JEVONS, DEBREU AND THE FOUNDATIONS OF MATHEMATICAL ECONOMICS: AN HISTORICAL AND SEMIOTIC ANALYSIS

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Bernard Dupriez, Gradus: Les Procédés Littéraires (Dictionnaire)

* "Anything related to action is rhetorical. [...] *The domain of rhetorics is an inbetween area where subjectivity and objectivity mingle with the help of language. The apparatus of rhetorics goes unnoticed and it only attracts attention when it is a re-used "oratorical" device, when it rings false, when it turns without gripping, artificially* [...]. Certainly neither I, nor the world, and not even language can be reduced to rhetoric. However, neither I, nor the world, and not even language would be what they are without it."

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ABSTRACT

This thesis analyses whether the criticism that 20th c economic theory is too abstract, and lacking in economic meaning as a consequence of being mathematical, is justified, from a methodological perspective that is epistemological in character (cf ch2 and Cheix, 1996). Using, firstly the 'external' historical approach, that compares Economics to the sciences (especially Mathematics chs5, 6, 7, 8); and, secondly, the semiotic approach, that inquires into the contribution of notation to meaning, the thesis examines the historical and cognitive *raison d'être* of mathematics in Economics.

The thesis identifies (chs1, 2) 20th c mathematical economics with model building and neo-classical theory. The main lines of argument are developed with reference to Jevons' Theory of Political Economy and Debreu's Theory of Value. This limitation is practical but not unnecessarily restrictive as the authors are major neo-classical writers, and mathematical economics has developed along the lines they envisaged. Further, neo-classical ideas have established themselves as paradigms of 20th c Economics, and have influenced theories in the social sciences and their mathematization.

It is shown that Jevons (ch5) used the symbolism, and in particular, the linearity property of differentials to unify economic *theory* and the sciences on the pattern of Physics. For him however, the mathematization of economics involved also empirical and experimental

inquiries using statistics. For the case of Debreu (ch6) it is shown how he used set-theoretic formalism and fixed point theorems to provide equilibrium theory with logico-mathematical content. This content is viewed as an axiomatic and deductive structure implying equilibrium.

The definitions of mathematical economic models discussed in Part 3 show that economics was mathematized through influences not only from Physics, but also from Logic, and, more widely from the 20th c (socio-cultural) trend of model building in science. It is argued that this latter trend is not exclusively, or even necessarily, rooted in neo-classical economics.

The semiotic analysis of chs 5 and 6 reveals how notations connect different interpretative levels ('isotopies') of mathematical theories, and how inconsistences may arise between these levels.

The general conclusion of the thesis given certain methodological *provisos*, is that mathematization, in itself, is not a cause of, or explanation for, the emptiness of economic theories.

PART ONE

FOUNDATIONS OF THE THESIS

CHAPTER 1

ELEMENTS OF THE HISTORY OF MATHEMATICAL ECONOMICS

"Mais on doit se demander de quel côté se trouve l'ambition la plus exorbitante. N'est-il pas plus prétentieux de se prendre pour une mémoire que de prétendre exercer un jugement? Du côté du jugement, l'erreur est une accident possible, mais du côté de la mémoire l'altération est d' essence."[1]

Canguilhem (1988)

INTRODUCTION

This chapter presents the reader with major methodological difficulties faced by contemporary historians of mathematical economics. The first section focuses on foundational issues in historical science.
The second section deals with the description of the historical sources of this study except primary sources. The latter are referenced as when they appear in the thesis. Furthermore, it recounts views of 20th c. historians on the history of mathematical economics. In addition to comprehensive collections of primary sources (Baumol et al, 1968; Baumol, 1991; Darnell, 1991a, b, c, d, e, f), we shall report the views on the history of mathematical economics developed by authors of monographs and specialised studies on mathematical economics. These are Theocharis (1961, 1983, 1993), Etner(1987), Zylberberg (1990), Ingrao *et al*(1987) and Israel(1996).

SECTION 1: HISTORIOGRAPHY OF ECONOMIC THOUGHT AT PRESENT[2]

At present, some authors are scrutinizing the way economists are writing the history of their subject (e.g. Backhouse, 1991a; Blaug, 1991; Mirowski, 1989, 1991; Mirowski *et al* 1994; Perrot, 1992 pp.7-60: 'Quelques préliminaires à l'intelligence des textes économiques'). At the same time not only is the role of the history of economic thought being criticized (e.g. Backhouse 1991a, Perrot 1992, Van Parijs, 1990), but also new sources are being made available to readers (e.g. Perrot 1992, Darnell 1991a, b, c, d, e, f, Bicquilley, 1804, and sets of fundamental texts have been recently published, e.g. the Pioneers in economics series at Elgar Publishing Company). These criticisms and these new insights also impinge upon the history of mathematical economics insofar as it is a subpart of the history of economic thought. We shall now develop explanations of why it is that methodologists are calling into question of the historiography of economics at present.

There are three reasons that explain why economists are calling into question the history of economic thought today.

The first is the 'crisis' of history exemplified by the controversies about the nature of both historical knowledge (which might be considered either as poetic or as scientific) and the history of the sciences (cf. Raison Présente, 1996; Ingrao *et al* 1987). Quite deliberately we do not distinguish between 'history' as a part of knowledge and 'history' as what happens. This is one core issue of the debates but it is beyond our scope. The way we handle the history in the concrete analysis of historical texts in mathematical economics was described in chapter 2 where the difference between 'external' history and 'internal' history is explained. An indication that debates in the philosophy of history have an impact on the historiography of economic thought is that Backhouse (1991a) reports the analysis of philosophers such as Richard Rorty in Objectivity, Relativism and Truth, on this subject.

Once polemical aspects of debates on history are set aside, we claim that one part of their underlying subject-matter is the close link between history and geopolitics. The Western tradition of historical knowledge, which historians usually date back to Thucydides (~460, ~400) was traditionally dependent upon the political domain. For example, many French historians before the 18th c. were state historiographers. History is now an academic subject taught in national institutions. Even though there is academic co-operation at an international level, geopolitical changes still affect historiography. So when the world is undergoing important geopolitical changes, as is the case today, (e.g. the collapse of the former Soviet Union and the re-formation of a

European Union), some historical facts or some historical eras become more relevant to modern thought than do others. This is either because they become relevant to an understanding of these changes, or because these changes reveal the relativity of previous historical analysis. Consequently, a critique of past historiography occurs and the methods or the topics of history as an academic discipline are re-evaluated.

To a certain extent, the history of economic thought is affected by these upheavals in history. There are two reasons for this: the debates on the foundations of historical knowledge; and the changes undergone by national economies.

The second reason for the criticisms directed against the history of economics is the 'crisis' in economic thought itself and that of most Western economies since the 1970s (cf. PB, 1993a and Gerrard, 1995, p.222) Usually when an academic subject or a particular theory is undergoing a crisis, the critique also affects its history. First such a critique can have heuristic virtues since it can lead to the exploration and the updating of forgotten ideas. Secondly, such criticisms are natural. One function of the history of a subject is to legitimate and explain its current form. In this connection Perrot (1992) suggests that studies on the Anglo-Saxon heritage of political economy are over-represented compared to those on the French tradition. And Backhouse (1991a) remarks that most historical studies are only dealing with market economies. According to Zylberberg (1990) the treatises on mathematical economics published in France in the first half of the 20th c. fulfil this function[3]. All these books:

> "aim at proving that mathematical economics is superior to traditional political economy. Hence these authors enlarge on the history of mathematical economics. It gives the reader the impression that the discipline is not as new as it seems to be and that it is rooted in a tradition. This explains why Ceva, Canard or Isnard, rather than Cournot are usually referred to as the starting point of mathematical economics.
> A surprisingly important number of pages is devoted to proving the relevance of the use of mathematics in political economy and also to answering objections to it." (Zylberberg, 1990, p.153, our translation)

For the purpose of accuracy it is worth mentioning that these debates about the history of economics are not caused by present historical circumstances only. Debates about the history of science are part and parcel of research in this field and in the philosophy of science as well, as Gaston Bachelard (1884-1962) and Imre Lakatos (1923-1974) for instance show. The repercussion on economics of the crisis affecting Western economies reveals a profound lack of convergence on the aim of economic knowledge as well as on the role of the history of economic thought. Such a lack of convergence about the aim of a science does not seem to exist in medicine. It exists in physics between 'pure' theory and technology or engineering but it seems that it does not generate as many debates as it does in economics. An approach to these debates is due to Van Parijs' (1990, p.17, our translation) who writes as follows:

> "I am not saying that a simplified reconstruction of the past, that of a species, of a nation, an institution or an individual is useless. It may help in defining and redefining an identity that one may sometimes take pride in or just put up with. Maybe this is the main rationale of historiography - from macro-history to biography, of which we are but little concerned here. However it seems to me that the role of such investigations ought to be limited as far as the rest of the social sciences we shall be concerned with is concerned. Indeed, if for instance, one is concerned with the advent of a socialist or a post-industrial society, then what is important is not to build up a conceptual apparatus that presents it as the "natural" consequence of an ongoing historical process, but it is rather to explore as rigorously as possible and without failing to differentiate between its desirability and feasibility."

Backhouse (1991a) is right when he remarks that issues in the methodology of the history of economics are issues in the methodology of economics. This is specially obvious in the particular case of mathematical economics as we shall now see.

For example, when authors believe that mathematics are not an essential feature of economics, they are likely tc hold the view that the history of mathematical economics is not very important to contemporary economics. They might hold this view on mathematical economics because they hold the ontological view that it is economic phenomena that are mathematical in essence rather than knowledge about them; or because they hold the methodological view that mathematical economic theories can be expressed without mathematics; or else because they hold the teleological view that the aim of economics is not to build 'abstract' theories such as mathematical theories are. Those who believe that mathematical economics is an important tradition are similarly met with obstacles about its history. Given a mathematised economic problem, it is sometimes hard to distinguish between its mathematical side and its economic side. On the one hand, economics sometimes uses, and has used, well established mathematical tools and results, so that mathematics appear to be either a processing device, a calculus a sieve for consistency, or from a logical positivist perspective, a well defined structure that can be empirically interpreted. On the other, it is the case that mathematical economic theorizing employing such techniques as maxima and minima analysis, game theory or the analysis of financial markets poses proper mathematical problems. In such cases both mathematical and economic inquiry are enhanced (cf. Debreu, 1986) by the development of mathematical economics. These two sides of mathematical economics echo questions of the philosophy of mathematics. Is the function of mathematics algorithmic or is it a particular way of theorizing? What is the role played by symbolism in achieving these two functions? In short, from a chronological point of view, mathematical economics sometimes looks like applied mathematics. Sometimes it consists in a true development of mathematics. In the first case the subject-matter of the history of mathematical economics is the transfer of knowledge, which may also concern the sociology of knowledge. In the second case it concerns typically biographical inquiry and the philosophical analysis of theorizing.

For these reasons, *synoptic* historical views on mathematical economics are dependent on the historians' views on these matters as well as on their views of the differences between subjects such as mathematics, physics, philosophy and economics. Mirowski's (1989) analysis of the relationship between physics and economics may be taken as exemplifying such a dependence. He takes the point of view of what he terms 'universal history' and postulates that there are cognitive 'metaphors'. He identifies variants of these metaphors in physics and in economics and compares their efficiencies.

In addition to standard methodological problems about the interpretation of historical material - some of which Stigler (1965b) describes, the methodologist is faced with small amounts of this material. This is the third reason for the criticisms addressed against the history of economic thought. Here is an appraisal of the importance of historical sources for the history of economic thought and of their neglect, by the French historian Perrot (1992, p.9):

> "Considering the large collection of archives available [...] economists have been very selective. In fact, they rely on about ten books whose authority is not restricted to a particular specialized subfield, as well as on two or three "secondary" works. From the XIXth century onwards, the history of the subject has been built upon far less than ten per cent of identified sources."

Similarly, Breton *et al*(1991a)pinpoint three 19thc trends (the liberal, the cooperatist and the nationalist) that have not been sufficiently studied given their importance on the history of French economic thought. Mirowski's (1989) Herculean endeavour to reconstruct the history of economic thought proceeds from an identification of this lacuna and it seems therefore to be a feature both of French and of Anglo-Saxon history.

It hits especially mathematical economics for which it seems that there is a bibliographical gap from 1911 to 1961:

> "We have, then, Jevons' extended bibliography in the 1911 edition of TTPE, Fisher's 1897 bibliography, and Theocharis' 1983 bibliography of work prior to Cournot. Together, these are what was, at the time of their compilation, thought to represent 'mathematical economics.'"(Darnell 1991a, p.xvii).

The book of Theocharis to which Darnell refers is in fact the second edition of a book first published in 1961.

Consequently, historical overviews of the methodology of mathematical economics are very likely to be ultimately limited, however exhaustive the author wishes to be.

Furthermore, this lacuna is a problem for those involved in the philosophy of economics. In addition, the bibliographical core of mathematical economics, as Darnell (1991, a, p.xviii) points out is hard to obtain. There is however a positive aspect to this lacuna: it gives a reason for additional studies on mathematical economics. Thanks to Darnell (1991a, b, c, d, e, f) and the Elgar serie already referred to, this additional drawback in the literature is being remedied.

In this context the critical assessment of the main authoritative historical sources of this study is very important. This is the subject-matter of the next section.

SECTION 2: ASSESSMENT OF THE HISTORICAL SOURCES OF THIS STUDY

SECTION 2@1: COLLECTIONS OF PRIMARY SOURCES

Baumol *et al*(1968) collected and occasionally translated thirty four articles and excerpts of works concerned with mathematical economics to the exclusion of those dealing with arithmetic and geometry. The texts are classified by economic subject-matter. The papers are either original works, or translations of original works or secondary literature. They date from 1738 up to 1965 and they were originally written in Latin, French, German, Italian or English. They divide the history of mathematical economics into two parts: the first landmark in terms of historical era is the end of the 19th c. and the exemplar economic figures are Jevons (1835-1882) and Walras (1834-1910). Before then according to Baumol and Goldfeld, mathematical economics studies were not fruitful and progress in this field was erratic. Further, according to the authors, they are a useless curiosity for contemporary economists. From then on, the diffusion of mathematical economics is continuous. The editors exemplify the continuity of the history of mathematical economics with the following authors: Jevons (1835-1882), Walras (1834-1910), Edgeworth (1902-1950), Ramsey (1903-1930), Wald (1902-1950) and von Neumann (1903-1957). Overall, mathematics contributed to clarifying economic ideas and explanations.

The editors criticise approaches to the history of economic thought that focus on the genealogy of economic schools of thought. They suggest that such genealogies are arbitrary, and it seems that they favour a cumulative and linear approach to the history of economics.

The editors do not define what they mean by mathematical economics. However, from the way they picture the history of mathematical economics, it seems that they refer essentially to the use of mathematical symbolism in economic texts. Besides, it seems that the criterion they use to assess the progress of mathematical economics is the 'complexity' of the mathematical techniques in use.

Baumol's view evolved with time. In the foreword he wrote to Darnell's anthology (Baumol, 1991), Baumol's view is altered in two major ways. First, he points to the fruitfulness of pre-19th c. mathematical economists as a source for contemporary analysis. Secondly, he plays down the state of mathematical economics at the end of the 19th c. Not only was the prestige of this activity limited but its proponents limited its scope on the ground that economic behaviour rests upon psychology, social organisation and social practice.

Darnell's (1991a, b, c, d, e, f) work aims at increasing the range of sources available and thereby he is contributing to the filling in of the lack of historical sources that we mentioned previously (cf. chapter 2, section 1). He selected 85 pieces of work from amongst the following bibliographies on mathematical economics. (a) the bibliography published in 1892 by Irving Fisher, which is an extension of the list published by Jevons in 1879;(b) that of N.T. Bacon and that of I. Fisher both published in 1897; (c) the 1911 extension of Jevon's; (d) these of Theocharis published in 1961 and 1983, (e) and that of Baumol and Goldfeld dated 1968. The criteria for selection were first that the texts were articles; second, that they were first published in the 19th c.; and finally that they were first published in English. By contrast with Baumol *et al's* (1968) anthology all articles appear in full length form. Darnell excluded 20th c. works because they are accessible and because since the 1930s, the value of the mathematical method in economics has ceased to be controversial. Darnell considers that there are two periods in the history of mathematical economics, and the frontier he draws between these two periods is the Second World War. The reason why he chooses this date is not clear. It is half way between Stigler's (1964) choice which is the 1920s and Boland *et al* (1986) which is the 1960s. The reason for choosing this date might be that

Darnell subjects time-division in the history of economics to general history. This might be the case if he considers that because economics is an empirical science, theories ought to be pictured in the light of the underlying economic context in which they are worked out. But the reason might also be that he considers that, generally speaking, the orientation of economic thought changed at that time. These latter remarks aim to show how history, epistemology and methodology are correlated to one another, in terms mentioned above (cf. chapter 1). Before these dates, mathematical economics was a tiny sub-part of economics, and one whose impact on the discipline (even up till 1930) was slight. Even though the mathematics used in economics was simple (algebra, geometry, diagrams), they constituted a "language barrier" within the discipline. This explains why some mathematical economics, which history has shown to be efficacious for economic theorizing (e.g. Hicks <u>Value and Capital</u>, 1939; Marshall's <u>Principles</u>, 1890) first appeared in footnotes or in appendices. By contrast, mathematics today no longer forms a "language barrier" to economists but remains one between economists and the layman. In between, 19th c. mathematical economists fought for their sub-discipline, and mathematics were integrated into the academic syllabus in economics.

Consequently, Darnell's criteria for

splitting the history of mathematical economics seems to be sociological in contrast to that of Collison-Black in his preface to Jevons (1871), and that of Baumol *et al* (1968). For the latter two, the criterion seems to be the relevance of past texts, whose authors are then called the "precursors", to contemporary analysis.

SECTION 2@2: SPECIALIZED SOURCES ON MATHEMATICAL ECONOMICS

Theocharis (1983, 1993) considers that the two landmarks in the history of mathematical economics are 1838 and 1871. Those prior to Cournot's essay Recherches sur les Principes Mathématiques de la Théorie des Richesses (1838) are termed 'early mathematical' economists. This 'proto-historical period' for mathematical economics opens up a period of transition, that runs from 1838 to 1871 when Jevons' Theory of Political Economy was first published. The survey of his bibliography suggests that Theocharis explored two main bibliographical sources on mathematical economics namely the 2nd and 4th editions of Jevons' Theory of Political Economy respectively published in 1879 and 1924, as well as Irving Fisher's 'Bibliography of mathematical economics from Ceva to Cournot' which is in all likelihood included in the 1929 edition of the translation of Cournot's Recherches(...) by N.T. Bacon. In addition, he expanded this bibliography. He jumps from Aristotle to the 18th c. He refers both to the 18th century's and the 19th century's editions of the books he mentions, as well as to the 19th and 20th century reprints of earlier editions. Theocharis' books are considered to be authoritative on the history of mathematical economics and Theocharis is quoted by Baumol et al (1968), Darnell (1991a) Perrot (1992) and

Mirowski (1991). By contrast with Baumol et al (1968), the selection of mathematical economists is not restricted to their using a particular type of mathematics.

