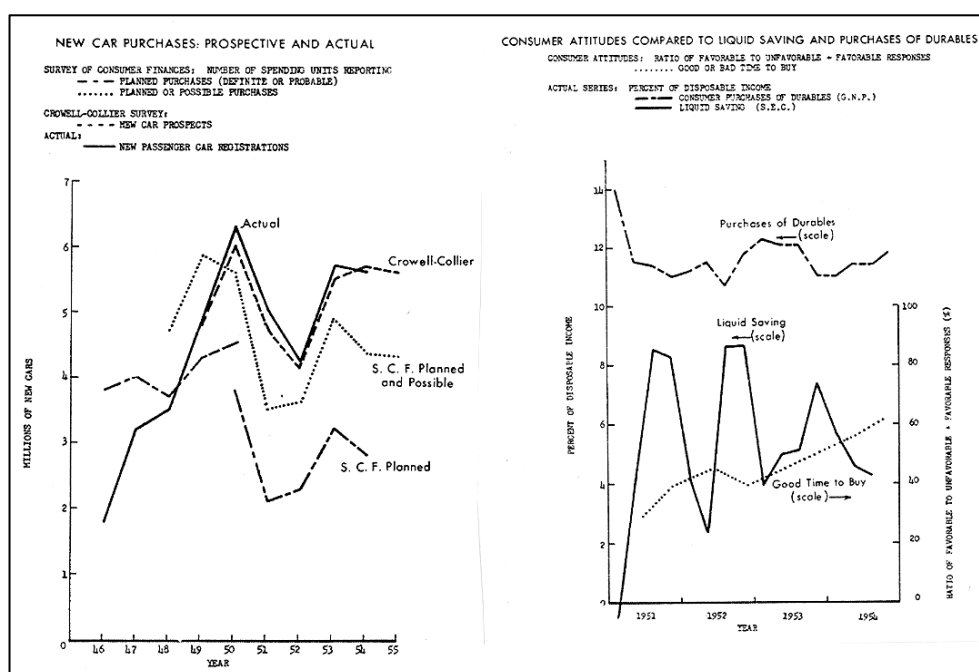
Sections 5.5 et 5.6. Sur la réception du projet de Katona

La section 5.5 montre qu'en dépit de sa carrière de psychologue de l'apprentissage et de l'enseignement par la compréhension, Katona a été mal compris par les économistes. En 1954, le Sous-comité des Statistiques Économiques du Congrès des États-Unis a demandé au Conseil Supérieur de la Réserve Fédérale d'évaluer la qualité « de l'information statistique concernant l'épargne, les stocks, ainsi que les anticipations tant des producteurs que des consommateurs » (lettre du Sous-comité, dans Smithies *et al.*, 1955). Cependant, à la différence de la conception holistique de Katona, le rapport du Comité formé par la Réserve Fédérale s'est focalisé sur la capacité prédictive individuelle des données d'attitudes, d'anticipations et d'intentions d'achat, qui ont été comparées aux résultats d'autres études.

En ce qui concerne les données rassemblées dans l'Enquête des Finances des Consommateurs (*Survey of Consumer Finances, SCF*), le Comité a été favorable à la production de données « d'intentions d'achat ». Leur rapport était par contre défavorable quant à la capacité de prévision des « données d'attitudes moins spécifiques » (*ibid.*, p. 37).

Le rapport présentait plusieurs diagrammes comparant la capacité prédictive des données d'intentions d'achat de la SCF avec celle des données d'attitudes moins spécifiques.

Figure 5.3 (Smithies *et al.*, 1955, p. 47, 60)



Le Comité affirmait que les données d'intentions d'achat (« S.C.F. », côté gauche) étaient suffisamment précises pour permettre les prévisions des changements de tendance dans l'achat de biens durables (*Actual*). Son avis était par contre défavorable pour l'analyse de données d'attitudes moins spécifiques (*Good Time to Buy*, côté droit) (*ibid.*, pp. 53-54).

Katona (1957) a répondu au rapport en affirmant que le Comité négligeait le caractère « socio-psychologique » de ses études en les comparant à des « études économiques plus étroitement conçues » (Katona, 1957, p. 40). Il a montré que le Comité était excessivement concentré sur l'utilisation des données d'attitudes en tant qu'outils de prédiction, « ne considérant pas de manière explicite les problèmes basiques de la recherche interdisciplinaire ou comportementale » (*ibid.*). Il a insisté sur le fait que les données d'anticipations, un « sous-groupe du concept plus général d'attitudes », n'étaient pas considérées en tant qu'outils de prédiction, mais en tant que variables psychologiques faisant partie d'un programme dont l'objectif était de comprendre l'attitude des consommateurs par rapport à leur situation passée, présente, et future (*ibid.*).

En 1959, James Tobin, un ancien membre du Comité évaluateur de la SCF, répond à Katona (1957) en insistant sur l'évaluation de la capacité prédictive des données d'attitudes (Tobin, 1959, p. 1). Il insiste aussi sur le fait que les données d'intentions d'achat ont des « capacités de prédiction » que les autres données n'ont pas (*ibid.*, p. 10) et il reproduit les conclusions du Comité en avançant ainsi une sorte de position « officielle » des économistes envers l'analyse des données concernant les attitudes.

La section 5.6 de la thèse conclut le chapitre 5 en montrant que les différences entre Katona et les membres du Comité d'évaluation de la SCF ont conduit à une controverse entre la Réserve Fédérale et le SRC au sujet de la qualité des données subjectives d'anticipations économiques. La distinction entre l'analyse d'attitudes (SRC), d'une part, et celle d'« intentions d'achat » et des « probabilités d'achat » de l'autre (NBER), a conduit à des approches différentes. Dans sa monographie intitulée *Anticipations and Purchases* (NBER, 1964), Thomas Juster reconnaît l'existence de ces deux approches alternatives à l'analyse des données d'attitudes : l'approche « holistique » de Katona et Mueller (SRC), et celle de J. Tobin, A. Okun, L. Klein, J. Lansing, S. Whitey et Juster lui-même (NBER) qui est plutôt concentré sur l'analyse des capacités prédictives des données d'enquête (Juster, 1964, pp. 140-141).

La différence principale entre les deux approches était que, contrairement au programme de psychologie économique de Katona (qui était une tentative théorique), le but de l'approche de Juster était de développer des moyens de prévision des achats de biens durables.

En ce qui concerne la production de données d'enquête, il y eut des nombreuses discussions entre les années 1950 et les années 1960, ainsi que des changements significatifs dans les programmes financées par la Réserve Fédérale (qui a interrompu son soutien financier à la SCF en 1960 ; Juster, 2004). La première « Enquête Trimestrielle d'Intentions d'Achat des Consommateur » (*Quarterly Survey of Consumer Buying Intentions*, SCBI) a été conduite par le Bureau du recensement américain à partir de 1963 avec le but de remplacer la SCF (McNeil, 1974, p. 2). Or, à la différence de la SCF, qui procédait par des interviews détaillées, la SCBI rassemblait des questionnaires expédiés par courrier qui récupéraient exclusivement des données d'intentions d'achat.

Dans les années 1960, Thomas Juster et James Byrnes du Bureau du recensement ont étudié la qualité des données de la SCBI et ont proposé une nouvelle méthode qui a conduit au remplacement des questionnaires d'intentions d'achat (qui collectaient des réponses du type « oui ou non ») par des questionnaires demandant des « probabilités d'achat ». Suivant les conseils de Juster et Byrnes, le Bureau du recensement a décidé de remplacer la SCBI pour « L'Enquête d'Anticipations d'Achat des Consommateurs » (*Survey of Consumer Buying Expectations*, SCBE) en 1966 (McNeil, 1974, p. 3), qui a été suspendue en 1973 en raison de la mauvaise qualité de ses prévisions.

Chapitre 6 : Données subjectives et politique économique

Le chapitre 6 de la thèse traite de l'analyse économique des données subjectives en tant qu'« outils de gouvernement » (Desrosières, 2008a, p. 7). Les premières sections du chapitre présentent deux des principaux domaines dans lesquels l'analyse des données subjectives s'est développée de façon importante : l'analyse des anticipations économiques (section 6.2), et l'évaluation contingente de biens environnementaux (section 6.3). Les deux dernières sections du chapitre (6.4 et 6.5) sont fondées sur l'analyse de l'histoire des statistiques d'A. Desrosières (2000, 2003, 2008) et reviennent sur l'analyse économique des données de bien-être subjectif. La thèse conclut en introduisant l'« approche par la satisfaction de vie pour l'évaluation de biens environnementaux » (*the life satisfaction approach for valuing environmental goods*) qu'elle présente comme étant le reflet du succès de l'analyse des données subjectives en tant qu'outils de gouvernement.