Etner (1987) refers to extracts of his book dealing with one aspect of French mathematical economics, namely "calcul économique"(economic calculation). He defines it as the branch of political economy that aims to justify economic and social policies. It provides a monetary estimation of the economic and social benefits of these policies, and criteria for decision making based on mathematical formulae. The book consists of both first hand analysis of the contributions of famous philosophers, economists, learned persons and engineers, as well as quotations from these contributions; however, business economics is out of his scope.

The book covers the post-16thc historical era, but we shall be concerned with Etner(1987), that is, with his research on the post-18thc period. According to Etner(1987), economic calculation developed in the 19thc as a result of research in civil engineering and not as a result of theoretical studies of society in general. In addition to the works of Jules Dupuit(1804-1866) and Clément Colson(1853-1939), whom he views as the most well known pre-1945 contributors to economic calculation, he studies the works of many others. He insists on the role played by the building of transport and also by the institutional position of French government in the development of economic calculation. French government were involved either directly or indirectly in the building or the operating of transport networks. This is because they acted either as financial investors or as controllers (technical, financial or commercial) of building companies or concessions holders, or else because they were levying taxes. Consequently, many state engineers -especially those who studied at the Ecole des Ponts et Chaussées- developed thorough costs analysis, detailed traffic and earnings estimations. They were also concerned with measuring the *utility* of civil engineering networks. It did not refer to the theoretical economic concept of utility but to the comparison of the social and economic advantages of the networks with the costs incurred. In connection to railway economics in particular, the mathematical tools and methods that were used came from either engineering physics, and were a combination of theoretical developments and experimentations, or from analytical accountancy, or else they were interpretations of statistical data in an econometric type of way (e.g. estimations of parameters of linear formulae). The engineers were neither applying nor developing economic *theory* in their works. Moreover, they were usually ignorant of it. However, it happens that they came to use similar mathematical technics and economic concepts that were being developed in economics during the same period. In costs analysis in particular, they discovered the importance of marginal costs.

Zylberberg (1990) is concerned with giving an account of the state of mathematical economics in France between the 1871 war and the first world war. His sources consist of articles, textbooks, treatises as well as theses. Most of them are written in French. Their subject-matter is mathematical economics or the methodology of the social sciences. In addition, his bibliography contains a few contemporary studies in the history of mathematical economics as well as in the methodology of economics. He insists on the institutional background of mathematical economics. This defines his historical standpoint.

He considers that the first landmark in the history of mathematical economics in France is the beginning of the 19th c. This new science was born with Cournot and Canard. Then he mentions Esmenard du Mazet, Mathieu Wolkoff and Jules Dupuit who considered that mathematics was the appropriate form of economic theory. In his bibliography however, only Dupuit's name appears. The second landmark in this history is the end of the 19th c. and the crisis of political economy. Zylberberg identifies two traditions in the history of mathematical economics between 1870 and 1914 as well as isolated individuals. One tradition consists of the followers of Walras such as Albert Aupetit (1876-1943), Etienne

Antonelli (1879-1971) and Hermann Laurent (1841-1908). The latter also belongs to the second tradition , that of actuaries. From 1872 to 1880, there is no connection between the two traditions. The other members of the actuary tradition are Léon Pochet, Emile Dormoy, Septime Avigor, Fontaneau, as well as Emile Cheysson (1836-1910). The latter also belongs to the third tradition, that of engineer-economists. In this tradition, there are also Clément Colson (1853-1939) and Marcel Lenoir (1881-1927). Isolated individuals who contributed to the development of mathematical economics are Jules du Mesnil-Marigny(1810-1885), Gustave Fauveau(1834-?) and Louis Bachelier (1870-1946).[4] At that time, there are definite developments in mathematical economics. However, mathematical economics is less successful than in other countries.

Zylberberg is not definite in identifying the third landmark. It is either 1914 or the 1930's. A defining feature of this turning point is the convergence of two historical trends that were previously separated. This convergence coincides with the 'professionalisation' of economists. They are the abstract-deductive trend and the pragmatic-inductive trend. They converge into the econometric tradition.

We shall now report the historical reasearch of Inrgao *et al*(1987). The book by Israel(1996) is concerned with the mathematisation of the sciences in general but we shall also refer to its parts that are concerned with economics (ch6 and ch22). Their research is based on the analysis of first hand economic literature from the 18thc onwards, literature on the history of economic thought and 20thc philosophical literature. The authors are concerned with explaining the evolution of the mathematical side and the economic side of general equilibrium analysis in terms not only of the history of economic thought but also in terms of the history of the sciences. In addition to expounding the fundamental contributions to the theory of Walras and Pareto, they assess the works of pre-Walrasian authors, Hicks and 20thc economics working in the United States. They also show that there are many historical trends that influenced this evolution, amongst which the mathematisation of the social and biological sciences, and the evolution of the physical sciences and their crisis. As a result, this evolution is complex and the mathematisation of economics is atypical compared to other social sciences.

Their view is that from the 1930's onwards, there is an increasing tendency for the economic side and the mathematical side of general equilibrium analysis to evolve separately. This separation has ultimately resulted in a contradiction between the theory's aims and its hypotheses. The aim of the mathematisation was to answer the questions of the existence, the uniqueness and the stability of the state of equilibrium of an economy. The mathematisation of the hypothesis if the theory and their mathematical treatment have shown that these three features were not characteristic of the idea of equilibrium as defined by the theory. The only positive answer to these questions was the Arrow-Debreu proof of the existence of an equilibrium state. The main reaction of economists to these negative results was to adjust partially the theory's hypotheses, in an attempt both to define the conditions under which unicity and stability occurred and to preserve the core hypotheses of the theory. For their part Ingrao *et al*(1987) advocate for "a *thorough* reexamination of the theory's hypotheses, i.e., a paradigmatic revolution".

It emerges from the previous survey that most economists and most of the sources they refer to in the history of mathematical economics, converge in focusing on the 19th c. as an important period in the history of the subject. Before then, works in mathematical economics were scarce, and afterwards there number increased and became a great proportion of economic literature. When it comes to qualifying this historical development and assessing its importance to economic thought the authors' view do not converge anymore. It should be noted that the search for cross-references in the bibliographies of these sources reveals that their works were often independent of one another.

NOTES TO CHAPTER 1

1 The question must be asked of where the most outrageous ambition stands. Is not there more conceit in claiming to be a memory than there is in meaning to formulate a judgement? Potentially, a judgement may be misleading, whereas alteration is the essence of memory.

2 Blaug(1991) provides material for the understanding of historiography of economics. It consists of fourteen articles originally written between 1964 and 1986. In the first article, Popescu(1964) comments on references that include non-European references. The references are of interest for general and specialised studies into the historiography of economics from the mid-18th c. onwards in various countries. These references are studies into the history of economic thought or bibliographical studies on economic science. The references start with the work of Du Pont de Nemours (1768,1769). However, an earlier author is mentioned, the German Georg Heinrich Zincke.

3 These treatises are: Introduction Mathématique à l'Etude de l'Economie Politique (1911) by Leopold Leseine and Louis Suret; Les Applications Mathématiques à l'Economie Politique (1912) by Pierre Boven; Théorie Mathématique de l'Echange (1913) by Antonio Osorio; Principes d'Economie Pure (1914) by Etienne Antonelli; Les Mathématiques Appliquées à l'Economie Politique (1914) by Waoislaz Zawadski; L'Emploi des Mathématiques en Economie Politique (1915) by Jacques Moret.

4 The biographical information that follows is a summary of that contained in Zylberberg (1990). For information on Louis Bachelier, see chapter 2, note 3. To the exceptions of the following authors who appear in the books subsequently mentioned, these authors do not appear in the indexes of the historical sources we surveyed: Marcel Lenoir, (Morgan, 1990; Stigler 1975a) Aupetit (Theocharis, 1983), Antonelli (Theocharis, 1983), Cheysson (Theocharis, 1993), Colson (Theocharis, 1993), Du Mesnil-Marigny (Theocharis, 1993), Fauveau (Theocharis, 1993).

Albert Aupetit (1876-1943) is the first French economist who used Lagrange multipliers in economic optimization problems. He improved the formal aspect of Walras' theory and he added to it. In particular, he studied equilibrium in the production sector which neither Walras nor Pareto studied. He introduced production functions with substitute factors. He also attempted at measuring "value".

The theoretical contribution of Etienne Antonelli (1879-1971) to mathematical economics is limited. However, he contributed to it in so far as he answered metaphysical and epistemological questions addressed against it.

Herman Laurent (1841-1908) contributed to risk theory, to which he applied probability calculus. By contrast with Walras, he had an "econometric approach" to statistical data. He considered that their role is both to suggest economic-theoretical premises and to test economic conclusions. However, he considered that they had not yet reached a mature stage. He developed and he improved mathematical statistics applicable to economics such as least square methods, which he, however, applied only to insurance problems. In addition, he gave a clear explanation of the ordinal aspect of utility function applied to the consumer equilibrium.

Léon Pochet wrote an article in 1873 on stock-markets. According to Zylberberg (1990), he explicitly associates actions of brokers to games. However, these games involve one individual only. He also introduces mathematical gain functions.

For taxation purposes, Emile Dornoy defined the value added to a commodity by economic agents.

Septime Avigor studied the price system of an economy in a way similar to Walras general equilibrium analysis. However, Avigor does not mention Walras. In addition, marginal utility does not play a basic role in Avigor's system, and the demand functions are not deductible from it.

Fontaneau wrote many articles around 1880 on the theory of value which are not of an outstanding originality. However, he is the first economist both to attempt at describing price variations as a mathematical function of the difference between supply and demand and also to write explicitly a production function.

Emile Cheysson (1836-1910) created the Institut de Actuaires Français in 1890. He developed the use of geometry in statistics for practical purposes. He and Walras exchanged a correspondence on the rationale of mathematical economics and on its development.

Clément Colson (1853-1939) was a public servant. According to Zylberberg he played an essential role in the development of mathematical economics because he trained the French mathematical economists of the 1930s. He was first and foremost a practitioner and not an academic. He understood economic laws as statistical laws. He used not only graphs but also "models" of the economy, which he described in terms of partial and general equilibrium. In connection to partial equilibrium he considered that the law of supply and demand was a manageable approximation of reality.

Zylberberg (1990) compares the work of Marcel Lenoir (1881-1927) on statistics with that of the American Henri Moore, even though the Frenchman was less known. Lenoir only published one book, Etude sur la Formation du Mouvement des Prix, in 1913. It contains the method of relative changes and also trend ratios, which Moore uses at a later date only. His work echoes later works in econometrics. In particular, he aggregates individual statistical demand functions. He also uses regressions with more than one variable. In addition, he draws the reader's attention on what will be known later as the identification problem in the econometric literature. However Zylberberg (1990) does not consider that he is an econometrician because his approach does not consist in *testing* economic theories with statistics.

Jules du Mesnil-Marigny (1810-1885) was a maritime engineer. He and Walras had planned to write a treatise on political economy. It was his ambition to make political economy an exact science, that is a rigorous and quantifiable science. He considered that mathematics was the adequate instrument for achieving this purpose. In particular, he wanted to prove that protectionism and liberalism were not contradictory.

Paul Gustave Fauveau (1834-?) was the only mathematical economist amongst those mentioned by Zylberberg (1990) to be recognized by the then orthodox school of political economy in France. However he is ignored by historians. He wrote on mathematical economics from 1864 to 1886. Several aspects of his work were innovative. For example, in taxation theory, he identified taxes with a risk bonus given by citizens to the state. He also anticipates the theory of index numbers. According to Zylberberg (1990), Fauveau provides the first dynamic model with continuous time and he is the first economist who uses variations calculus.

CHAPTER 2

SUBJECT-MATTER AND METHODOLOGY OF THE THESIS

Subject-Matter and
Methodology of the Thesis

"Si les chiffres ne gouverenent pas le monde, ils indiquent au moins la manière dont il est gouverné"

Goethe

INTRODUCTION

The subject-matter of this thesis is the use of mathematics in contemporary economic knowledge. Mathematics, including mathematics that may appear "sophisticated", is a significant component of this knowledge, and since this knowledge is used in political decision-making in various countries, it thus contributes to social order. Consequently, whilst the subject-matter of this thesis "abstract" it is connected to pragmatic social issues.

In this thesis, a semiotic epistemological approach to the subject-matter is taken, which involves historical studies. However, this thesis should not be considered as a study in the history of economic thought in the orthodox sense. It is complementary to it but it differs from it since the historical dimension is subordinate to the semiotic analysis[2].

We shall first define our general historical and philosophical approach to the subject-matter and the specific research program of the thesis. Then, basic concepts of the philosophical framework we shall use are explained.

SECTION 1: TOPICS, METHODOLOGY AND AIMS OF THE THESIS

In this thesis, we equate contemporary mathematical economics with the general survey of Arrow *et al* (1981, 1982, 1986) and Hildenbrand *et al* (1991), and with its definition by Stigum (1990). These books refer to a wide range of mathematical tools including probability and non-standard analysis.[3] By mathematical economics therefore, we refer to the corpus of mathematical economic theory, to the corpus of econometrics, as well as to original logico-formal frameworks of economic theory such as Stigum's. We also follow Chiappori(1992), who notices that mathematics is not merely a 'conceptual tool' for contemporary economic knowledge, it also an "empirical tool".

We are interested in the "mathematisation" of economic ideas from an epistemological point of view that comprises a historical approach. By "mathematisation" we mean the historical and epistemological relationships between the "mathematical form" and the "economic content"[4]. According to Israel (1996), the history of mathematical modelling in general, which constitutes an important part of the mathematisation of the sciences, still has to be written. In addition, according to Franklin (1993), epistemological studies neglect important newly mathematised scientific fields (economic fields included, cf. chapter 7, section 2).

There are different ways of approaching the history of a science. One way may be called 'internal history'[5]. This involves the historical study of ideas and methods used in a particular academic discipline with references only to the literature that this discipline acknowledges to constitute its corpus. The internal approach to history prevailed in economics until recently, as Mirowski *et al*(1994, p246) notice. By contrast, 'external history' aims at setting the history of a particular science within the history of other sciences, and as far as possible, within economic and social history. For example, historical studies in the sociology of knowledge belong to external history.
This thesis falls within the province of the external history of mathematical economics. One advantage of this historical approach is that it includes the study of the contributions of mathematicians and other professionals to mathematical economics, and the contributions of economists to the development of mathematics.[6].

The philosophical approach to the subject-matter consists essentially in taking up semiotics as the school of thought *influencing* our methodology for reading historical texts in mathematical economic theory. Semiotics is a tradition in both linguistics and in philosophy, that provides a general theory of meaning. Authorities and concepts in semiotics are presented in the section that follows (Chapter 2, Section 2).

Specifically, this thesis is concerned with two topics in the history of mathematical economics. One is the Neo-classical school of thought in economic theory, the other is the concept of "mathematical model" in economics. Both are important for understanding contemporary mathematical economics.
The Neo-classical school of thought has long been equated, not always accurately, with mathematical economics. Many economists share Carlo Benetti's (1995) view that the Neo-classical General Equilibrium analysis is "the norm out of which economic "science" develops". In addition, most studies in the epistemology of economics take it as their main subject-matter. For example, this is the case of Hahn *et al* (1979), Hausman (1992), Hollis *et al* (1975), Nelson (1984). There are two reasons for the predominance of the Neo-classical school of thought in philosophical literature. Firstly parts of this literature belong to the Marxian tradition, which develop an economic theory that competes with Neo-classical theory to a certain point[7]. Consequently, an important focus of Marxian epistemology of economics is Neo-classical economics. Secondly, Neo-classical analysis influenced 20th c. Western economic theory as a whole (regardless of schools of thought and mathematical economics included), so that much of modern economics is neo-classical economics[8]. Furthermore,

Ingrao *et al*(1987, px) and Israel(1996, pp.313-314) consider that the mathematization of economic science itself originally developed within General Equilibrium Analysis and that mathematization is its rationale.
The importance of General Equilibrium Analysis for contemporary mathematical economics and the philosophy of economics does not imply that it is well defined. Its status is adequatly defined by Punzo(1991, pp2-3):

> "What is commonly called the study of general equilibrium is neither very clear nor agreed upon. Is it an analysis, a theory, a sequence of models, a metatheory? Nevertheless, everybody seeems to agree that it is the fundamental economic teory"

Mathematical models in general play an important part in the production of scientific knowledge today including economics. In addition, they are important in social and political decision-making.

After having defined the two topics of this thesis, we shall now turn to defining our methodology.

The first topic, Neo-classical mathematical economics, is treated through the study of two treatises in which the use of mathematics coincides with a gain in generality in the approach of their subject-matter (cf. Part 2). They are Jevons' Theory of Political Economy and Debreu's Theory of Value (cf. Chapter 5; Chapter 6). Both are popular representatives of mathematical economics. As such, they have a place in the 'internal' history of economic thought, and one which is not disputed in the literature.

The methodology of our analysis of these two seminal works is as follows. It deals first with the mathematics they use (e.g.: finite differences, Calculus, Topology of real vector spaces). It comes under the heading of 'external' history both with regard to the sources it refers to and with regard to its scope. Not only does it refer to the texts themselves but also it links them to texts by the same authors in which methodological views are expounded. In addition, it aims to replace the mathematics they use in the broader context of the development of both the social sciences and mathematics (cf. Chapter 3; Chapter 4). Consequently, our historical methodology is similar to that of Punzo(1991) and Ingrao *et al*(1987, Preface). Information on this matter is mostly derived from secondary sources on the history of mathematics, and from encyclopaedias as well as from mathematical textbooks.

In each case, the aim of the analysis, apart from the description of the mathematical tools used, is to answer such questions as:

- has this use of mathematics contributed to the development of mathematics themselves?
- do the authors consider that mathematics is a "conceptual tool" or an "empirical tool" for building up economic theory?
- what, if anything, do the authors count as an "economic result" as opposed to a "mathematical result"?

- what are the arguments the authors put forward to warrant their using mathematics in economics theory?