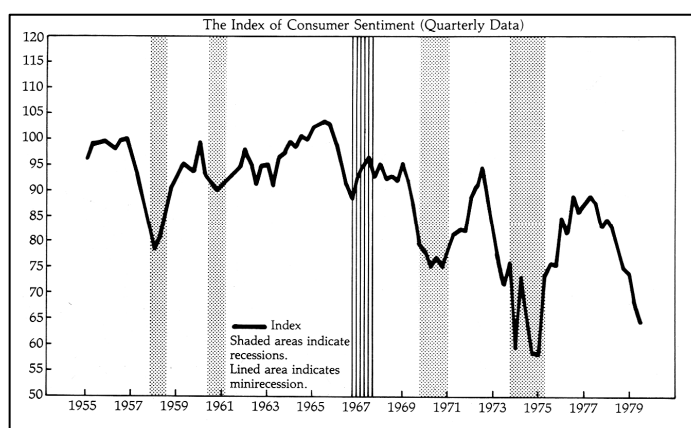
Sections 6.1 et 6.2. Introduction et suite de l'histoire de l'analyse des données d'anticipations

La section 6.1 introduit le chapitre en montrant que l'analyse économique des données subjectives s'est intensifié récemment, notamment grâce à l'usage de données de « satisfaction dans l'emploi » (*économie du travail*), de « santé subjective » (*économie de la santé*), d'« anticipations économiques » (*économie des anticipations*), d'« évaluations contingentes » (*économie environnementale* et *économie écologique*), et des résultats à des « interrogations et enquêtes en ligne » (*éducation économique*). Cette section affirme qu'une grande partie du développement de ces disciplines est le résultat d'analyses appliquées (plutôt que théoriques) et montre que le programme théorique de Katona a été éclipsé par le développement de l'Indice de Sentiment du Consommateur qui est un outil de prévision économique produit par le SRC et employé par des organismes gouvernementaux et par des hommes d'affaires.

La section 6.2 montre qu'un certain nombre des questionnaires introduits par Katona dans les années 1940, les questionnaires d'attitudes, notamment (*Survey of Consumer Attitudes*, SCA), sont employés à présent pour produire

l'Indice de Sentiment du Consommateur qui est devenu un outil de prédiction reconnu dans les années 1960. Or, cohérent avec le développement de son programme théorique, Katona a, dès le départ, défendu le rôle théorique joué par l'Indice en particulier et par l'enquête SCA en général. Il a insisté sur la distinction entre la « prévision » et la « compréhension » des fluctuations de court terme de la demande agrégée et a affirmé que les prévisions dérivées de l'Indice n'étaient qu'une partie de l'ensemble de son programme « d'économie comportementale ». Pour Katona, l'Indice était suffisant pour prévoir des changements de tendance économique mais insuffisant pour comprendre le comportement des consommateurs. La figure suivante reproduit la présentation de l'Indice par Katona (1980) où les chutes de l'Indice précèdent systématiquement les périodes de récession économique.

Figure 6.1 (Katona, 1980, p. 10)



La thèse montre ensuite qu'en dépit des efforts de Katona pour promouvoir son programme d'économie comportementale, c'est l'Indice de Sentiment du Consommateur qui a eu le plus grand des succès (Curtin, 2004). Dans « *Psychology and Macroeconomics* » Curtin (2004) montre, assez nettement, que le résultat principal du « Programme de Comportement Economique » de Katona a été l'Indice, et que l'approche de Katona était « tout à fait différente de l'approche qui est devenue dominante dans la théorie économique » (c'est-à-dire l'hypothèse des anticipations rationnelles) (*ibid.*, p. 141).

La section 6.2 renoue avec l'histoire de l'analyse des probabilités d'achat, introduite par Juster et ses associées dans les années 1960 (Chapitre 5). Elle présente l'Enquête sur les anticipations économiques (*Survey of Economic Expectations, SEE*) conduite depuis 1993 par le Centre d'enquêtes de l'Université de Wisconsin et étudiée par des économistes tels que Dominitz et

Manski (1997a, b) ou Manski et Straub (2000). Méthodologiquement parlant, la SEE a été fortement influencée par l'approche de Juster et constitue une tentative pour renouer avec les programmes fédéraux d'enquêtes des années 1960 et 1970. Elle explore les « probabilités d'achat », et, plutôt que de proposer une théorie alternative, elle se conforme aux théories économiques dominantes (Dominitz et Manski, 1997a, p. 857). L'objectif à long terme de Manski et de ses associés est « d'utiliser des données d'anticipations pour prévoir les comportements économiques » (*ibid.*). La section conclut que malgré les différences qui existent entre les deux principaux programmes produisant des données d'anticipations (la SCA et la SEE), leurs données subjectives ont rencontré un certain succès en tant qu'outils de prévision. L'analyse de ces données est devenue utile en tant qu'« outil de gouvernement » (Desrosières, 2008a, p. 7).

Section 6.3. Le développement des évaluations contingentes

La section 6.3 présente brièvement l'histoire du développement des évaluations contingentes. Elle montre que ces évaluations ont émergé en tant qu'alternatives aux méthodes établies de « coût de voyage » et de « prix hédoniques » dans l'évaluation de biens environnementaux, et que les premières « applications concertées de la méthode » sont apparues dans les années 1970 (Smith, 1993, p. 8) pour s'intensifier à partir des années 1980. Cette intensification s'explique par l'entrée en vigueur du *Comprehensive Environmental Response, Compensation and Liability Act* (CERCLA) en 1980 aux États-Unis, et par le nombre croissant d'études qu'il a suscité. Le CERCLA a en effet donné aux organismes gouvernementaux le droit d'engager des poursuites judiciaires contre les individus responsables de dommages sur les ressources naturelles. Les règlements du CERCLA incluaient la compensation des pertes liées aux « valeurs de non-usage » ou aux « valeurs d'existence » de ces ressources (le concept de valeur d'existence avait été introduit par J. Krutilla en 1967).

En 1990, suite à l'accident du pétrolier *Exxon Valdez* en Alaska, le Congrès des États-Unis a voté l'*Oil Pollution Act*. L'agence américaine chargée de l'étude de l'atmosphère et des océans (*National Oceanic and Atmospheric Administration*, NOAA) a eu la responsabilité de rédiger les règlements établissant les méthodes d'évaluation des pertes. Elle a cité une commission

constituée par K. Arrow, R. Solow, P. Portney, E. Leamer, R. Radner et H. Schuman afin d'obtenir un avis sur la « fiabilité potentielle de la méthode d'évaluation contingente » (Portney, 1994, p. 8). Face à l'absence de « méthodes indirectes » pour évaluer le dommage sur les valeurs d'existence à partir de données de marché (Arrow *et al.*, p. 3), la commission NOAA (*the NOAA panel*) a dû prendre position quant à la capacité des évaluations contingentes à « donner des estimations fiables » (*ibid.*). Elle a conclu à la pertinence des estimations produites par cette méthode, à condition que cette dernière respecte une série de directives proposées dans le rapport (Arrow *et al.*, p. 43).

La section montre ensuite que le rapport de la commission NOAA a été fortement contesté par des économistes tels que Diamond et Hausman (1994) qui affirment que la méthode d'évaluation contingente « entre en contradiction avec la théorie économique » (Diamond et Hausman, 1994, p. 46) car elle « ne mesure pas les préférences qu'elle est censé évaluer » (*ibid.*). Enfin, cette section conclut qu'en dépit des nombreuses imperfections et des critiques économiques qui ont été adressées à la méthode, l'évaluation contingente est à présent une technique établie de quantification subjective. Une recherche sur *EconLit* pour la période 1999-2009 fournit 330 références d'articles économiques contenant le terme « évaluation contingente » au titre, contre 58 contenant « prix hédonique » et 49 contenant « coût de voyage ».

Section 6.4. L'usage de données subjectives en tant qu'outils de gouvernement.

D'après la section 6.4, l'analyse des données d'attitude et d'anticipations (section 6.2), tout autant que l'évaluation contingente de biens environnementaux (section 6.3), prouvent que l'analyse économique des données subjectives est devenue un outil de gouvernement plutôt qu'un outil scientifique. Cette section présente l'analyse historique d'Alain Desrosières dans *L'Argument Statistique* (2008), qui spécifie les liens qui existent entre des « formes statistiques », des « modes d'action » et des « formes de penser la société et l'économie » au travers d'une série de configurations historiques.