The second topic, the concept of mathematical economic model, is dealt with from a different standpoint (cf. Part 3). We are looking for the lexical origin and the historical origin of the concept of a model in economic literature (cf. Chapter 7; Chapter 8). Necessarily, we rely on the research of others on this matter. We communicate their results and what is more, we explore additional bodies of knowledge in order to add to these results. The 'external' feature of this historical research is that the search for the lexical origin involves a comparison with other scientific disciplines (cf. Chapter 9).

The philosophical aspect of the methodology consists in applying semiotic analysis to the reading of the texts of Jevons and Debreu mentioned above.

After having explained our methodology we shall now turn to stating its aims.

First it is expected that this study will reveal whether semiotics as a methodological *framework* discloses new insights on the subject-matter. In addition, it might reveal how this methodology can be improved and extended to topics other than Neo-classical economics.

Secondly, it is expected that the study will reveal elements for answering the following question:

does the idea of "mathematical economic modelling' - which refers today to the aim of a large part of economic theorising, come from the Neo-classical school of thought which exerted a great influence on economic theorising?

In addition, even though in this thesis, we are interested in other sciences only in so far as they shed light on economics, this study may contain elements for methodologists interested in social sciences as a whole. For example, readers interested in the methodology of the social sciences may find that this study contributes to answering the question of whether the domination in the social sciences of explanatory frameworks borrowed from economics is connected with the "mathematical nature" of this model. This domination is acknowledged by Van Parijs (1990) and Hausman (1992), who argue that it is based on the Neo-classical theoretical framework. Consequently, assuming that Neo-classical economics is a good representative of the use of mathematics in economic theory, the previous question makes sense.

SECTION 2: PRESENTATION OF SEMIOTICS

SECTION 2@1:GENERAL PRESENTATION

In terms of academic disciplines, semiotics is part of linguistics. However, many disciplinary influences from the sciences as well as from the arts, are evident in the works of semioticians. So far as the subject-matter of semiotics is concerned, it consists, as its etymology suggests, in the study of signs, without restriction to vernacular languages. Historically, the Collected Papers of the American Richard Sanders Peirce (1839-1914) is referred to in contemporary semiotic studies as the main contribution to the foundation of semiotics. Eco (1984), who is concerned with the historiography of semiotics, shows that in previous centuries semioticians are to be found amongst philosophers, especially those interested in the philosophy of language. One characteristic of semiotics is its diversity. It can be considered as Eco (1984) and Anderson *et al* (1984) point out, as a perspective, a methodology or a field. As these authors also point out, this diversity is at present an important issue of debate amongst semioticians.

Before explaining a selection of basic and methodological semiotic concepts that are relevant to our study of mathematical economics, let us first explain the reasons why we believe that this framework (as it is dedveloped by Umberto Eco) might appropriately be considered in the context of certain methodological problems.

Some of these reasons are at most a matter of mere coincidence: semiotics puts forward ideas about history, the sciences and symbolic representations that share interests with ours (cf. Cheix 1996).
For example, the semiotic perspective recognises that the sciences are a cultural phenomenon, and also a hallmark of Western culture (cf. Cheix 1996, section 4), but at the same time, that the sciences entail something more than culture-related values and ideologies. Thus:

> "Science appears to be the more dynamic and emergent dialectic of all constraints, temporal and spatial, informational and energetic, and may be more apt to transcend culture, although it rarely if ever does."(Anderson *et al* 1984, p.16)

The semiotic perspective seems also to consider that written communication of results and proofs and also texts are a defining feature of these sciences (cf. Cheix 1996, section 4, first philosophical 'landmark')[9]. Another similarity between semiotic studies and our 'philosophical landmarks' is a common interest in interdisciplinarity. This means that it is considered to be fruitful for a particular discipline to take into account what other disciplines have to say about subject-matters similar to its own[10]. An additional coincidental appeal of semiotics is that it is committed to a view on science that gives meaning to an idea of scientific progress, or evolution of human knowledge, that explicitly asserts its non-identity with historical evolution and with theories about the evolution of species. Semioticians strongly emphasize the fact that evolution is a part of a theory and they do not take it as an ontological system.

Semiotic analysis has other advantages of a more fundamental nature.

For example, it provides a perspective from which one can talk about science without having to commit oneself to a theory of science, but "only" to a theory of meaning (cf. Cheix, 1996, section 4). This theory of meaning uses the idea of linguistic competence. This has the advantage of asserting the centrality of the problem of the relativism of knowledge, and, more broadly speaking, problems arising from scepticism. It is especially to be noted that the theory or the perspective does not get rid of these problems.

A third kind of reason for embracing a semiotic perspective is the very nature of our subject-matter, mathematical economics, and the issues which have been raised about it in the methodology of economic literature.

Some of these issues are brought about by the dual nature of mathematical economics, a point to which we now turn. Mathematical economics can be described in semiotic terms as a compound of two systems: one consists of mathematical signs, the other consists of signs of vernacular languages. It is clear that the dual nature of mathematical economics is the cause of part of the debates that bring into conflict realists and antirealists[11]. Although the methodological *problem* these debates are concerned with today is not clearly identifiable or unique, and although they do not seem to be targeted on mathematical economics specifically, as seemed to be the case in the 1950s, certainly one of its facets concerns the interpretation of mathematical formalism in economics. Other aspects of mathematical economics also lead one to expect new insights from the adoption of semiotic analysis in comparison to say, formal logic analysis. Indeed, Mahieu (1989) has argued that contemporary formal logic do not allow one to consider mathematical economic theories as clearly identifiable logical objects(cf. Chapter 6, section 2, note 8). Consequently, one can expect semiotics to provide interesting new insights into mathematical economics that are in harmony with current issues of inquiry in the methodology of economics.

A second advantage of semiotics so far as this thesis is concerned is that, contrary to what is usually understood to be the case in vernacular language and in philosophy, semiotics does not consider that oppositions between the formal and the empirical, the abstract and the concrete, induction and deduction etc.are absolute (cf. reprint of Anderson *et al*, 1984, p.14, Table 1).

Table 1. Fractures in knowledge arising from the division of scientific labor

Imputed discontinuities	*Some disciplines affected*
living - nonliving	natural sciences - physical sciences
animal - plant	zoology - botany
human - alloanimal	social sciences - natural sciences
verbal - nonverbal	linguistics - ethology
Disregarded continuities	*Some disciplines affected*
space - time	ecology - evolution
matter/energy - information	ecology - ethology
	social sciences - humanities
	economics - psychology
macroscopic - quantum	ethology - biochemistry/astronomy
Regularly-negotiated boundaries	*Some disciplines affected*
inside - outside	all
self - other	
digital - analogue	
episodic level/storage - continuous flow	all
quantification - qualification	
analysis - synthesis	
abduction - deduction - induction	all

When such categories are taken for granted, one is sometimes led to caricature the pros and cons of the use of mathematics in economics. For example, mathematical economic theory is often put exclusively on the side of "the formal" and it is said that it lacks "empirical content" or "evidence". To contrast "the formal" with "the empirical" thus comes down to neglecting the connection between mathematical economic theory and "empirical" statistics. In addition, this opposition results in the disposing the problem of the production of economic data. Similarly, Nelson (1984, p.40) notices that this opposition is a drawback: "The sharp, evident division between theory and evidence in economics suggests that economists may have been led astray by a positivistic philosophy of science, and a crude one at that." (our emphasis). In this connection, Nelson (1984, chapter 4: "The Micro/Macro Interface: Foundations, Reduction, and Unanswered Questions" pp.125-165) has shown that "empirical" micro-economic statistics are currently built up out of macro-economic statistics by means of computational procedures that resort to "formal-mathematical" micro-economic *theory*. This opposition between the formal and empirical may also lead to the omission of the potential historical and heuristic co-development of the "mathematical form" and "the economic content". Similarly, the opposition between "induction" and "deduction" limits the understanding of the research programs of traditions in mathematical economics such as econometrics (cf. chapter 2 note 16; chapter 3, section 2@2).

After having considered the relevance of semiotics to the methodological approach to mathematical economics, let us point out that, conversely, economics actually has its place within semiotic studies both as a specific semiotic system and as a field of application of general semiotics (cf. "Economics" entry in [7.1] and Eco, 1984, p.11). In the Western culture we are concerned with, it can be considered that economics is a specific semiotic system because it is used as an important guide for collective and individual social behaviour. It is a specific semiotic system also because it can be considered as a kind of logic, which can be applied not only to the study of the economy *stricto sensus*, but also to other domains[12]. So far as mathematical economics is concerned, it is less philosophically and methodologically committing to consider it as a compound of sign systems, rather than as something such as a theory, a hypothesis, or a body of knowledge that is translated into the language of mathematics, as some economists believe. This is so in the case of Samuelson (1947, epigraph by the physicist Josiah Willard Gibbs, 1839-1903) for instance. A hypothesis that one can posit is that the view that mathematics itself is a language is one cause of the separation of the history of mathematical forms from the history of content in the history of mathematical economics. The works of Blaug (1990) and Schumpeter (1954) on the history of Economic Thought, according to Zylberberg (1990), are representative of this trend. Because we have an interest in intra-scientific epistemology(cf. Cheix 1996), and because we want to study the interrelation of the form and the content of knowledge, the semiotic perspective on mathematics seems a better framework to start with than the linguistic one.

SECTION 2@2: SEMIOTIC CONCEPTS

We are now presenting three basic semiotic concepts: that of 'sign', that of 'inference', and the concept of 'cultural unit'.[13] They make clear in particular why semiotic categories concern the basis of knowledge (cf. Cheix 1996, section 4). In presenting these ideas, we are following Eco (1984, chapter 1: Signe et Inférence, pp.17-62) as well as Eco (1968, section A, Chapitre 2, III: Le signifié en tant qu' "unité culturelle", pp.63-65) but the economic examples as well as the commentary, are our own. In addition, in connection with written sign systems and with our analysis of texts in particular, we use the following semiotic terminology as well. We define 'expression', 'content' of an expression, 'semiotic function', 'level of expression' of a text, 'level of content' of a text as Alain Herreman (1996b) does. However, we differentiate ourselves from Herreman insofar as the definition of 'isotopy' is concerned, for in this case we follow Michel Arrivé (1973) instead.[14]

Historically, semiotic studies have identified two features according to which something can be called a sign rather than, for example, an object: it must be part of a process of both substitution and interpretation. The leading characteristic of contemporary semiotics is that it considers these two processes to be of the same kind; both are the result of an inference. Depending on the strength and on the natural, sociological, cultural etc. origins of the constraint upon the inferential process (or, to use linguistic terms, on the 'instructional power' of the sign), the inference itself may appear more or less *necessary*. Perception itself, to illustrate, may be considered as a necessary inferential process. On the other hand, scientific processes such as abduction, are examples of weak inferential processes.[15]

Eco points out that this distinction between weak and necessary inferential schemes does not match exactly, the distinction between reasoning from effects to causes, as opposed to from causes to effects. One could also add that the first dichotomy (between weak and necessary inference) does not match that between induction and deduction. This latter dichotomy is traditionally used to oppose the methods of the mathematical sciences to those of the empirical sciences. Thus when one is concerned with the mathematization of empirical sciences, this is not an entirely adequate distinction to note.[16] This distinction between different aspects of inferential processes is also useful in breaking down, or decomposing complex traditional conceptual clusters. An example of this is provided by the formal-abstract-certainty-necessity-scientific connotations with which the expression 'mathematical reasoning' is laden. In traditional epistemological approaches to mathematical economics this connotation is often tacit or understood. However, this is unsatisfactory, first because the necessary character of mathematical reasonings can be that at one and the same time, it displays both rhetorical *and* scientific features. It is also unsatisfactory because the acceptance by the scientific community of economics as a science was not historically completely tied up with its being considered a mathematical science (cf. Chapter 3 , note 6).

In Eco's acceptation of the term, 'sign' does not refer exclusively, as it often does in linguistics and in the philosophy of language, to a 'static' sign, that is something that stands for something else. By 'sign', Eco refers to a *dynamic* interpretation process. A system of signs consists in a set of (dynamic) signs. Altogether it comprises both static signs, and rules of interpretation which are partly or essentially disclosed by the analysis.

Let us just indicate further possible avenues of enquiry of the use of the concept of sign in economic methodology. We shall only raise these questions in this thesis, as a way of indicating how the principal lines of argument can be further extended. Regrettably, answering them is beyond the scope of this present body of work. Eco distinguishes between semiotic codes on the one hand and scientific laws and hypothesis. He does so on the grounds that in the former system, unlike in the latter, inferences are *determined* by social context, whether they be weak or necessary. From then on, questions such as the following can be asked (and might be answered in a non-conventional way). Is economic knowledge better accounted for when it is conceived of as a semiotic code or as a scientific theory? Has its status as a semiotic system changed throughout the history of the discipline? Can mathematics or mathematization have any function in structuring these systems or in transforming one kind of system into another kind? etc.

If we return to more traditional methodological questions, semiotics might be fruitful as well, as the following examples aim to suggest. One deals with the assessment of mathematical economic models using statistical data, the other deals with the comparison between mathematics in physics and mathematics in economics.

In analysing cause-effect relationships, Eco considers that the sign from which originates the inference can bear two functions. It is 'prognostic' if what is inferred causally follows from the sign. However, if that which is inferred actually causally precedes the sign, then this sign is called 'diagnostic'. For example, in economics if we take the cause/effect relationship as being the linear temporal relation (or "historical time" to use Mahieu (1989)'s terminology) and signs as being the numbers of statistical tables, then the *same* signs can be both diagnostic economic signs and prognostic economic signs. Consider 'economic prediction' in the sense in which Friedman (1953a) in his famous Essays in Positive Economics, or more generally as used in the literature of 'time-series analysis' (e.g. as applied to a sequence of prices or of outputs over the business cycle). Nowadays such models are explicitly mathematical, in most cases. However, as Perrot (1992, pp.139-140)'s report on Eléonore Marie Desbois de Rochefort's work on demographic statistics (published between 1784 and 1788) shows, 'models' were once a form of reasoning using common-sense knowledge and basic logic. In this case, statistics are diagnostic signs because thanks to a 'model', one makes inferences about the state of an economy in the past. In national planning or simulation, using input-output models, for example, statistics are prognostic signs from which one makes inferences about the alternative (and possibly future) states of the economy. Here are some methodological issues that can be asked about these two uses of statistical data.

In the practice of economic model-building, are the *same* models used for diagnostic and prognostic analysis? If yes, do they differ however in the status they give to some symbols (e.g. variable as opposed to parameter, function as opposed to operation)or by the sets to which the variables belong?

Since a major debate in the methodology of economic literature deals with defining criteria for choosing between models, one can ask here whether the same criteria can be used for prognostic statistics as for diagnostic statistics. For example, one can ask whether formal generality criteria (e.g. capacity of a single formalism to 'give account of' several sets of statistical data) and confirmation criteria are more valuable for time-series (diagnostic) analysis as opposed to simulation (prognostic) analysis.[17] In the latter case, what is looked for seems to be not so much the generality of the model, as its ability to represent real and potential relationships between variables, constants and parameters (e.g. as in the simulation of alternative tax regimes in a computable general equilibrium model). Compared to the diagnostic usage of statistics, the important issue here may be not so much to have criteria for choosing between different models as to have, instead, *different* models, which display a range of configurations or scenarios that can be qualitatively analysed so as to get an idea of different *possible* cause/effect relationships.

The semiotic distinction between inferential schemes may be of help in providing an explanation of the fact that even though the ideas and the symbols used in economic dynamics are sometimes the same as those of and in mathematical physics, the meaning and the cognitive content of basic notions such a time, cause, effect, equilibrium, stability, statics, and so on, are very different from one discipline to the other. This point was discussed and emphasized at the "Première rencontre interdisciplinaire entre physiciens et économistes", a conference organised by the CNRS in Aussois, France 13-17 March 1995. This fact might be taken as an argument for supporting the view that economics is a specific semiotic system.

Eco uses the definition of 'cultural unit' phrased by David M. Schneider:

> In a culture, «a unit... is simply anything that is culturally defined and distinguished as an entity. It may be a person, place, thing, feeling, state of affairs, sense of foreboding, fantasy, allucination [sic], hope or idea. In american [sic] culture such units as uncle, town, blue (depressed), a mess, a hunch, the idea of progress, hope and art are cultural units» (Schneider, quoted from Eco, 1968, p.64).

Cultural units that do not vary from one language to another are called inter-cultural units. In the paragraph of Eco (1968) mentioned above, cultural units are linguistic units in the following sense. They always express themselves as denotations of a signifier. For example, what we have called the 'conceptual cluster' (cf. *supra*) associated with mathematics may be considered as describing the cultural unit associated with the term 'mathematics' in economic literature. The association of a particular term with a particular cultural unit does not necessarily last over time. We claim that in the 20th c the cultural unit associated with 'mathematics' and 'statistics' has changed. This, together with our having an 'external' historical approach, justifies our devoting several chapters of this thesis to describing some features of the historical context related to the texts upon which we comment (cf. chapter 3, chapter 4 , chapter 5, chapter 9).

We shall now define other semiotic concepts which we use exclusively in connection to written sign-systems.

Given a text, an 'expression' is one graphic mark or a set of graphic marks that occur in the text. In order to make clear to the reader that we consider a set of graphic marks to be nothing more than an expression (e.g.: 'the consumer x') we shall write it between two slashes (e.g.: /the consumer x/). Two visually different set of graphic marks (e.g.: 'consumer', 'consumers') are considered as one single expression (e.g. /consumer/). Different forms of analysis of one and the same text may identify different expressions. For example, in a grammatical analysis 'consumer' and 'consumers' may be considered as two different expressions. In all cases however, the identification of expressions in a text requires no more than the knowledge of vernacular languages. Generally speaking, an expression need not be meaningful. As far as we are concerned, the expressions we shall identify in the texts we analyse are English words or groups of words, notations and combinations of those, as well as figures.

The 'content of an expression' in a given text is what makes series of typographical marks meaningful in this text. The content of an expression is determined ideally by all the relationships that exist between expressions in a given text. For example, the series of marks 'the consumption set of consumer i is closed' is not meaningful if it is considered as '/the consumption set of consumer i/ is closed', because in the English language a set of alphabetical marks can not be closed. In this context, the content of /consumption set of consumer i/ is that which makes it closed. Contents of expressions (e.g.: the content of /consumer/) are written in square brackets (e.g.: [consumer]). The contents of expressions are defined in connection to one another. In the sentence just mentioned, [the consumption of consumer i] comprises the grammatical identification of the expression as a subject since the sentence is grammatically correct. The contents of the expressions of a given text are inter-connected. A relationship between the expression and the content of a set of marks is called a semiotic function. The set of an expression, a content of this expression and a semiotic function define a written sign.