Figure 6.5 (source: Desrosières, 2008, p. 56)

L'ÉTAT, LE MARCHÉ ET LES STATISTIQUES			
	Façons de penser la société et l'économie	Mode d'action	Formes de statistiques
État ingénieur La production et les hommes (depuis le XVII ^e siècle)	Institution hiérarchisée et organisée rationnellement France de Colbert à de Gaulle URSS	Optimisation sous contrainte. Réduction des coûts. Planification centralisée. Technocratie. Grands travaux. Vision à long terme.	Démographie. Production en quantité physique. Tableaux d'échanges interindustriels. Balances matières (URSS)
État libéral Le commerce et les prix (depuis le XVIII ^e siècle)	Physiocrate Un grand marché. Concurrence libre.	Lutte contre les corporatismes. Libre-échangeisme. Lois anti-trust protégeant la concurrence.	Statistiques favorisant la transparence des marchés (cas de l'agriculture américaine). Mesures d'éventuelles positions dominantes. Parts de marché.
État-providence Le travail salarié et sa protection (depuis la fin du XIX ^e siècle)	Le marché du travail n'est pas un marché comme un autre. Il doit être protégé.	Lois sur la durée du travail, sur les accidents, le chômage, les retraites. Systèmes d'assurances obligatoires garantissant des droits sociaux.	Statistiques du travail. Salaires, emploi, chômage. Enquêtes par sondage sur les budgets ouvriers. Mesures des inégalités sociales. Indices des prix à la consommation.
État keynésien La demande globale et ses composantes (depuis les années 1940)	Le marché ne peut fonctionner tout seul sans engendrer des crises. Il doit être régulé au niveau global.	Surveillance et pilotage du gap éventuel entre offre et demande globales via les politiques monétaires et budgétaires.	Comptabilité nationale. Analyse de la conjoncture. Budgets économiques.
État néo-libéral Polycentrisme, incitations, palmarès (depuis les années 1990)	Un grand marché. Concurrence libre et non faussée. Financiarisation. Démultiplication des centres de décision sous forme de réseaux.	Passage des droits aux incitations. Exemples : bonus-malus, marché des droits à polluer. Transformation des administrations en agences. Contractualisation. Coordination par émulation. Exemple de la Méthode Ouverte de Coordination européenne.	Objectivation d'espaces d'équivalence nouveaux. Objectivation des qualités contractuelles des statistiques. Construction et usage d'indicateurs et de palmarès pour évaluer et classer des performances. Le <i>benchmarking</i> complète ou remplace directives et règlements. Controverses sur la quantification du PIB.

L'analyse de Desrosières consiste à dévoiler des « modes de description statistique spécifiques aux différentes configurations historiques de relations entre l'État et le marché » (Desrosières, 2008a, p. 40). Elle est fondée sur « cinq configurations jugées typiques » : l'« État ingénieur », l'« État libéral », l'« État-providence », l'« État keynésien », et l'« État néo-libéral » (*ibid.*, pp. 39-56). La Figure 6.5 reproduit le tableau qui résume ces cinq configurations (la Figure 6.5 de la thèse est construite à partir du même tableau ainsi qu'à partir d'une présentation de ce tableau par Alain Desrosières au *Cercle d'Epistémologie* (Janvier 2009)). En ce qui concerne l'analyse des statistiques produites sous l'État néo-libéral en général, et l'analyse des données de bonheur en particulier, Desrosières suggère que la question importante à se poser est « à quoi servent ces données ? ». L'État néo-libéral, explique-t-il, se caractérise par l'usage des statistiques comme éléments d'incitation et comme éléments utiles à la construction « d'indicateurs et de palmarès pour évaluer et classer des performances » (cf. Figure 6.5). L'évaluation contingente en tant qu'outil de quantification de biens environnementaux (Section 6.3) permet de déterminer des incitations grâce à l'usage de données subjectives. L'usage de données de bonheur comme un outil de classement international est un autre exemple de l'utilisation des données subjectives au sein de l'État néo-libéral.

La section 6.4 souligne deux éléments importants qui ressortent de l'analyse de Desrosières, et du développement de l'économie du bonheur. Il y a d'abord le fait que l'« État néo-libéral » produit des nombreuses données d'enquête de façon décentralisée tant au niveau général de la société qu'à des niveaux locaux (l'État central n'est plus le responsable exclusif de la production de données statistiques). En second lieu, cette section souligne que les bases de données produites sont ensuite disponibles pour l'usage au sein de différentes disciplines. Ceci signifie que le lien entre la production et l'analyse des données n'est plus si étroit que dans le cas des quatre autres configurations. À la différence de l'analyse psychologique des données de bonheur, l'économie du bonheur n'est pas directement impliquée dans la production de données subjectives. La plupart des économistes du bonheur emploient des résultats d'enquêtes générales qui n'ont pas été conçues pour produire des outils économiques.

Deux bilans de l'analyse des données de bonheur faits par des économistes montrent que le manque d'application économique de l'analyse de ces

données est une des faiblesses évidentes de la sous-discipline. Dans son article intitulé « *Subjectives Outcomes in Economics* » Daniel Hamermesh (2004) traite de « l'émergence croissante de l'utilisation et de l'analyse » de données subjectives en économie (Hamermesh, 2004, p. 2). Il affirme que l'analyse économique « court » après ces données plutôt qu'elle ne les produit afin de résoudre ses propres problèmes (*ibid.*), et il ajoute qu'à la différence des données de santé subjective (qui sont employées comme approximations pour l'analyse économique de la « santé objective »), il n'y a aucun « lien évident » entre les données de bonheur et le concept d'utilité (*ibid.*, p. 3).

Dans « *Happiness: What's the Use?* », K. Tribe (2008) passe en revue deux des principales contributions récentes à l'analyse économique du bonheur : *Happiness: Lessons from a New Science* (Layard, 2005), et *Economics and Happiness* (Bruni et Porta, 2005). S'il partage la position d'Hamermesh en affirmant que les économistes devraient « laisser les états subjectifs des êtres humains aux sociologues et aux psychologues » (Tribe, 2008, p. 467), il affirme aussi que la « (ré)découverte du 'bonheur' par l'orthodoxie économique reste partielle et non convaincante » (*ibid.*, p. 461). Il montre qu'à la différence des économistes du bonheur, les économistes du bien-être avaient des idées claires quant à l'objectif de leur discipline (*ibid.*, p. 464).

Section 6.5. Conclusion : sur l'usage économique des données de bonheur

La dernière section de la thèse renoue avec l'analyse quantitative du chapitre 2 (Fig. 2.5) et montre que la plupart de l'analyse récente des données de bonheur consiste à étudier les corrélations qui existent entre ces données et d'autres variables¹. Les économistes du bonheur n'étudient pas seulement l'effet de variables économiques sur les données de bonheur, mais également ceux des autres variables présentes dans les données d'enquête (*ibid.*). Cette section présente certains des bilans de la sous-discipline établis par Frey et Stutzer (2002b), et par DiTella et MacCulloch (2006). Ces bilans suggèrent qu'au lieu de produire des données de bonheur dans l'idée d'élaborer des

¹ Clark *et al.*, (2009), par exemple, montrent que l'analyse économique des données de bonheur est en grande partie issue de la disponibilité de données de panel, notamment dans la « *British Household Panel Survey* (BHPS) », la « *German Socio-Economic Panel Study* (GSOEP), et la « *Russian Longitudinal Monitoring Survey* (RLMS) » (Clark *et al.*, 2008, p. 99). D'autres sources de données de bien-être subjectif sont la *World Values Survey*, la *World Database of Happiness*, l'*Eurobarometer Survey*, ainsi que la *Satisfaction with Life Scale* introduite par Ed Diener et ses associés (Frey and Stutzer, 2002a, pp. 26-28).

nouveaux programmes de recherche, la démarche des économistes du bonheur consiste à explorer les alternatives potentielles à l'usage de ces données.

L'absence d'application des analyses économiques du bonheur semble avoir conditionné la stratégie de développement de la sous-discipline. Les derniers paragraphes de la section 6.5 montrent que son développement récent est marqué par l'apparition de « l'approche par la satisfaction de vie pour l'évaluation de biens environnementaux » (*life satisfaction approach for valuing environmental goods*). Cette approche a été introduite par Heinz Welsch dans un article intitulé « *Environmental Valuation based on Happiness Surveys* » (2002) qui consiste à utiliser l'analyse des données de bonheur comme une alternative aux méthodes de « cout de transport », de « prix hédoniques » et d'« évaluation contingente » (Section 6.3)².