Both the 'level of expression' and 'level of content' apply to a text. The 'level of expression' of a text consists in the expressions of a text together with the relationships they have with one another, considered as expressions. For example, the numerical series $(\mu_m)_N$ defined by μ_i = number of graphic marks of the ith expression of the text, belongs to the 'level of expression'.

A 'level of content' of a text consists of a set of *signs* whose contents are interrelated, together with this relationship. A text may have different levels of content if different relationships can be identified between the contents of its expressions. We claim that because the expressions we identify in the texts are words, groups of words, notations and figures, it is impossible to know the difference between a mathematical and a literary extract of a text by simply studying their level of expression, except in a weak quantitative sense. Mathematical texts use words and sentences just as literary texts use notations and figures. They might differ only with respect to the quantity of each kind of expression they contain, but we do not consider that this difference is significant. Consequently, the difference between mathematical and literary signs is a difference between their content and not between their expressions.

Herreman (1996b) identifies an 'isotopy' in a text with a 'level of content'. We shall use 'isotopy' in a wider sense so as to refer to relationships at the level of expression and at the level of content altogether. Arrivé's (1973, p.54) definition of 'isotopy' will be ours: "An isotopy consists of the redundancy of linguistic units related to the level of expression or to the level of content; these linguistic units may be tangible or not." (our translation)[18] A text that has more than one isotopy is called 'poly-isotopic'.

NOTES TO CHAPTER 2

1 "It may be that numbers do not rule the world, but at least they indicate how it is ruled". (Our translation).

2 Our views on the relationships between epistemology, history and methodology are expounded in Cheix(1996).

3 Nevertheless, there is one subfield of mathematics, namely abstract algebra, which is not widely used in economic theory. In particular, quotient sets of points or of functions are rarely used except maybe in connection with utility theory. They might be useful however since they provide a straightforward way to conduct a calculus on classes of "complex" objects. In addition, in some cases, they provide a way to consider the solutions of a system of equations as a structured space which is the result of an operator. However, Philippe Mongin, who is currently conducting research at the Delta (CNRS, Paris) and at the CORE (Université Catholique de Louvain), mentioned to us an article concerned with social choice by Chichilinsky *et al* (1979) that uses basic algebraic properties as a key argument in the proof of theorems they provide. Chichilinsky *et al* (1979) are using the decomposition of finitely generated abelian groups into a direct sum of a finite number of mathematically identifiable cyclical groups.

4 More generally we are interested in the cognitive position of numerical structures in the phenomenological division between the formal, the empirical and the physical. Aristotle in the Physics, book 4 (esp. from 219a10 onwards) analysed this position. So also does Beneze(1961), who studies this position in 'experimental sciences'. The neurosciences might also provide material for defining this place. Studies into the connection between the performance of mathematical calculus and neurophysiological functions involving language were indeed reported at the Third Annual Conference of the European Society for Philosophy and Psychology (Paris, 1-4 September 1994).
This is important for the methodology of this thesis insofar as the interpretation of mathematical texts is concerned. We *believe* that sometimes mathematical *entities* play a psychological function in reasonings. This function, which is similarly played by the use of regular *notations*, is that of focusing the attention of the reader. This is the case of [0, 1], in real vector space analysis and in probability theory, and it might be the case of '0' in maximisation problems. For example, in integration analysis, the point of the proofs of general theorems is often to come down to solving a problem involving [0, 1]; then the proof of the original result is obtained thanks to structural algebraic properties. To this respect, [0, 1] has "psychological" properties. The study of Cantor's Set can be considered in a way to set apart these properties from the properties of [0, 1] considered as a "purely" mathematical entity.

5 According to Philippe LeGall and Claude Ménard (book review of Morgan (1990), Economics and Philosophy, 8(1), April 1992, pp.286-290 'internal history' is defined by Georges Canguilhem in "Sur l'Objet de l'Histoire des Sciences" as well as by Henri Guerlac in "Some Historical Assumptions of the History of Science" both at the beginning of the 1960s. It has also been defined by Imre Lakatos.

6 These contributions are sometimes under-estimated. Franklin(1981) deplores this lack of acknowledgement of each discipline to the other. According to Popescu (1964) and Etner(1987, p115), the history of economic thought usually neglects the contribution of business economics.
Here are five examples which illustrate this point. One is the mathematician Giuseppe Peano (1858-1932). Peano is an important figure in the history of mathematics. He is most well known for his contribution to the axiomatization of arithmetic. Peano's interest in the foundational issues of mathematics is a feature of his work as a whole. By foundational issues it shall be understood at the same issues about mathematical logic, issues in mathematical logic and issues about their representation through symbols (Eco (1992) also consider that he is not a minor figure in the history of linguistics. As far as his contribution to economics at large is concerned, we have not noticed that it has been referred to in the literature of the history of economic thought (cf. Chapter 3). In addition, Kennedy's bibliography on Peano (Kennedy, 1980, pp.211-215) does not refer to any specialised study on this aspect of Peano's work. Our information on this subject comes from Kennedy's chronological list of Peano's publications (Kennedy, 1980, pp.195-209). From 1901 onwards until it seems, 1909, he published studies of insurance systems. For two reasons it is likely that he kept an interest in applied mathematics: the first is that he published studies on numerical approximation problems later on, and the other reason is that he published an article in a periodical specialising in financial mathematics. The titles of Peano's studies on economic subjects suggest that most of them contain mathematics. Peano is not an isolated example of a mathematician with an interest in insurance problems. According to Passemore (1966, p.129) Augustus De Morgan (1806-1871) had an interest in the extension of the theory of probability to problems of assurance.

The second example which we take from Zylberberg (1990) is Louis Bachelier. Bachelier is a historical example of the contribution of an economist to the development of a mathematical tool which revealed itself useful outside economics. According to Zylberberg (1990), who is our source of information on this matter, Louis Bachelier (1870-1946) submitted a thesis in 1900 for the degree of Doctorat Ès Sciences entitled Théorie de la Spéculation for the study in economics concerned with speculative phenomena on the stock market. In this thesis, the author analyses moves on markets in terms of stochastic processes of a martingale type. Even though the underlying subject matter concerns economics, the thesis itself is purely mathematical and aims at proving that markets behave according to the laws of probability. In addition, he uses a mathematical formalism adequate to the study of Brownian motion. According to Zylberberg (1990), the use of such tools resulted from his considering economic agents as particles. There are not many references to Bachelier in the historiography of economics. The table of contents, the indexes or the prefaces of Baumol *et al* (1968), Darnell (1991a, b, c, d, e, f), Theocharis (1993; 1961; 1983) do not mention Bachelier. As for Mirowski (1989), he refers to Bachelier in his bibliography but not in the index. It seems that not only physicists, but also economists with an interest in stochastic processes in the mid-20th c. do not know Bachelier's contribution either. For example, Samuelson (1947, p.268, p.317 note 13) suggests the use of stochastic analysis in economics. He refers to publications in physics and in astronomy as providing the bibliographical origin of the study of stochastic processes. In addition the economic example he treats is set in parallel with an example in physics.

We are indebted to Bernard Bru for the last three examples. The first is the mathematical analysis of the behaviour of a particular type of function. These functions have great differences in real image-values for comparatively small variations in the variable. The analysis of these functions originally developed in insurance studies in connection with generalised bankruptcy problems. It dates back from the beginning of the 20th c. it seems. Then it also became a purely mathematical subject.

The second example concerns the analysis of time series. It seems that the mathematical treatment of randomness into continuous processes was inspired by studies of economic time series. Today it composes the mathematical theory of processes.

The third and last example is the little known Charles-François Bicquilley (1738-1814) and his writings on mathematical economics. Apparently, none of our historical sources (cf. chapter 3) mentions this author. Between 1784 and 1787, Bicquilley wrote a dissertation for the 1787 prize set by the French Académie des Sciences on "La Théorie des Assurances Maritimes" (Theory of Insurance in the Maritime Business sector). In 1799, Bicquilley also hand in to the Académie des Sciences a manuscript entitled Principes Elémentaires du Commerce. This second dissertation was later revised and published in 1804 under the title Théorie Elémentaire du Commerce, which Pierre Crépel (1995) has recently edited with thorough comments. Bicquilley's Théorie is noteworthy in the history of the use of mathematics in economic science not only because of its axiomatic (cf. chapter 7, section) and general nature but also because the author makes use of probability calculus. These features are noteworthy first because the 1799 manuscript precedes a similar treaties, namely Nicolas-François Canard's early version of Principes d'Economie Politique (1801) entitled Essai sur la Circulation de l'Impôt (1801), and also because in all probability the two authors did not know each other's work in 1799. In addition, according to Crépel (1995), previous writings in mathematical economics such as Paolo Frisi's or Condorcet's lack the generality of Bicquilley's treatise. Finally, whereas Bicquilley draws no epistemological difference between calculi performed on probability numbers and calculi performed on other numbers, later authors either do make such an epistemological difference, or they simply do not use probability calculus. Contrary to Canard, Bicquilley was neither a mathematician, nor an academic. He was first a royal body-guard and he later worked as an administrator in Toul and Metz. Consequently he knew both the noble milieu and also the business milieu. In addition, thanks to few of his acquaintances, he got into contact with the academic milieu. Even though he was awarded prizes for his economic writings, it seems that they have long been ignored by historians of economic thought.

7 We are grateful to Professor Reid for drawing our attention on Catephores, George; Morishima, Michio; 1978. <u>Value, Exploitation and Growth: Marx in the Light of Modern Economic Theory</u>. The authors show the conceptual common ground shared by Marxian and Neo-classical theory, which is classical economic analysis. Methodological similarities between these two schools of thought are also mentioned later (cf. Chapter 7).

8 This view is supported by Van Parijs (1990, p.29 esp. note 4), Hahn *et al* (1979), Hausman (1992) and Backhouse (personnal communication).

9 What leads us to believe that semioticians as a whole share this view is Derrida's idea of 'inscription', which Bruno Latour and Steve Woolgar have used to conduct their anthropological study of a biological research unit (cf. Latour *et al*, 1988, note 2 p.35).

10 Concretely, an application of this principle with regard to economics and the assessment of game theory in particular, is that we agree with authors such as Nemo (1995). His view is that there is something to learn about the game theoretical analysis of economic interactions and about these interactions themselves from considering sociological and linguistic studies of individual interactions. In doing so, one can understand the limits and the strength of game theory and also explore alternative perspectives on economic interactions.

11 A description of this debate in the methodology of economics from the 1980s onwards is provided in Backhouse (1991b) as well as in Pierre Jacob (1988) and P. Salmon (1986).

12 Here are examples giving evidence that economics can be considered as a kind of logical system or, to use the epistemological terminology, a hermenentics. In <u>Cours de Linguistique Générale</u>, the linguist Ferdinand de Saussure (1857-1913) compares the linguistic analysis of the meaning of words with the economic analysis of the value of goods. In drawing a

difference between the signification of a word and its value he implicitly parallels Adam Smith's distinction in the Wealth of Nations between the "utility" of a good also termed its "value in usage" and its "value in exchange". Gary Stanley Becker (1930-) applies maximisation techniques to social behaviour in general, such as criminal behaviour and political behaviour of pressure groups (cf. Sandmo, 1993). Similarly, in an article stuffed with puns, Boulier *et al* (1991) compare the explanatory power of human capital theory with that of the economic theory of defense to give account of schooling behaviour of fishes. Another reason for considering economics as a specific semiotic system was suggested at the first interdisciplinary conference of economists and physicists (cf. chapter 2, section 3@2).

13 The term 'symbol' is used in this thesis, in a vernacular, non-theoretical meaning.

14 We shall not use the basic semiotic concepts of the 'form' and the 'substance' of the expression, and of the content, because they are not relevant to our analysis of texts.

15 According to Eco (1984), abduction as well as deduction are patterns of reasoning that consist in applying a general rule to a particular case. In deductive reasoning however, both the rule and the applicability of the rule to the particular case are taken for granted, and the result is a product of the reasoning. In abduction on the other hand, the result is taken for granted and both the rule and the case to which it is applied are based on the reasoning.

16 The distinction between weak and necessary inference may be more useful than that between induction and deduction. For example, it may provide a more adequate account of the history and the nature of econometrics. Thus econometrics can be seen, retrospectively, as a successful attempt to turn the initially weak inferential scheme of (statistical) "induction" into a stronger form of (mathematical) necessary inference.

17 For example, it can be considered that the Quantitative Theory of Money achieves generality in accounting for price levels. In this connection Friedman (1968) points out that several historical eras of the history of money such as the discoveries of mines of silver or gold in the New World in the 16th c.; the hyperinflation in Central Europe after the First World War, and also price variations in the United States after the Civil War (1861-1865), can be accounted for with mathematical models inspired by the Theory.

18 For example, lexical categories are non-tangible linguistic units; and a word or a letter are tangible linguistic units.

SUMMARY AND CONCLUSION OF PART ONE

It is maintained that this thesis is concerned with the methodology of economics and that it also has attributes that are included in the epistemological studies of mathematics and of the social sciences (cf. Cheix, 1996). By methodology, we mean the study of the methods used in a *particular* science and the problems occurring in a science. By epistemology, we mean the philosophical study of *scientific* knowledge, equivalently named the philosophy of science. The kind of epistemology this thesis claims to be relevant to is what Piaget (1976) calls 'intra-scientific' epistemology. It aims neither at being normative, nor at being unifying. It is based on the study of problems occurring in a particular science and it treats general epistemological questions only in connection with these problems. In this thesis however, we shall focus on specific problems, and only mention general epistemological questions. It is also maintained that neither methodological studies nor epistemological studies can avoid considering the history of science. This is a way to cast light upon what, in scientific practice, is based on cultural habits and what is not.

The subject matter of the thesis is mathematical economics. The first topic it analyses is mathematical economics in the theory of the Neo-classical school of economic thought. This school has its roots in the late 19th c and prevailed in the 20th c, both within academic economics and, generally speaking, within Western ideology. The second topic analysed is mathematical economics, considered within in the 20th c. scientific practice of model-building. The originality of the thesis does not lie in its topics, but in the way they are approached. This approach is a critique of the use of mathematical formalism in economic theory. It is an epistemological-methodological critique that uses a semiotic framework. It is neither a teleological critique of economic science nor a critique that procedes from a Political Economy perspective. The first part of the thesis is concerned with describing both this approach and the methodology of the thesis.

A standard approach to the use of mathematical formalism in science is that of an important movement in 20thC. philosophy of science, namely logical positivism. This movement has many facets and we can only claim to convey its views in connection with our subject-matter. From a positivist point of view, the use of logico-mathematical formalism, as opposed to vernacular language, is one characteristic of a true science. In so far as a theory is *formal*, its *logical cogency* can be assessed. In so far as it is grounded on *arithmetics* and it formulates equations and inequalities, its statements are quantified. They can be *assessed empirically*, with the aid of statistics in particular. Logico-mathematical formalism thus guarantees the objectivity of both the reasoning and its results. This approach tends to focus on the role of mathematics as a conceptual and logical tool rather than as an empirical tool, as in statistics.

In this thesis, another approach to the use of mathematics in science and in economics in particular is developed. We follow Gilles Gaston Granger (cf. Bibliography) and Giorgio Israel (1996) and understand the subject-matter, namely mathematical economics, as the *mathematization* process of economic science. By mathematization, we mean the historical and epistemological *relationships* between the "mathematical form" and the "economic content" (cf. chapter 2, Section 1).

This approach leads us to adopt an 'external' perspective on the history of mathematical economics for the reasons that follow. Firstly, the mathematical formalism used in economics has sometimes developed initially in other fields (cf. chapter 2, note 6) such as physics (cf. Mirowski, 1989), commercial life (see Bicquilley, 1804) and political science (cf. Armatte, 1991; Perrot, 1992). By 'external' perspective, we mean (cf. chapter 2, section 1) that we replace the history of the use of mathematical formalism in both the history of science in general, and in the history of mathematics. Authors who have adopted a similar approach to the history of economic thought are: Armatte (1991, 1995), Ingrao *et al*(1987), Israel (1996), Mirowski (1989, 1990, 1991), Mirowski *et al*(1994), Perrot (1992), Punzo(1991), and Zerner (1993). Authors who adopt an 'internal' approach are: Morgan (1990), Theocharis (1961, 1983, 1993), Stigler (1954, 1964), and Zylberberg (1990). The survey of the academic corpus available on the history of mathematical economics shows first that there are not many sources on the subject (e.g. Theocharis, 1961, 1983, 1993; Baumol *et al*, 1968; Darnell, 1991a, b, c, d, e, f; cf. chapter 1) and that they proceed from 'internal' historical approaches. Consequently, any general historical or epistemological statement on the development of mathematical economics must be handled with caution. Secondly, the survey of this corpus also shows that contemporary historians and epistemologists challenge the historiography of economic thought (cf. chapter 1, section 1).

From an epistemological point of view, approaching mathematical economics in terms of mathematisation does not lead us to contrast vernacular-unscientific language and mathematical-scientific language as sharply as in a positivist perspective. In economic practice, vernacular language is indeed used to relate mathematical symbols to economic ideas. This also applies to mathematics, since mathematical texts also contain vernacular language. Taking this position is also a way to avoid the layman's paradoxical position, that on the one hand, economics is not a "true" science, but on the other hand, it is scientific because it uses mathematics intensively. Our keeping a distance from positivist approaches to the subject matter does not contradict general concerns emerging from the literature in the epistemology and the methodology of economics (cf. Cheix, 1996, section 2 and section 3). One concern is that economic science and standard epistemological categories are not adequate to one another. One explanation is that standard epistemology, logical positivism included, has been strongly influenced both historically and sociologically, by the study of problems typical to physics. Our position on the epistemological status of mathematical formalism explains our choice of semiotics rather than formal logic for the study of the structure of two Neo-classical texts in mathematical economics upon which we focus, namely Jevons' Theory of Political Economy and Debreu's Theory of Value (cf. chapter 2). Because the semiotic approach to "meaning" strongly differs from that of the positivists, the semiotic perspective is explained in some details (cf. chapter 2, section 2 and Cheix, 1996, section 4).

To the extent that this thesis is concerned with the 'external' history of mathematical economics, it places the two topics it deals with (the use of mathematical formalism both within the Neo-classical tradition and also in connection with model-building), in the history of science (chapter 3; chapter 6, section

1; chapter 9, section 1) and in the history of mathematics (chapter 4; chapter 6, section 3@1). In so far as it is concerned with the methodological/epistemological analysis of the use of mathematics in economics, first it defines the mathematics which are and have been used in economics (chapter 5, section 3@1; chapter 6, section 2@1; chapter 6, section 3@2; chapter 7). Then, it explains both the biographical origins of this use (chapter 5, section 1; chapter 5, section 2; chapter 6, section 3@1), and the historical origin of model-building (chapter 8). Finally, it attempts to 'describe' the role of mathematical formalism in economics. A general description is provided (chapter 9, section 2). In addition, specific analysis, using semiotics, attempts to clarify the idea that mathematical formalism in economic theory may have either several "meanings" or none (chapter 5, section 3@2; chapter 6, section 3@3; chapter 6, section 2@3).

The reader may find that the foundational part of the thesis is extensive. This is justified because we are taking an unorthodox philosophical view on mathematical economics and also because comprehensive historical sources on the subject are lacking. The reader might also find that such philosophical authors such as Karl Popper, Thomas Kuhn, Imre Lakatos, Larry Laudan etc. are not referred to as often as the reader might expect since it is claimed that this thesis is relevant to epistemology. This lack is not a sign of ignorance. It results from the choice to refer to philosophers (e.g. Granger, 1955; Hausman, 1992; Rosenberg, 1983; Morton, 1990, 1993; Van Parijs, 1990) with specific interest in economics rather than in physics or in general epistemology, so as to avoid difficulties to which we have already alluded.

PART TWO

MATHEMATICS AND THE NEO-CLASSICAL TRADITION

CHAPTER 3
19TH C. SCIENTIFIC HISTORICAL CONTEXT

"Il en résulte qu'à titre même d'institutions sociales et de produits de la civilisation d'un peuple, les religions, les systèmes philosophiques, les sciences se succéderont dans l'ordre indiqué, partout où l'accumulation des incidents historiques, les révolutions et les importations étrangères ne troubleront pas cet ordre régulier. Mais l'énoncé même des conditions montre que l'exception peut être aussi fréquente ou plus fréquente que la règle; et dans les cas qui nous intéressent le plus, c'est bien l'exception qui prévaut."[1]

Antoine Augustin Cournot, Considérations sur la Marche des Idées et des Evènements dans les Temps Modernes, 1872.

INTRODUCTION

This chapter is concerned with describing external and internal features of the historical background of the development of the use of mathematical formalism in economic theory. It focuses on the state of science in the 19thC. because historians of economic thought agree that this century is an important period in the history of both economics and mathematical economics (cf. chapter 3).

In the first section, we shall first sketch the state of Social Sciences in Europe in the 19th c., considering it as the union of two trends: one towards the separation of the social sciences; and the other towards their institutionalisation. Then we shall consider whether this picture applies to economics in particular. The purpose of the first sub-section is simply to indicate the nature of a historical trend; in this sense, it is illustrative. The second sub-section has a more descriptive purpose. Consequently, the conclusions of this section should not be generalised out of the context of this thesis. In this thesis concerned with external history (cf. Chapter 2, section 1@1 and section 2), this section aims at suggesting links between the history of economic thought and the history of the social sciences.

The second section focuses on features of 19thC. mathematical economics. Generally speaking, authorities consider that the 1870s 'marginalist revolution' is an important event in the history of mathematical economics. However, it was pointed out that there was a lack of convergence in the literature in qualifying this statement. This is why, before giving some indication on the state of mathematical economics at the end of the 19thC., we shall attempt to define what kind of historical event the marginalist revolution was.

SECTION 1: SOCIAL SCIENCES: SEPARATION AND INSTITUTIONALISATION?

SECTION 1@1: SOCIAL SCIENCES, OUTSIDE OF ECONOMICS[2]

The 19th century is an important epoch in the history of the Social Sciences. Even by the end of the 19th c., their various branches did not resemble their current form. In showing the limitations of Jevons' epistemology of the social sciences, Mays (1962, pp.232-233) is therefore right to point out that:

> "Jevons then suffered from the drawback that he wrote before many of the social sciences, as we know them today had adequately developed"

However, as the inquiry that follows will suggest, supporting the point made by Louis (1988), the 19th c. is the time when the seeds of contemporary specialisation in the Social Sciences were sown. This view is supported by Porter's (1986) view that new scientific fields were being delineated in the 19th c. However, our view differs from his because we believe that the idea of a 'new science' is an empty concept (cf. Chapter 1, section 4, first philosophical landmark).
We shall now define what we mean by specialisation.

The 19th c. is the time when manifestos defining the methods and the objects of what we now call the Social Sciences were published. Here are some examples. As far as Psychology is concerned, the first part of the 19th c. is important. Two views were competing with one another. One was that of Auguste Comte (1798-1857) who believed that there was no such science as Psychology, whether it be Psychologia Rationalis or Psychologia Empirica as defined by Wolff in the first half of the 18th c. The proponent of the other view was John Stuart Mill. In 1843, he published A System of Logic and advocated that Psychology was fully scientific and separate from other sciences. Later in 1895, Sigmund Freud published with Joseph Breuer Studien über Hystereie in which the psychoanalytical methodology was laid down. As far as 'sociology' is concerned, Auguste Comte is in all probability the first French author to create the word in the 1830s in order to replace the term 'social physics' which was previously in use. After Comte, Emile Durkheim is worth mentioning. In 1895 he published the Règles de la Méthode Sociologique. In this book, he defines the subject-matter and the methods specific to Sociology in a way that is still controversial. Durkheim (1858-1917) considered that sociological explanations must be homogenous in the sense that a social phenomenon must be explained by another social phenomenon as opposed, say, to a natural phenomenon. According to Durkheim, social phenomena ought to be considered as 'objects', by which he meant that in Sociology as well as in the Natural Sciences, scientific phenomena are not given by Nature but they are the result of a minute analysis. Further examples could also be given from Linguistics and Anthropology, but these are omitted for the sake of conciseness.

Not only were particular Social Sciences promoted by literate individuals in the 19th c., but they became institutionalised. Thus specific learned societies were born, and specialised journals were launched which contributed to define their scope and purpose. In addition, Social Sciences began to be taught as academic subjects proper, thereby replacing 'moral sciences'. For example the German Society for Sociology came into being in 1910; L'Année Psychologique, was born in 1895 and L'Année Sociologique was first issued in 1896. Similarly in 1903, the University of Munich in Germany created a chair of Sociology.

The separation of the Social Sciences and their institutionalisation really make the key distinguishing differences between the 19th c. and the previous centuries. Earlier, one can still identify, post hoc, inquiries which were not identified as belonging to a particular social science, as nevertheless having a disciplinary application. For example, La Bruyère's Caractères (first published with no authorship in 1688) and Descartes' Les passions de l'âme (1649) contain elements of psychological investigation. So also with anthropology: Jesuit missionaries' reports and diaries have today an ethnological value. In view of the

Scottish context and his Saint Andrews affiliation, Adam Ferguson's (1723-1816) Essay on the History of Civil Society, published in 1767 is worth mentioning since it is considered as a precursor of contemporary sociological treatises.[3] It is however the union of the trend towards the separation of different subjects with the birth of the social status of the 'scientist' that characterises Social Sciences in the 19th c. This term occurs in French and English at that time. More precisely, Wightman (1980) writes that the term 'scientist' did not exist in the English language before 1839. This union may be called specialisation.

A first symptom that the separation of the Social Sciences had become substantial by the 19th c. is that in 1690, John Neville Keynes was in a position to make a comparison between economics and other Social Sciences in The Scope and Method of Political Economy. At the beginning of the 20th c. Max Weber (1864-1920) was in a position to write a methodological critique of the Social Sciences. In the last two decades of the 19th c., he wrote a series of books and articles concerned with the philosophy and the methodology of the Social Sciences.[4] The methodological issues dealt with in these publications are still being debated today. So also the Problemi Della Scienza of Frederigo Enriques (1871-1946) first published in 1906. Enriques expounded a critique of Positivism and his analysis is echoed today in the philosophy of the Social Sciences. Following a line of argument which has since been explored systematically by the logical positivists (cf. chapter 5, section 2@2) Enriques (1910) describes the role of linguistic symbols and of graphic symbols in the conduct of reasoning. He considers that they are essential tools for expressing general statements. In addition, he brings into light the limitations both to the use of symbols in science and to the concept of homo-economicus in economics. The first criticism anticipates criticisms such as Caldwell's (1982), that if a 'schematization' - to use Enriques' (1910) terminology, or equally 'mathematical formalism' - to use contemporary methodological terms, is too abstract or too general, it runs the risk to lack cognitive content. The second criticism anticipates criticism's such as Van Parijs (1990)'s criticism of methodological individualism. It is that the concept of rational homo-economicus, which has long been used in economics as the representative of the *economic* behaviour of *individuals* is a misleading representative because it does not take into account collective *economic* behaviours that influence individual economic behaviours.

The trend towards the unification of the sciences, and not only the Social Sciences, that began at the end of the 19th c. can be considered *a contario* as a second symptom of the separation of the sciences, Social Sciences included, that occurred in the 19th c. The periodical The Monist belongs to this trend. A survey of all issues of this journal from 1890, when it was first issued, to 1896, reveals that its contributors were reacting against scientific specialisation. It is clear from this survey that the specialisation does not concern only the disciplines in relation to one another. It was also an intra-disciplinary phenomenon. Two additional later elements characteristic of this trend are the Vienna Circle's advocacy of a unified scientific language (cf. Neurath, 1931 and 1935) as well as the commissioning and publishing of the International Encyclopaedia of Unified Science. The latter appeared in parts from 1938 onwards. Historical trends towards the unification of the sciences in the inter-war period can indeed be interpreted as a reaction against the institutional and methodological separation process of the sciences in the 19th c. and the beginning of the 20th c. As our selection of authors and periodical suggests, the two features of the Social Sciences we described are similar in different European countries. The holding of an international congress for the teaching of the Social Sciences in Paris in 1900 confirms this suggestion (see Zylberberg, 1990).

SECTION 1@2: ECONOMICS

In this sub-section, we are concerned with presenting evidence for the institutionalisation of Economics and of its separation from other Social Sciences in the 19th c. Unless otherwise stated, the main sources for this evidence are Armatte (1991, pp.8; 31-32), Etner(1987), Le Van-Lemesle(1991), Zouboulakis (1993), Zylberberg (1990) as well as general references.

Compared to England and France, Germany seems to have been an early innovator as far as the creation of University chairs of Political Economy is concerned. Some were created by Frederic Wilhelm I (1688-1740) as early as 1727. In 1870, the chair of Political Economy of the University of Lausanne was created for the precocious Léon Walras, who was unable to obtain a post in France.

In 1877 the French government issued a decree, making Political Economy a compulsory subject in the syllabus of legal facilities within the universities. This dating explains both why Zylberberg (1990) considers that until the 1860s there were no professional economists in France, and why Breton(1991, p402) holds the view that the profession was born in 1877. However, the institutionalisation of economics (that is, to use Le Van-Lemesle's(1991) definition, its appearance in education, proper learned societies, journals and textbooks) developed during the entire 19thc. Before then, Economics or Political Economy was essentially taught in independent institutes and at the Grandes Ecoles. In 1860, there were indeed three chairs of Economics in these institutions: one at the Ecole des Ponts et Chaussées, which was created either in 1846 (cf. Le Van-Lemesle, 1991) or in 1847 (cf. Etner, 1987, p126); one at the Centre National des Arts et Métiers, which was created in 1820 and the third was created for J.B.Say (1767-1832) at the Collège de France in 1831. Later, in 1864, a chair of economics was ceated at the Faculté de Droit de Paris. Similarly, when the Ecole Libre des Sciences Politiques was established in 1871 in Paris, a chair of economics and a chair of statistics were set up. It was considered that the courses delivered in statistics were the practical complement of courses in economic theory. Still later, in 1878, a department in Economics and Politics was created at the Ecole des Hautes Etudes and in 1885, a chair of Industrial Economics was created at the Ecole des Mines, where Walras first studied as an engineer.

England stands between Germany and France so far as the dates of creation of chairs of Political Economy is concerned. Nassau William Senior (1790-1864) was the first Professor of Economics in England. He held the Drummond chair at Oxford in 1825. A chair of Economics was created for Richard Whately (1787-1863) at Trinity College, Dublin, in 1832. So far as the academic separation of political economy in England was concerned, it consisted, on the one hand, in the separation of Moral Sciences from Divinity. At Cambridge, this separation emerged out of the 1871-1882 academic reforms. On the other hand, it consisted in the separation of Political Economy from the Moral Sciences. At University College, Bristol, it seems that it occurred under the Principalship of Marshall (1842-1924) between 1877 and 1881.[5] At Cambridge, it occurred, due amongst others, to John Neville Keynes in 1906.

Other evidence supporting the argument for the institutionalisation of economics is the birth of learned societies as well as that of specific periodicals. The Société des Economistes was created in france in 1842 and was named Société d'Economie Politique from 1847 onwards. In 1841, the Journal des Economistes was created in France. The Quarterly Journal of Economics and the Revue d' Economie Politique were created respectively in Harvard in 1886 and in Paris in 1887. The first issue of the Economic Journal was published in 1891. As far as the Giornale degli Economisti is concerned the first issue was published in Padua in 1875.

At first sight, Economics in the 19th c. fits into the above picture of the institutionalisation of Social Sciences. Institutionalisation is part of what is sometimes referred to in the economic literature as the 'professionalisation' of economics. According to Zouboulakis (1993), it is both a European and North-American phenomenon, and it accelerated in the 1890s.

It is a much more delicate matter to identify the separation of Economics from the other Social Sciences in the 19th c. We shall now present four elements that provide prima facie evidence for such a separation. The first is the professionalisation of Economics described previously. The second is the fact that Economics was considered a science from the 19th c. onwards [6]. In addition, it is often considered in the literature that in the 19th c. Economics became separated off from politics. This resulted in the study of the 'social organism' being sub-divided into specific studies of its particular components. This division gave to each Social Science a specific and central object of analysis. The fourth is the definition of the proper object of economic science. According to Zouboulakis (1993) the Ricardian tradition contributed to a large extent to the last three points. For example, Ricardo defined the two most important phenomena to be studied by economic science as being produced goods and income. Zouboulakis (1993, p.49) points out that this tradition is the first in the history of economic thought to insist on setting limits to economic inquiry and on defining its relationships with the other Social Sciences.

It might be argued, however, that the last three elements are not convincing reasons for supporting the view that in the 19th c. economics has separated from the other Social Sciences, and has become an independent body of knowledge.

One could argue first, that Economics and, more generally, theories in the Social Sciences are not separable from politics. This is the point of view held by Breton *et al*(1991), since their historical methodology consists of considering the evolution of economic science in France in connection with that of political life. The first reason for advocating this view is that institutions such as universities or learned societies are connected to politics. The second reason concerns the ambivalent connection between statistics and the Social Sciences in general. On the one hand the Social Sciences, from the 19th c. onwards relied on statistics as their 'empirical' and 'objective' foundations. On the other hand the evolution both of statistical data and statistical analysis was partly, but strongly, connected to politics, from at least 1800.[6] Secondly, one could argue that the object of economic science was not separate from that of the other Social Sciences. A first line of advocacy of this view would minimise the importance of the Ricardian tradition for the development of economic thought in the 19th c., and insist, rather, that Comte's

Positivism had a greater bearing on economic thought.[7] A second line of advocacy for this view would be to point out that few economists, even in the Ricardian tradition, denied that, to a certain extent, economic phenomena are dependent on non-economic data. What is meant by the isolation of economic phenomena from the subject-matter of other Social Sciences depends on what is meant by the last two phrases in italics.

On the whole, there is evidence for supporting the view that Economics became institutionalised in Europe in the 19th c. and, in all likelihood, in the United States as well at this time, just as the Social Sciences did. However, arguments in favour of the view that Economics separated from other Social Sciences are not yet conclusive, at least so far as they can be assessed from the literature we have surveyed.

SECTION 2: MATHEMATICAL ECONOMICS

SECTION 2@1: REMARKS ON THE SO-CALLED MARGINALIST REVOLUTION

Zouboulakis (1993, 'L'Emprise de l'Ecole Historique sur la Théorie Economique', pp.150-159) shares our view that the end of the 19th c. is an important, though ambiguous, era in the history of economic theory. He reports the views of historians of economic thought on this period from the 1950s to the 1980s. The subject-matter of the literature he refers to is the appraisal of the contribution of differential calculus to the progress of economic theory. The lack of convergence in this literature is striking. The historical events themselves are: the publication of Jevons' (1835-1882), Theory of Political Economy in 1871; the publication of Carl Menger's (1840-1921) Grundsätze der Volkswirtschaftslehre in 1871; and the publication of Walras' (1834-1910) Eléments d'Economie Politique Pure from 1874 to 1877 out of dissertations published from 1873 onwards. It seems that these events are not considered to be historical data out of which the evolution of economic theory may be reconstructed. Instead, these events play the role of either illustrations or counter-examples to general views on the evolution of economic thought. These views are sometimes a consequence of deploying a philosophy of science which is not fully adequate to the history of Mathematical Economics (cf. chapter 4, section 2). Authors, who want to emphasize the use of mathematical symbolism in theorising, tend to identify this event with the works of Jevons and Walras mentioned above, to the exclusion of the work of Menger. Authors, who consider that it is the 'marginal reasoning' about value itself that is mathematical, include Menger as well. Specifying the marginalist revolution is even more difficult if one takes into account Etner's(1987) research (cf. chapter 1) and also the influence of railways studies on Jevons' work (cf. chapter 5, section 1@1). Etner(1987) shows that in the second part of the 19thc, French engineer-economists who were using mathematics, focused on the importance of marginal values (e.g. marginal costs) for assessing the economic utility of engineering work (e.g. bridges, roads, railways). According to Etner(1987), they were not influenced by the theoretical studies of the marginalists. Hence one could go so far as to argue that the marginalist principles have practical origins and not merely theoretical ones. The lack of convergence in identifying unique features of the marginalist revolution as an historical event is not a characteristic which is specific to the contemporary historiography of economic thought. It is also mentioned in the work of J.N. Keynes (1917). This shows that it is also a feature of the historiography of economics at the beginning of the 20th c. In accordance with chapter 2 we shall focus on mathematical symbolism and identify the marginalist revolution with the works of Jevons and Walras mentioned above. This focus on symbolism and writing in science has neither an ontological nor theological meaning, as it does in Jevons' view on the matter (cf. chapter 5). Our idea is rather that the mode of writing plays a special role in the diachronic and synchronic communication of knowledge, and that this role may be a defining feature of scientific knowledge.[8]

Zouboulakis (1993) also explores how, and when the marginalist revolution was perceived in both Europe and the United States. It is worth noting that the phrase 'marginalist revolution' does not occur in J.N. Keynes (1917)'s table of contents, nor in his index, nor in the chapter referenced. However, he compares, in the core of the text, the Jevons' and Walras' books just mentioned; and he also refers to Walras in this connection in notes. Generally speaking, the opinion that prevailed in the 1870s, was that Political Economy was going through a crisis. In the first place, the novelty of Jevons' theory was either ignored or else put on an equal footing with authors who are no longer important today. Zouboulakis' view, that the marginalist revolution was neglected, is confirmed by Jevons' own words (cf chapter 5). Alternatively, Jevons' use of mathematics was condemned simply on principle. The acceptance of Jevons' theory was both slow and late. It did not occur before the 1890s, and Zouboulakis reckons that Marshall contributed to it to a great extent.[9] According to Zylberberg (1990) Walras' contribution to marginalism was met with a similar fate and it was neglected in France; Zouboulakis (1993) points out that generally speaking, Walras was similarly ignored in the United Kingdom.[10] For his part, Punzo(1991) opposes the view that the marginalist and neo-classical movement was a revolution in the history of economic thought. Instead, he considers that the impact of the school of mathematical formalism on the Viennese circle of mathematical economists is what the historiography should consider as revolutionary.