Pour Welsch, l'idée « remarquablement simple » qui sous-tend l'approche par la satisfaction de vie est que le bonheur des individus peut être modélisé « en fonction de leurs revenus ainsi qu'en fonction des conditions environnementales dominantes, tout en contrôlant l'ensemble des autres facteurs » (*ibid.*, p. 2740). Cette idée repose sur le principe qu'il existe des corrélations positives entre données de bonheur et revenus, et interprète les données de bonheur comme des approximations « adéquates et valides » à des mesures d'« utilité totale » (Frey, Luechinger, et Stutzer, 2009). La fin de cette section conclut le chapitre (et la thèse) en montrant que les défenseurs de la nouvelle approche négligent la distinction avancée par les psychologues du bonheur entre « évaluations immédiates » et « évaluations rétrospectives » du bonheur (chapitre 2). Dans « *Experienced Utility as a Standard of Policy Evaluation* », Kahneman et Sugden (2005) présentent clairement la position des psychologues à l'égard de la nouvelle approche. Cet avis est pourtant négligé par les économistes du bonheur qui se contredisent, d'ailleurs, en ce qui concerne la fiabilité relative des mesures rétrospectives et immédiates du bonheur (Frey et Stutzer, 2002b, p. 30; chapitre 2).

La thèse conclut en affirmant qu'il y a un certain anarchisme méthodologique dans l'analyse économique des données de bonheur.

² van Praag et Baarsma (2005), Welsch (2006, 2007a, b), Ferrer-i-Carbonell et Gowdy (2007), Carrol *et al.* (2009), Frey *et al.* (2009), Luechinger (2009), Luechinger et Raschky (2009), MacKerron et Mourato (2009), Welsch (2009), et Welsch et Kuhling (2009).

CONCLUSION GENERALE

La thèse débute par une présentation de deux types de reconstructions normatives de l'histoire de l'économie du bonheur et d'une reconstruction officielle de l'histoire du rapport entre l'analyse économique et l'analyse psychologique (sections 0.2.1, 0.2.2, et 0.2.3). Elle se conclue sur une révision de ces trois analyses.

(1) Le bonheur est-il « de retour » ? Et ce retour constitue-t-il « une révolution dans l'analyse économique »?

La première partie de la conclusion montre que plutôt que de « redécouvrir » l'analyse classique du bonheur (Bruni et Porta, 2007), les auteurs qui ont initié le champ de l'analyse contemporaine du bonheur (comme Easterlin 1974, Scitovsky 1976, et certains de leurs successeurs) ont participé aussi à un mouvement qui consistait à analyser l'effet de l'abondance économique sur le bien-être des consommateurs. La plupart des ces auteurs ont contesté la théorie du choix rationnel et ont avancé des recommandations de politique économique à la fois nouvelles et assez radicales. À l'inverse, le développement récent de l'économie du bonheur suit une stratégie qui consiste à situer l'approche comme complément (plutôt qu'alternative) à la théorie du choix ainsi qu'aux politiques macroéconomiques. La première partie de la thèse conclut que l'économie du bonheur contemporaine est loin d'être révolutionnaire cela tant en termes analytiques qu'en termes de politique économique.

(2) L'analyse psychologique est-elle « de retour »?

La seconde partie de la conclusion montre que l'histoire du rapport entre l'analyse économique et la psychologie expérimentale est bien plus complexe que ne le suggère l'analyse historique couramment admise, qui consiste à analyser l'« inclusion » ou l'« exclusion » de l'analyse psychologique dans l'analyse économique (Section 0.2.2). En explorant cette histoire du point de vue de l'histoire de l'économie du bonheur, la thèse conclue (i) qu'il y a toujours des fondements psychologiques à l'analyse du comportement

économique et (ii) qu'il y a plus d'« une psychologie » impliquée dans l'histoire des relations entre les deux disciplines.

(3) Y a-t-il quelque chose de plus qu'« un scepticisme infondé » envers l'analyse économique des données subjectives ?

Enfin, sur la troisième partie de la thèse, la conclusion rappelle que le succès de l'analyse des données subjectives s'est produit sur le champ des pratiques économiques plutôt que sur le champ du développement théorique de la discipline. Ce succès semble expliquer la stratégie la plus récente des économistes du bonheur, qui consiste à avancer « l'approche par la satisfaction de vie » comme un outil pertinent pour évaluer les biens environnementaux.

Sur la base de ces trois points, la thèse conclue que l'histoire de l'économie du bonheur présente un point de vue pertinent pour analyser les relations entre économie et psychologie : cela tant pour la relation entre analyse économique et psychologie expérimentale (Partie II) que pour la relation entre économie et psychologie sociale (Partie III).