Given this historiographical context and the subject-matter of the thesis, we shall now report only the admitted contribution of Jevons to the marginalist revolution. It consists in the combined use of differential calculus and of marginal values in the theoretical explanation of economic actions. It is considered that an economic action is virtually composed of a succession of sub-actions, and that the value or the utility of the original action essentially depends on the value or the utility of the last sub-action (marginal value). The values of economic actions are considered as differentiable mathematical functions, and the values of sub-actions are the corresponding values of the differentials.

From a broader perspective than Zouboulakis' study, the assessment of the mathematics used by the marginalists ought rather to be placed in the context of quarrels about the methods of Political Economy. In France, according to Breton(1991), these quarrels lasted fron 1800 to 1914, and were concerned with the relationships between economic science, mathematics, history and statistics. This context explains Zylberberg (1990)'s remark that even mathematical economists such as the engineer Emile Cheysson, put forward a philosophical argument against the mathematical deductive and numerical method in economics, namely that it conflicted with human freedom.[11] The ethical side of the argument was that such a method amounts to considering humans as objects. The epistemological side of the argument was that human actions do not follow laws that can be accounted for with numbers or within a deterministic scientific framework, and also that they are not directly measurable. One idea involved was that social phenomena are not stable enough over time to be investigated mathematically. Such philosophical questions about mathematical economics have been repeatedly addressed throughout its history. For example, as late as the 1960s, the same questions are raised in Lipsey (1963)'s economics textbook. These questions have been addressed either because they were conceived to be epistemological obstacles to this kind of inquiry, or for mere rhetorical reasons. Some authors in this tradition are opposed to Mathematics because they were opposed to the use of a purely deductive method in economic theory. Others, such as Mill, were opposed to Mathematics first because they equated mathematical formalism with quantification, and secondly because they believed the changing nature of economic phenomena made such quantification impossible. In contrast with the 'marginalist revolution' it seems that the 'quarrel on methods' is not just an element in contemporary historiography but is present in 19th c. historiography as well. Zylberberg (1990) mentions never-ending debates on the definition of the right method for political economy in that period. The existence of these debates is confirmed by J.N. Keynes (1917) who mentions them and reviews the arguments involved. In these debates, the historical-inductive method was opposed to the deductive method. It was also debated whether the proper scope of economic inquiry was to deal with individual phenomena or the general feature of these phenomena. Finally the view that economics was a descriptive science, was opposed to the view that economics ought to find laws and patterns.

The use of mathematics by the marginalists was assessed along these lines rather than by direct analysis of the formalisms used in the theory. It was criticised very differently, from one author to the other, depending both on where they stood in the general methodological debate and on what they identified mathematics to be. When authors referred to statistical data, the use of Mathematics was an example of the study of an individual phenomenon. When they referred to statistical theory, it was an example of historical deduction or of generalisation. Alternatively, it was identified with the deductive method. Also under discussion was whether it was necessary to state the laws of Economics in mathematical terms in order to be able to check their concordance with the facts. In addition, it was discussed whether Mathematics as in statistical data could heuristically suggest such laws.

SECTION 2@2: TRADITIONS IN MATHEMATICAL ECONOMICS

In this sub-section, we shall be concerned with putting the marginalist revolution into the context of 19th c. Mathematical Economics as this context presents itself from a 20th c. standpoint. According to Zylberberg (1990), the first French thesis in Mathematical Economics was attended by Albert Aupetit (1876-1943) at the Faculté de Droit de Paris in 1901 and published in 1901 under the title: Essai sur la Théorie Générale de la Monnaie. It deals with the study of monetary fluctuations. In his thesis, Aupetit considers that there are two kinds of mathematical analysis in Economics. One is rational Economics and the other is experimental Economics which is based on the study of statistical data. In this case, Mathematical Economics institutionalises as a synthesis of the different traditions in Mathematical Economics which we shall now describe.

One tradition in Mathematical Economics is usually referred to as economic 'theory'. In this connection, historians of Economic thought refer chiefly to neoclassical economists such as Jevons, Walras, Marshall; or else they refer to Cournot, Fisher amongst others (cf. chapter 1, section 2). Other traditions include those of the French engineers (cf. Etner, 1987,;Zylberberg 1990; Theocharis, 1993, pp.20-103) that of specialists in actuarial science as well as the theory of finance (cf. chapter 2 note 3). In addition, there is the tradition of statisticians who deal with economic subjects, which culminate with Arthur Bowley (1869-1957).

Those who have aimed to embed the history of mathematical economics into a scientific context larger than that of the internal history of economic thought have had a tendency to explore no further than the history of Physics rather than other traditions (cf. chapter 4, section 2). In this sub-section, we shall focus on statistics instead, and on the practical origins of Mathematical Economic 'theory' in the sense stated above, in order to shed new light on the wider scientific context. Another reason for this focus on statistics is that we are interested in the use of numbers as an intermediate between formal reasonings and apprehension of objects (cf. chapter 2). Statistical reasoning provides an example of such an intermediate process of thought. Finally, we affirm that the subject is important because statistics contributed to the development of Mathematical Economics are defined in chapter 2.

It is common to refer to the 1940s, and the development of econometrics as the key period in which the connection between mathematical economic theory and statistics was previously explored. However, this connection has been located earlier since Desrosières (1992, p.2), after Morgan (1990), traced the genealogy of econometrics before 1940. He shows that the connection between statistics and mathematical economic theory was being explored by economists from the 1910s onwards. Even earlier, as a methodologist, Jevons had disagreed with the view that the 'inductive' method usually associated with statistics was opposed to the 'deductive' method usually associated with mathematics. As a marginalist, Jevons did not separate the use of mathematics as a theoretical tool in economics from the use of statistics both at the heuristic stage of the theory and at the stage of its validation.[12] By theoretical tool, we mean both an instrument for organizing a body of propositions as in the 'deductive' method and an instrument for defining concepts. Similarly, Zylberberg (1990) points out that ingeneer-economists such as Lenoir, Cheysson as well as the actuary Laurent (cf. chapter 1, section 2@2) put forward ideas that appear in econometrics in the 1930s. He holds the view that econometrics developed over the time interval 1870-1914. Breton (1991, p411) shares this view since he considers that Marcel Lenoir's thesis (examined in 1913) entitled Etudes sur la Formation et le Mouvement des Prix is the first French thesis to use the then new mathematical statistical technique, and that it is the first French study in econometrics. For his part, Etner(1987, pp174-181) considers that in the second part of the 1880's, the engineer Théophile Ricour(1831-?), developed economic calculations in connection with railways economy, that proceeded from a true econometric approach to economic analysis Consequently, at a time which is considered in economic historiography to be critical to the foundation of mathematical economic theory, such a theory was conceived of in relation to statistics or at least the foundation coincided with early developments in econometrics. We shall now consider the nature of statistics as a discipline at the end of the 19th c.

It is very difficult to give a sharp picture of the state of statistics in the 19th c. Armatte (1991; 1995) shows that from a 20th c. standpoint, it appears to be a combination of various historical trends. This is also the picture that both J.N. Keynes (1917) gives of statistics and Breton(1991, p413) gives of 19thc statistics. Intellectuals working within these various traditions sometimes ignored one another, even though some problems set by one tradition were deal with independently by another. One tradition, which is identified by Theodore M. Porter (1986) as well as by Armatte (1991), is the theory of errors. At the end of the 19th c. the tradition of the theory of errors in statistics was itself very diverse. It was composed of, amongst others, problems formulated in geodesy, artillery, and psychology. It embraced the studies of business cycles and manufacturing control. Another tradition is that of descriptive and comparative statistics which had a German influence. This tradition comprises inquiries financed by governments in order to get a general 'realist' picture of the state of their country. It developed prior to the 19th c. Then there is the tradition in political arithmetic. According to Armatte (1991) it dates back to the 17th c. This is confirmed by Keynes (1917, p.330, note 1). Armatte (1991) associates this tradition with the names of Graunt (1620-1674) and Petty (1623-1687). As for the 18th c., the tradition can be illustrated by the work of Condorcet and Laplace. Not surprisingly, with this inheritance, the subject-matter of statistics in the 19th c. was varied. It ranged from geography to politics and economic subjects.

Unless otherwise stated the information on statistics that follow draws on Armatte's (1991, 1995) study of fifty six treatises of statistics from the beginning of the 19th c. to the mid-20th c. The theoretical foundations of statistics existed in Germany from the mid 18th c.[13] However, the first treatise in which the theoretical side is more important for the author than the applications, dates back to the beginning of the 19th c.[14] Armatte's statistical study consists of a breakdown of the percentage of pages in the treatises cited into theoretical and applied subject-matters. Overall the study shows that the importance of statistical theoretical subjects significantly increased during the 19th c. From the 1860s onwards every treatise but one has more than sixty per cent of its pages dealing with statistical theory rather than with its applications. Consequently Armatte defines a 'second birth of statistics' at the end of the 19th c. Keynes (1917) mentions both the empirical and the theoretical tradition in statistics. From a 20th c. standpoint, the aim of statistical inquiry by the end of the 19th c. appeared to be the formulation of general laws from statistical data, and then the explanation of these laws. Armatte makes the following remark: on the one hand, this aim is not clearly expressed in the 19th c. statistical treatises themselves; but on the other, this aim is stated by Condorcet earlier on, at the end of the 18th c. and the beginning of the 19th c. in the context of applying the probability calculus to moral sciences, even though Condorcet himself does not refer to 'statistics'.

We shall now consider the place of Economics amongst these 19th c. treatises on statistics. Economics appears in Armatte's classification as an application of statistics under the following topics: production, consumption, income, business cycles, actuary, rents insurance, and micro-economics as well as labour and the statistics of the firm. The importance of Economics compared to other applications varies considerably from one treatise to another. Armatte remarks that overall, statisticians in the 19th c.

consider that Political Economy is an ideological discourse, or that it consists in reasonings the assumptions of which are not legitimate. For their part, economists such as J.B. Say criticised statistics on the ground that it provided only static and ex-post pictures of reality (cf. also Breton, 1991). Another critique addressed by economists to statistics is reported in Perrot (1992). At that time, economists, whatever their view on the nature of economic theory, were well aware that, given its then current state, statistics could not provide the theory with a sound empirical basis. This applies to the French economist Jacques Peuchet, according to whom economic inquiry is inductive. It is clear from Armatte (1991, pp.13-16) and Perrot (1992) that Peuchet was opposed to the use of Algebra, Geometry and graphs in statistical analysis meaning the analysis of a collection of government data but not to that of arithmetic in cases when there is a limited number of numerical data.[15] At the same time, Peuchet is in favour of the use of 'complex' mathematics and probability in political arithmetic. However, when it comes to analysing statistical data, Peuchet uses estimation techniques that were of concern to political arithmetic. According to Perrot (1992), the mistrust of statistics also applied to early equilibrium theorists (cf. chapter 6) according to whom economic inquiry was deductive, and to Ganilh (1815) who believed it was both inductive and deductive. According to Breton(1991), not only did French economists mistrust statistics in general until the 1840's; after that period, they still considered that statistics were of secondary importance, compared to political economy, *in the discovery of causal* laws. In contemporary terminology, we would say that these economists agreed that statistical data did not have an axiomatic value for economic theory, in the sense that they did not contain information reliable enough to be taken as valid in a line of reasoning. The following quotation confirms that this view on statistical data was very probably a widespread one amongst economists:

> "But to proceed, we find that the essay of Malthus on population far from being, as many people probably suppose, a collection of rash generalisations and hypotheses, consists mainly of a most careful inquiry into historical and statistical facts concerning the numbers and conditions of mankind in all part of the world. It is a model of inductive inquiry so far as information was available in his day." (Jevons, 1876, p.226).

Later, Keynes (1917 and 1891) similarly assessed the limitations to the use of statistical data.

As far as one can judge from Perrot's (1992) and Armatte's (1991) inquiries, the contrast between the use of statistics in economic theory before the 20th c. econometrics tradition, and its subsequent use by this new tradition, has not been eased by the discovery of new methods of statistical investigation. It seems that these methods and technical devices had been formulated by Laplace and Condorcet before the 19th c. As early as 1795, Pierre Simond de Laplace (1749-1827) suggested ways to dealing with technical problems which were attributed to the limitation, both qualitative and quantitative, of statistics. He later commented on them, before 1814 in Essai Philosophique sur les Probabilités. These methods mostly consist in generalizing about the use of probabilities, as Condorcet also did in his late writings in 1782-1785. Probabilities were previously only used in calculation life rents.[16] But Laplace has promoted the modern ideas of 'significant relationship', 'confidence intervals', 'least square method', and 'scatter phenomena'. These concepts were to be used in cases where chance and natural laws were combined. By chance Laplace, who is known mainly as a determinist means chance resulting from human ignorance.[17] Even though, in retrospect, it so happens that Laplace frames concepts that were sufficient to answer the questions his contemporaries were asking about the application of Mathematics to the study of social phenomena, his important suggestions went unnoticed. One may explain the lack of recognition of Laplace's ideas by biographical or sociological factors. By this light, the time lag between the statement of a statistical calculus and it subsequent development into econometrics appears to be either a sociological phenomenon, such as the willingness by the community of economists to adopt the use of techniques developed by Laplace; or else a mathematical phenomenon, such as the development of these techniques. Perrot (1992) attributes the phenomenon of this lag to the background of metaphysical belief that prevailed in the 18th c. and not to concerns of economic or social inquiry as such. One indicator of this, which is noted in Armatte (1991), is that until the end of the 19th c., statistical studies and the probability calculus developed fairly independently. According to Armatte (1991) two metaphysical views were commonly held in the 19th c. The first was that numbers could represent only material objects or forces. The second was that 'moral' forces were involved in political and economic phenomena, in addition to physical factors. Consequently numbers and probabilities in particular could not represent these 'moral' factors. However, Keynes (1917, p.339; 1891) notices in passing that: "There is a close connexion between the statistical method and the doctrine of chances". He refers to Adolphe Quetelet (1796-1874) and considers that the doctrine of chances may help separate features of statistical data that fall under a law from those that do not.

Even though the use of statistical data in economic theory was criticised by 19th c. economists, Perrot (1992)'s and Etner's(1987) inquires show that together with practical factors dating back to the 17th c., statistics significantly influenced mathematical economics in the 19th c. According to Perrot (1992) a number of new techniques were being developed at the same time: statistics; the use of double entry book-keeping (generating large bodies of data on business enterprises) the use of diagrams. These techniques were a necessity because of the increasing complexity of economic and commercial life. One possible origin of economic curves Perrot mentions might be the political graphs of Pierre Samuel Dupond de Nemours (1739-1817).[18] Another possible origin of economic curves Perrot mentions is the graphs of the long-running empirical studies conducted before 1789, by William Playfair for the British Treasury's department, out of which curves were extrapolated.[19] Perrot (1992, p.31) holds the view that in all likelihood the convex or concave shape of some of theoretical economic curves such as those of Cournot, was suggested by such empirical studies. He holds the view that the development of the mathematical analysis of such curves occurred later, with Cournot in particular. Etner(1987) shows that the work in mathematical economics of the French 19thc engineers was grounded on the analysis of detailed statistical data provided by empirical inquiries or used for the managment of large industrial firm.

This view that there is an empirical origin to Mathematical Economics is independently confirmed by

Stigler (1954). He shows that there have been two approaches to the analysis of consumers' behaviour since the end of the 18th c. One is utility-maximisation techniques. The other consists in generalisations out of empirical observations. He shows that, as far as the theory of income, empirical studies have preceded the theoretical approach, whereas the contrary holds good so far as supply and demand analysis is concerned. This view might also be indirectly confirmed by the comparison of Bicquilley (1804) (cf. chapter 2 note 3) and Jevons (1879). Jevons (1879, pp.74-80) illustrates the inobtrusive occurrence of probability numbers in his 'theory' with the example of a hazardous maritime journey. It might be argued on this ground, that practical commercial problems are the historical origins of his theory. According to Bicquilley (1804) and Crépel (1995), it was usual in the late 18th c. and 19th c. to treat such problems with the help of probability calculus. The reluctance of economists for using statistics must not be overestimated, since according to Breton(1991, p418), the majority of French economists, at the end of the 19thc, considered that statistics could *assess* both economic theories and principles of political economy.

By the mid-19th c., what we would call today statistics had passed the stage of being just a collection of data. By 'statistics' authors belonging to this historical era referred not only to such collections, but also to mathematical analysis which has no immediate reference to any applications. However, some early techniques, which we would today identify as statistical, even though they have existed since the 18th c., had still not identified as such by the end of the 19th c.

There is evidence that some elements of 19th c. mathematical economic 'theory', as defined at the beginning of this paragraph, were suggested both by statistical data and by practical problems. However, it seems that economists as a whole were reluctant to use statistics in their theoretical inquiries. This reluctance did not only concern statistics, but also mathematical economic theory per se, as Breton(1991, especially p419) and Le Van-Lemesle(1991) notice as far as French economists and the pre-1880's period are concerned. In spite of the prejudice of 19th c. economists against mathematical economics, it followed the trend of economics and social sciences towards institutionalisation.

NOTES TO CHAPTER 3

1 "Given that the religions, the philosophical systems and the sciences are social institutions and given that as such, they are the product of a people's civilization, they will succeed one another in the order mentioned above whenever it is not disturbed by historical upheavals, revolutions and alien causes. The very terms of these conditions shows that the exception may be as common as the rule or even more common than the rule. In the cases we find most interesting, exception prevails." (Our translation).

2 Social sciences may also be called moral sciences or human sciences. We choose the former, firstly in order to emphasise the dependence of the activity of human beings on social structures, intellectual activity included. We choose it also to leave the question open of whether there is a social behaviour in animal life. This explains why we consider Psychology and Psychoanalysis to social sciences. Marcel Mauss in 'Une Catégorie de l'Esprit Humain: la Notion de Personne, celle de "Moi"' (in Sociologie et Anthropologie, [1st edition 1950; 9th edition] Paris: P.U.F. (Quadrige), 1985, 428p., pp.333-352) as well as anthropological studies, has indeed shown that the perception of the 'self', which plays an important part in intellectual activity, depends to a great extent on social representations and habits.

3 We are grateful to Professor Reid for signalling this historical figure to us.

4 In 1883, he wrote: Untersuchungen über die Methode er Sozialwissenschaften und der politischen Okonomie insbesondere. In 1903 he published Roscher und Knies und die logischen Probleme der historischen Nationalökonomie. In 1904, he wrote an article entitled "Die Objektivitäte sozialwissenschaftlicher und sozialpolitischer". And in 1917, he wrote about axiological neutrality.

5 We are indebted to John Broome as he was a member of the department of Economics at Bristol (UK) for this suggestion.

6 However, in the 17th and 18th c., according to Perrot(1992) whose research we now report, the issue about the scientific nature of economics was disconnected from that of the mathematization of both economic theory and economic data. Such an author as J.B. Say (1767-1832) for instance, neglected the use of both mathematics and statistics but did not claim that economics was not a science. He did not think mathematics was appropriate for human-related facts because he believed there was a contradiction between human-related values which are necessarily changeable, and fixed mathematical variables. He neglected statistics because they were dealing with the particular, which was not considered by him and by most of his contemporaries as a suitable object for scientific inquiry. Economics began to be claimed a science, in France, in the mid-18th c.; at that time, science referred to knowledge of the general and to the study of cause/effect relationships. The first occurrence of the term 'science' in the title

of a book on economics is in one published in 1751 by Réal de Curban. It had been used previously in the core of economic texts but the term was used in a pejorative sense and it pointed to an illegitimate knowledge or an illegitimate practice. At that time and before, matters that would be regarded today as matters for economic science were dealt with by statesmen. It is later used more generally from 1767 to 1774 (nine books have it in their title) with the development of the Physiocrat school of thought. When this school became less fashionable in the 1770s, the term disappeared until the years 1799, 1801 and 1802. From then on, it became accepted. According to Perrot, this terminological change is not connected with an epistemological or methodological change. Instead, there is a continuity in the authors' quest for rational and causal knowledge in economics. Whether or not this state of affairs which Perrot describes with regard to the history of French economic thought applies to the British case as well is not straightforward; but it might, since at that time, exchanges of ideas were common between economists of these nations. As far as 'Political Economy' (Perrot, 1992, note 27, p.71) mentions its use by Stewart (in 1767), Smith (in 1776), in Britain as well as in Italy.

7 cf. Armatte (1991) for the period 1800-1914 and Bihr *et al* (1995) for the identification of this link today with a particular phenomenon: social inequalities.

8 Comte was indeed opposed to the view that there could be a separated economic science because economic phenomena were essentially connected to social factors.

9 Diachronic communication refers to communication from one human generation to another. Synchronic communication refers to communication with individual of a given generation.

10 We are grateful to Professor Reid for pointing out to us that this statement needs qualifying. In a book review of Jevons' Theory of Political Economy, Marshall may have at first criticized Jevons' use of Mathematics and he assessed this book more positively only later.

11 This does not apply to Jevons, who corresponded with Walras (cf. chapter 5, section 1@1).

12 For details on Cheysson, and his work, see Zylberberg (1990) and chapter 1, section 2@2.

13 However, it seems that Cournot and Walras were opposed to statistics to some extent (cf. Armatte, 1991, p.4 note 35).

14 In this connection, Armatte (1991, p.8) mentions the following names: Conring, de Oldenurger, de Bielfeld, Achenwall (1719-1772). These individuals were professors of Political Economy in German universities who also taught the principles of the analysis of statistical data fairly independently from this analysis itself.

15 The treatise Armatte (1991) refers to is the French 1805 translation by D.F. Donnat of de Schloetzer's writings in: Introduction à la Science de la Statistique, d'après l'Allemand de M. de Schloetzer.

16 Perrot refers to Peuchet's Essai d'une Statistique Genérale de la France mentioned in Armatte (1991, p.6).

17 According to Perrot (1992), the first detailed presentation of statistical techniques applies to economic problems is

Deparcieux's Essai sur les Probabilités de la Durée de la Vie Humaine (1746). Deparcieux refers to de Montmort's and de Moivre's works on probability.

18 This would be called as opposed to in the Aristotleian terminology, because it concerns being who can act (Physics II, 4-7).

19 Perrot refers to Dupont de Nemours' dissertation entitled "Des Courbes Politiques" which is referenced in Perrot (1992, note 77).

20 Perrot (1992, p.30 note 76)refers to William Playfair Tableaux d'Arithmétique Linéaire du Commerce, des Finances et de la Dette Nationale d'Angleterre, 1789, French translation. Playfair later wrote himself a treatise on statistics which is referenced in Armatte (1995, p.584) and which was published 1800.

CHAPTER 4

THE MATHEMATICS OF THE MARGINALISTS

"L'objet mathématique est d'abord caractérisé par l'apparition de «contenus formels», absents de la logique. [Ils] n'ont pourtant pas leur origine dans les *donnés* du sensible [...]. «Contenu» signifie ici propriétés de l'objet échappant d'une certaine manière au système opératoire démonstratif, bien que l'objet en question ait été introduit comme corrélat du système opératoire. La sémantique, pourrait on dire, prend alors une vie propre et se détache de la syntaxe."[1]

G.G. Granger (1994)

INTRODUCTION

Mathematicians and historians of mathematics agree in acknowledging that the 19th c. is important for contemporary mathematics. This is the opinion of Boyer (1949), Edwards (1979), Guillaume (1994), Gispert (1994), Fichera (1994) and Dahan-Dalmedico (1991). Our analysis in this section relies on these authors both for our knowledge of contemporary mathematics and for general reference sources. Many mathematical ideas and also the formalisms that are used today, were developed in the 19th c. The history of Mathematics in this century is also very important, since in this time there has been a crisis surrounding the 'geometrical model', by which we mean both a model for mathematical intuition and for deduction in general. In addition, this period, or more precisely from the mid-19thc. onwards, is important in the history of logic. From that point onwards, the Aristotelian formal logic handed over to symbolic formal logic.

In this section only the first aspect of the history of mathematics will be presented. The second aspect which concerns logic, is important for the study of individual mathematical economists of this period such as Jevons as we shall see. For example, Boole's logic influenced Jevons' epistemological views on science (cf. Chapter 5, section 1). But it is less important in the historiography of mathematical economics since it identifies the marginalist revolution both as having introduced differential calculus in economics and as having promoted the use of mathematics in economics on the grounds that it is the model of rigour, of scientific method and of scientific proof. After explaining what the 'geometrical model' consists of and why mathematicians challenged it in the last third of the 19th c., we shall focus on the state of differential calculus at that time.

In the second section, we assess a particular methodological and epistemological appraisal of the mathematics used by the marginalists, namely that of Friday (1950). Friday's (1950) thesis is considered in this thesis as a fair representative of historiographical approaches to Economics aiming at explaining the use of mathematical formalism in the discipline.

SECTION 1: IN THE CONTEXT OF 19THC. MATHEMATICS

SECTION 1@1: MATHEMATICAL LIMITATIONS TO THE GEOMETRICAL METHOD

Before the 19th c., geometry was important not only within mathematics but for philosophy and science in general. The Elements of Euclid were often considered as the canon for a logically ordered theory. For example, Baruch d'Espinoza (1632-1677) gave his treatise on Ethics the "axiomatic " form of the Elements. As the Scottish philosopher Dougald Stewart (1753-1828) clearly saw, according to Blanché (1955), this order was either used as a rhetorical device or it had the merely logical function of making clear the link between ideas, for communication purposes. For Euclid, it seems that it bore the latter function, whereas Pascal considered that geometric reasoning was a model for rhetoric. In mathematics, many ideas used today in the study of functions were formerly approached only from the point of view of geometry, but this is no longer the case. From the 17th c. onwards integration was associated with the calculus of areas so that, to use the modern terminology, the existence of an integral for a function and the measure of its areas were considered one and the same problem. The continuity of a function was either associated with the idea of a segment of the real line or with the uninterrupted drawing of a curve. Similarly, it was common to illustrate the idea of a limit by considering a circle as the limit of a polygon. We are grateful to Professor Reid for drawing our attention to the Scottish mathematicians such as MacLaurin, who took an active part in the development of the use of geometrical devices in analysis. Colin MacLaurin in his Treatise on Fluxions (1742) attempted to provide geometrical and Euclidean foundations to Newton's fluxions. According to Davie (1964), the study of the Elements of Euclid was the basis of the mathematical syllabus in Scottish Universities in the 18th c. until the 1820s. This over-emphasis on Euclid, compared to the then new developments of algebra (cf. Diagne, 1989), was the core of a controversy on mathematical education in Great Britain in the first half of the 19th c.

Throughout the 19th c., mathematicians build up and explored entities that challenged this geometrical intuition. Such entities or 'objects' were introduced at the beginning of the century. As early as the end of the 18th c. there had been attempts to depart from the traditional geometrical intuition. However, it is in the second part of the 19th c. that these new ideas became a challenge to mathematical communal sense i.e. the experience of the mathematicians considered as a community (cf. Danesi, 1992)[2]. It can be argued that Riemann's 1854 dissertation, which was published in 1867 is a sign of this awareness. This dissertation expounded a new theory of integration. From then on, there were clear attempts in this community to build mathematics on non-geometrical grounds, a process which constituted the axiomatization of mathematics. Before this happened individual mathematicians already called into question the mathematical 'evidence', or common sense. In this connection, Louis Abrogast (1759-1803), Peter Gustav Lejeunne-Dirichlet (1805-1859) and Bernard Bolzano (1781-1848) and also Thomas Reid (1710-1796) and Nikolaï Ivanovitch Lobachevski (1792-1856) were precursors of the later development of mathematics. The first three contributed to the renewal of analysis, the latter to that of geometry. Firstly, the Euclidean axiom that there exists a unique line containing a given point that is parallel to a given line was challenged. This gave birth to alternative geometries, which Thomas Reid had explored, and which Lobachevski published as a system in 1829, but on which he had already been working in the 1820s. Secondly, as far as functional analysis is concerned, in 1829 Dirichlet presented an example of an analytically well defined function, which was, in modern terminology, discontinuous everywhere. In 1834, Bolzano gave an example of a continuous function with no derivative anywhere, as Weierstrass had done in the 1860s, with more success. In 1875, Darboux (1842-1917) studied integrable functions which are discontinuous at an infinite number of points.[3] Similarly, in 1890

Guiseppe Peano (1858-1932) published an example of a function on [0, 1] whose image is a surface, namely a square i.e. a curve filling an area. This amounted to identifying two geometrical objects which were previously considered to be substantially different. Georg Cantor (1845-1918) obtained a set, the triadic cantor set which is a discontinuous set with the power of the continuum, and which has counter intuitive properties in many other respects.

SECTION 1@2: DIFFERENTIAL CALCULUS IN THE 1870S

Let us now consider more specifically the state of differential calculus at the time of the marginalist revolution in the 1870s. Two related historical trends in Western culture converged at the end of the 19th c. to create today's standard calculus. One, that of infinitesimal calculus, can be traced back at least from the 17th c. onwards and its origins are traditionally illustrated with the names of the R.P. Bonaventura Cavalieri (1635; 1598-1647), Wilhelm Gottfried Leibniz (1646-1716), Blaise Pascal (1654-1659; 1623-1662) and Isaac Newton (1642-1727). It can be characterized as an attempt to extend the operations on finite quantities to operations on infinite ones. The other trend is the study of functions. Its origin is unclear but it is closely linked with the study of motion. The mid-18th c. is an important landmark for this trend because Euler identified a function as the combination of several operations. This trend culminates with Vito Volterra's (1860-1940) definition of 'functions of lines' in the 1910s. It consists in disconnecting the function considered as one and the same object from its unique analytical representation. In 1872 several seminal works on the foundations of the calculus were published, described by Boyer (1949, pp.288-289) as follows:

> "It saw, besides the presentation by Weierstrass of his continuous nondifferentiable

> function and the publication by one of his students of Weierstrass' lectures on the elements of arithmetic, (...) the appearance of the following: Nouveau précis d'analyse infinitésimale of Charles Méray [1835-1911]; a paper in Crelle's Journal by Eduard Heine [1821-1881] on "Die Elemente der Funktionenlehre"; the first paper by Georg Cantor [1845-1918] on the principles of arithmetic (...); and the Stetigkeit und die Irrationalzahlen of Richard Dedekind".

Before then, the idea of the derivative of a function was generally unclear. It either referred to an algebraic operation over a differential ratio, as is the case in Leibniz and Carnot's works, or it referred to one particular coefficient in the serial development of a function, as is the case in Lagrange's work. Since the 18th c. it contained the idea of differential ratio of a function: $\frac{F(x+\Delta x)-F(x)}{\Delta x}$, that of its limit F'(x) and that of a differential, $\frac{dy}{dx}$. Bolzano made clear the distinction between f'(x) and $\frac{dy}{dx}$ by interpreting the first as a symbol for a limit and the second as a symbol for a function. If Boyer (1949) has strictly reproduced Bolzano's symbolism, then the one above is Bolzano's. This is partly confirmed by Vojtech (1948). In Voljtech (1948), Bolzano indeed uses the two ratio in the sense mentioned above. However, he does not use the symbols /F'(x)/ or /f'(x)/. He uses instead $/fx/, /\bar{f}x/, /\bar{\bar{f}}x/$, for the derivatives of F(x). With the same proviso, Augustin Cauchy (1789-1857), who also helped in clarifying these concepts, used instead the following symbolism:

$\frac{\Delta y}{\Delta x} = \frac{f(x+i) - f(x)}{i}$, f'(x) and $\frac{dy}{dx}$. He contributed to defining the derivative of a function. Around 1874, there were other notations in use for differential calculus. In thermodynamics for instance, the following were used, according to Ramunni (1988). Rudolph Julius Emanuel Calusius (1822-1888), following Leonhard Euler (1707-1783) it seems, used: $dz = \left(\frac{dz}{dx}\right)dx + \left(\frac{dz}{dy}\right)dy$ the following were also used by others:

$dz = \left(\frac{dxz}{dx}\right)dx + \left(\frac{dyz}{dy}\right)dy \quad dz = \left(\frac{\delta z}{dx}\right)dx + \left(\frac{\delta z}{dy}\right)dy$. The importance of the metaphysical problem of the existence of infinitesimals was accentuated in the 19th c. by the posthumous publication of the philosophical writings of Leibniz. In order to solve this problem, 19th c. mathematicians worked on definitions of the operation of 'taking the limit' of a quantity, which ultimately led them to a definition of the real numbers.[4] These researches generated the contemporary concepts of differential calculus. According to Dahan-Dalmedico (1991, p.208), the ideas of the limit of a series, the limit of a function and the continuity of a function became well defined from the end of the 1870s onwards. That is to say, these ideas were operational in the 1870s. As for the existence of infinite quantities involved both in these ideas and in differential calculus, it had long remained a metaphysical problem.

It can be considered as an algebraic operation on a set of functions or as a calculus on infinite quantities (cf. chapter 4 , note 3). However, Dahan-Dalmedico (1991)'s statement needs qualifying or at least, its application to the case of differential calculus needs scrutinising. Throughout his work, which was published after the 1870s, Peano expressed axiomatic concerns about the ideas involved in functional analysis. For example, in an article published in 1912, Peano (1912) reviewed the different uses and the symbolic representations through history of the derivative and the differential of a function. In this article, Peano quotes a text by Henri Poincaré dated 1899. This quotation suggests that there was no standard notation for these entities at that time.

SECTION 2: IN THE CONTEXT OF 20TH C. HISTORIOGRAPHY

SECTION 2@1: A BORROWING FROM PHYSICS?

One view on the use of mathematics in economics is that they are borrowed from physics. According to Friday (1950) this view was standard amongst economists in the 1940s. In this connection, most approaches to the *history* of mathematical formalism which are concerned with connecting it to the history of science tend to focus on the historical link between economics and physics only.

This is the case of Friday's (1950) thesis on the historical and conceptual relationship between physics and economics. We consider that it usefully portrays this standard approach. It is useful in the sense that as a stylization of most 'external' historical approaches to mathematical economics it therefore contains their main points. We shall now turn to the relevant content of Friday's thesis.
His historical thesis concerns the foundations of the methods used by classical economists. Friday interprets 'classical' in the Keynesian sense and he takes Marshall as the representative of this trend in the history of economic thought.[5] According to Friday the methods used by the classics, including the 'mathematical method' were

similar to the methods of 'classical physics', by which the author means Newtonian physics and cosmology. These methods involved considering an economy or a physical system both as the result of forces bringing about a state of equilibrium and as an isolated system. Also these methods entailed the assumption that time and space were divisible. However, the basic assumption, determinism, held that the subject-matter of scientific analysis endures over time. This assumption is connected to the metaphysical belief in the world's harmony. One cause of this harmony is the existence of unchanging natural laws. These various views on the foundations of knowledge define the 'static' approach to physical phenomena and economic phenomena.

Friday (1950) extends this historical comparison of physics and economics in two directions. One is a comparison of physics and economics in the first half of the 20th c. The other is a view on scientific methodology. In considering the first view, Friday extends the historical comparison between classical physics and classical economics in terms of scientific methodology. He considers that there is a unique scientific method for all the sciences at a given time in history. He attributed to philosophy a pivotal role in providing the foundations of this method for the sciences.

The second view which Friday expresses as an extension of the method of historical comparison, is that economics in the 1950s should be influenced by the emergence of relativist Physics, especially as regards mathematical methods. There are two arguments that support the author's requirement. The first is that there had been a philosophical shift from the pre-19th c. to the post-19th c. Since he confers on philosophy a foundational role, he holds the view that the sciences should echo the philosophical shifts in the methods they use. Consequently economics, as well as all the sciences, should regenerate the classical methods. The second argument is that methods in accordance with the new philosophy were initially developed in Physics , thus

physics is a good representative of this new scientific methodology. Consequently one way for economics to adopt elements of the new philosophy is to use and adapt the methods of the 'new physics'. According to Friday (1950, p.16) the cause of the time-lag between the use of the new method in physics and its use in economics is that: "(...) most economists get their philosophical ideas in a second hand and haphazard manner."

In the main, Friday views the mathematical methods used by the classics as a part of a methodological heritage from Newtonian physics. He does not analyse the improvements of these methods by classical economists.

Neither does he search for mathematical methods that economists may have developed independently. Friday's statement that economics borrowed many of its concepts and methods from the physical sciences is not an isolated case in the historiography of economic thought. No matter whether historians praise these borrowings as Friday (1950) does, or whether they hold the view, as Mirowski (1989) does, that they are unfortunate and unsuitable, or, even, that they are neutral as Perrot (1992) points out, there is evidence of this borrowing. Historical references on this topic can be found in Sebba (1953), or Mirowski (1989, 1990, 1991). They can also be found in economic theoretical texts themselves. We shall identify such borrowing throughout this thesis. At this point, let us mention Adam Smith and Jan Tinbergen (1933a). One connection between Smith's views on Astronomy and his views on economics is the idea of an 'invisible hand'. According to Wightman (1980, note 5, p.49), this connection has been extensively studied.[6] Another connection between Adam Smith's view on Astronomy and his view on Economics is the correspondence he establishes between "gravity" in physics and "sympathy" in social philosophy. According to Lefebre (1995), both are concepts used by Adam Smith to explain a state of harmony either between planets or between individuals. Lefebre (1995) reports that the extent to which Adam Smith's Wealth of Nations ought to be considered as a perfect *translation* of Newton's astronomical system is a very fiddly topic discussed amidst historians of science. Later Tinbergen (1933a) uses the concepts and the formalism of oscillation theory in connection with the study of business-cycles.

Overall, it is highly probable that Friday's account of the origin of mathematical formalism in classical economics is faithful to historical events. However, it is limited from the point of view of the methodology of economics(cf Cheix, 1996).

In addition, it is not clear whether the association of the mathematical tools used by economics with statics is valid so far as the marginalists are concerned. We shall now look for limitations on the view that mathematical formalism in economics has historically been borrowed from physics.

SECTION 2@2: CRITIQUE

In this sub-section, we shall use Friday's (1950) work as a representative of standard historical studies which are concerned with comparing economics with other sciences. We shall analyse the philosophical background of Friday's approach. Then we shall turn to explaining why, from the point of view taken in this thesis, they may impose boundaries on the study of mathematical economics. First we shall consider boundaries to historical studies, and then boundaries to methodological studies.

There are three philosophical views that Friday (1950) holds and that overtly influence his study.
The first is Mill's view on scientific method, which Friday quotes. According to Friday, Mill's view is that if a method succeeds in one discipline it should then be developed in disciplines in which other methods have hitherto been revealed as unsuccessful. The second is the view that the referent of theories in physics is 'the external world'. The third view is that these theories constitute the 'common ground' of various sciences. Even though Friday does not refer to Otto Neurath in his bibliography, similar ideas occur in the latter's work (Neurath, 1931; 1935). They are components of the philosophical view called 'physicalism'. Another characteristic of Friday's view, which is that of

physicalism as well, is the advocacy by its proponents of the unity of science. Neurath (1931, p.49) describes physicalism as follows:

> "In the formulas of science, with the aid of which human beings succeed in understanding one another, only logical-mathematical signs are utilized. [...] How my friend combines the symbol 'red' with other signs clarifies for me the structure of his system of expression. More cannot be done by science. Signs can indicate a 'near', a 'between' and a 'so much', but no more. [...] In a sense unified science is physics in its largest aspect, a tissue of laws expressing space-time linkages - let us call it *physicalism*."

Thus Neurath identifies the symbolic 'mathematical method' with scientific method. In his view, the feature of mathematics which makes it fully adequate for scientific purposes is that it is a way of communicating reasoning that does not rest on metaphysics. This identification is not solely a *historical* remark about the influence of the methods of classical physics on other sciences. The quotation shows that it is a crucial feature of the philosophical programme of physicalism which Neurath (1935) describes in general terms. We shall now turn to analyse two drawbacks to this view so far as the subject-matter of this thesis is concerned.

When used to study the end of the 19th c., physicalism may imply a limitation to the corpus of potential contributors to mathematical economics to physicists. It may also lead to an underestimation of the contributions of supposedly 'non-scientific' figures such as merchants, accountants, engineers, statesmen and financiers.[7] In addition, the identification of pre-1950s 'mathematical method' with static classical analysis may lead to the underestimation of thermodynamics as one origin of the use of mathematical symbolism in economic theory.[8] Economists such as Paul Samuelson and Nicolas Georgescu Roegen were influenced by thermodynamics, and it is clear from Passet (1979), that generally speaking, Development Economics is also strongly influenced by this discipline. In addition, we showed (Chapter 3, Section 1@2) that the symbolism of differential calculus was used in thermodynamics in the 1870s. From a methodological point of view, Friday (1950)'s ideas that the mathematics of economic theory is a borrowing from physics is incomplete. It is obvious from our reading of him that he defines the 'borrowing' of mathematical methods from a historical standpoint, treating it only in terms of the precedence of its use in one field of study as compared to another. We have seen that contemporary methodologists do not content themselves with merely pointing out this historical borrowing, but that they identify 'fortunate' and 'unfortunate' borrowings. It is unclear whether Friday's definition of a borrowing is sufficient to make such an identification. Does Friday consider that the borrowing of differential calculus was fortunate because it coincided with a renewal of the economic theory to which it was applied? There was such a fortunate borrowing in thermodynamics in the 19th c. it seems. According to Ramunni (1988) the use of the then new infinitesimal differential calculus coincided with the development of the concept of entropy and the use of probability distributions. Friday does not pinpoint such a development in economics. In the literature however, the definition of the value of a commodity as a function of the marginal value of that commodity is considered to be such a development. But we mentioned (Chapter 3, Section 2@1) that people disagree on the extent to which this definition of value relies on its expression in terms of the formalism of differential calculus. Another definition of fortunate borrowing would be that it has been used over a longer period of time. According to this definition then, differential calculus would be a fortunate borrowing. Samuelson (1947) still used it in his Foundations of Economic Analysis but it is true that Debreu (1959) brought discredit to it in his Theory of Value. However, according to Ahmed (1993, pp.191-195) differential calculus has been used again in important contexts by Steve Smale (1930-) in the 1970s. Steve Smale considered that it was an adequate tool for approaching dynamic economic phenomena. In addition, he considers that differential calculus served a practical function. It provides a method for the approximation of functions and as such, it can be used as a computing device. Friday's justification of 'fortunate' borrowing differs somewhat from the two definitions we have just mentioned. Friday's justification rather rests on his philosophical view that mathematical physics is a more accomplished science that economics[9]. This view, when applied to the 19th c. and the beginning of the 20th c., leaves out the fact that the use of mathematics in physics itself has been controversial. Allais (1992) mentions a physicist, Bouasse, who criticised the use of Mathematics in Physics in the 1920s. He held the view that there were cases in which the use of mathematical formalism was not necessary to the physical theory, which could be expressed in vernacular language. In addition, the attempt to define the use of Mathematics in Economic theory as mere borrowing from Physics does not draw attention to the special features of mathematical economics that are identified in what follows. The idea that economists have been using, and still do use, concepts and mathematical formalism (such as those of equilibrium), that are used in physics as well is not controversial. However, physicists at the first interdisciplinary workshop with physicists and economists (C.N.R.S., Aussois, March 1994), while acknowledging this borrowing pointed out that such concepts and techniques had a different meaning in physics and in economics. Friday's historical and philosophical definition of a borrowing is not sufficient to accommodate the fact mentioned by physicists that the transfer of mathematical formalism from one field of study to another goes together with a change in meaning. Explanations of such transformations may be found in the philosophy of mathematics or in the theories of interpretation.

NOTES TO CHAPTER 4

1 "A mathematical entity is identified first and foremost with the emergence of "formal contents" which do not belong to logical systems. Neither do [they] come from sensible data [...]. "Content" refers here to these properties of the mathematical entity which can not be accounted for in terms of the operations of the existing demonstrative system; this is the case even though the entity were introduced as a correlate of this system of operations. It is as if the semantics developed a proper life and separated off from the syntax." (our translation).

2 Following Danesi (1992) reporting Giambattista Vico's (1668-1744) analysis of common sense, we hold the view that what is commonly referred to as 'common sense' - which we term 'general common sense', might be divided into 'common sense' on the one hand and 'communal sense' on the other. Common sense refers to physico-biological experiences of human beings, such as sight; communal sense refers to the cultural experiences of human beings. With this principle, we want to point out that when assessing a scientific theory, it is important to bear in mind that it is 'general common sense' oriented in two ways.

First of all, science is grounded on the shared experiences of living human beings. Up to a point, physics and medicine can be considered as being originally founded on common sense. Had our senses been different, then physics would have evolved differently . By contrast, the social sciences are built up from communal sense which involves shared cultural values.

Secondly, science is grounded on the economically and culturally dominant representation of the world available at a given time. This representation is based on vernacular languages.

As for the question whether human beings share general common sense, one way to find an answer to this question is to look for an ontological argument such as a proof or a refutation of 'the real world'. Another way, which is ours, consists in considering that it is a fact that human beings do share general common sense essentially though language. The question then is not to find the ontological ground of common sense. It is instead to describe the process of communication of experiences. It is a linguistic problem, not an ontological one. We believe that in reflecting on the foundations of the sciences, it is impossible not to raise the question of what is meant by "common sense" . We believe that in reflecting on the foundations of the sciences, it is impossible not to raise the question of what is meant by "common sense" .

3 Presumably Dahan-Dalmedico (1991) from whom we take this information refers to Riemann type of integration.

4 This type of answer must be contrasted with that of non-standard analysis in the 20th century, which was instigated by Abraham Robinson (1918-1979/1974). It consists in the definition of new rules for the calculus, adapted to the 18th c. reasoning on infinitesimals.

5 According to Keynes (1936, note 1 p.3), the term 'classical' economist was first used by Karl Marx (1818-1883). Marx designated David Ricardo (1772-1823), James Mill (1773-1836) and their precursors as classical economists. Keynes' definition is broader than that of Marx, but controversial, as it includes many who are regarded as neo-classical economists. In addition, it comprises John Stuart Mill (1806-1873), Alfred Marshall (1842-1924), F.Y. Edgeworth (1845-1926)and A.C. Pigou (1877-1959).

6 The reference Wightman (1980, note 5 p.49) gives is: A.L. Macfie, 1971, 'The Invisible Hand of Jupiter', Journal of the History of Ideas, 32, pp.595-599. This is confirmed in Macfie et al. (1976, note 7, pp.184-185). It is pointed out that Smith first used the phrase 'invisible hand' in the History of Astronomy (III, 2). He uses it also in An Inquiry into the Nature and Causes of the Wealth of Nations (IV, ii, 9) and in the Theory of Moral Sentiments (IV, 1, 10).

7 In the 20th c., there are such contributors. One is the financier Karl Schlesinger (1889-1938). According to Ahmed (1993, pp.80-85) he did not belong entirely to the academic world, which was therefore slow to recognise his work. However, Ahmed argues that he made important contributions to general equilibrium analysis. Even though he did not solve them, Schlesinger was very good at discerning the mathematical difficulties involved in solving general equilibrium equations (e.g. confining prices to non-negative real values). Ahmed (1993, p.83) writes that: "(....) the examination of the existence of competitive equilibrium first began with a paper by Schlesinger." Other 20th c. and 19th c. examples are mentioned in this thesis (e.g. Chapter 2, especially note 3; Chapter 3, section 2@2).

8 For example, it remains to be shown that the French mathematical economist-engineers applied only 'static' analysis to economics, since during their training, they benefited from a good training in the whole range of mathematical sciences.

9 Even though Friday's philosophical view on the historical precedence of physics over other sciences

conforms to historical facts, it is somehow inconsistent. This inconsistency reveals itself if one analyzes the arguments he puts forward to support the claim that there ought to be a new Economics imitating the new physics . Recall the first argument he puts forward, which is that there ought to be a new science because there is a new philosophy. On the one hand Friday points out that the main feature of this philosophical shift is the relinquishing of determinism in favour of a probabilistic approach to the world as well as the focus on the interrelation between phenomena. On the other hand, the author reckons that these shifts were caused by shifts in physics. Overall, there is a contradiction between the thesis Friday holds on the prominent role of philosophy in science and the historical arguments he puts forward for promoting the new economics. On the one hand his thesis is that philosophy is the ground for scientific methodology. On the other, he identifies concepts of physics as the basis for the new philosophy as well as the basis from which scientific methods are to be derived.

CHAPTER 5

EMERGENCE OF MATHEMATICAL ECONOMICS: THE CASE OF JEVONS

"(...) there can be no doubt that the form of the most available arithmetical instrument, the human hand, has reacted upon the mind and moulded our numerical system into a form which we should not otherwise have selected as the best."

Jevons, 1870

INTRODUCTION

This chapter is concerned with the study of the mathematics used in Jevons' Theory of Political Economy. this book is considered as an uncontroversial representative of the neo-classical school of mathematical economics. The first section expounds Jevons' view that generally speaking, mathematics is the method of science. The second section expounds how Jevons qualifies this role in connection to economic science. The third section is concerned first with identifying the mathematics used in the Theory of Political Economy. It also uses the semiotic concept of an 'isotopy' (cf. chapter 2, section 2@2) to define the connection between the mathematical formalism and the English language in the extract of the treatise known as "The Analogy of the Theory of the Lever".

SECTION 1: MATHEMATICS AS THE SCIENTIFIC METHOD

SECTION 1@1: THE PROMOTION OF MATHEMATICAL SYMBOLISM OUT OF PHILOSOPHY OF SCIENCE VIEWS

In the preface to the second edition of the Theory of Political Economy, Jevons complains about the lack of positive impact upon his fellow countrymen of his former attempt at "investigating economics mathematically"(Jevons, 1879, p.xviii). In between the first edition of the book in 1871 and the next, not only did mathematical economics fail to be incorporated into common practice, but paradoxically it also added to the confusion about economics, which Jevons originally wanted to allay (Jevons, 1871a). He reported that debates were focusing on defining "the logical method of the science" and on "the question whether there exists any such science at all". He blamed the predominance of Ricardian thought for the neglect of mathematical economics in Britain and suggested that the case was different in other European countries.

From a historical point of view, he viewed Condillac (1715-1780), Dupuit (1804-1866) and Cournot (1801-1877) as the substantive contributors to mathematical economics. As far as direct influences on his own work are concerned, he referred to French, German and Italian scholars. Amongst the correspondents who contributed to the second edition of the book were Walras and Harald Wastergaard of Copenhagen, who suggested to Jevons the use of the symbolism of differential calculus.[2] From a biographical point of view, his project originated back to the mid-19th c. It sprang from the reading of an 1850 study by Lardner on "Railway Economy". Jevons then developed his thought around 1860 into a synopsis of the Theory of Political Economy.[3] Using his own words, his achievement was to apply differential calculus to economic quantities and specifically to optimisation problems as it is the case in physics. (Jevons, 1879, Preface and Introduction to the second edition). By differential calculus, he meant both the symbolism and the corresponding reasoning on infinitesimals, which extended finite differences analysis. In order to appease his opponents' reluctance to accept mathematical economics, he repeatedly asserted that the novelty of his enterprise did not consist in introducing mathematical reasoning into economic theory but rather in introducing mathematical symbols: "Economists have long been mathematicians without being aware of the fact." (Jevons, 1879, p.xxiv).

If we turn now to his writings, in addition to the symbolism of differential calculus he referred to studies in the physical sciences that used this calculus, namely kinetic and energy studies. Indeed Jevons (1879, pp.101-102) mentioned first the difference between statics (finite quantities) and dynamics (infinitesimal quantities) as a difference in the nature of quantities involved in the calculus, and secondly he compared economic equilibrium in exchange with the physical equilibrium of a lever subject to opposing but balancing forces (cf. chapter 5, section 3@2). On the grounds of these three aspects of this work, Jevons may be said to have been influenced by Mechanics.

The reason Jevons put forward for "a symbolic treatment of the theory" (Jevons, 1871a) was that it would thereby be regarded as scientific. In order to bring about the improvement of the gnosiological status of economic knowledge, he suggested a terminological shift that remains in place today. It consisted of eradicating the "old troublesome double-worded name of our science" - Political Economy and in using Economics instead (Jevons, 1879, p.xiv). He equated the achievement of science in general with physics because, he claimed, physicists agreed about the notions and the words they used (Jevons, 1879, Preface and Introduction to the second edition). He deplored the fact that economists, unlike physicists did not have an unequivocal terminology. He exemplified this inconsistency with the 1870s debate on the measurement of utility: "It cannot be surprising that many debates end in logomachy, when it is still uncertain how many meanings the word *value* has, or what kind of a quantity *utility* itself is." (Jevons, 1879, Preface and Introduction to the second edition).[4]

In his view, the use of symbolism was a sufficient method of reaching agreement.[5] And to paraphrase him: when the real meaning of the formula was seized, mathematics became self-evident(Jevons, 1871a). Not only did Jevons consider that the unequivocal meaning of terms was a prerequisite for sound debates, but he claimed that it was a necessary condition for producing knowledge. Science was the art of producing knowledge from out of the unknown, and one of its principles was the identity principle. As economics dealt with quantities, this art would consist of a symbolic algebraic calculus, also called Universal Arithmetic (Jevons, 1864, §15).[6] In economics, this methodology was in his view practised by Cournot, who:

> "Presents a beautiful example of mathematical reasoning, in which knowledge is apparently evolved out of ignorance" [...] "In reality the method consists in assuming certain simple conditions of the functions as conformable to experience, and then disclosing by symbolic inference the implicit results of these conditions." (Jevons, 1879, p.xxxiii).

However, the mathematical symbols were not necessary for the performance of the calculus. Whenever one used the words 'equal' and 'ratio', one was reasoning mathematically. In addition, referring to Laplace for authority, Jevons wrote that mathematical results could be expressed in language. In economics in particular:

> "Whether mathematical laws of economics are stated in words, or in the usual symbols, x, y, z, p, q etc., is an accident, or a matter of

mere convenience." (Jevons, 1871a and 1879 p.4).

The use of symbols in science is therefore a sieve for consistency and, it seems, a visual device to recognise identity of terms. Jevons (1870, §3) notices that:

> "mathematicians are well aware that their science, however much it may advance, always requires a corresponding development of material symbols for relieving the memory and guiding the thoughts."

Similarly, he asserted that mathematical symbols facilitated reasoning. In Economics in particular, they helped to handle complex relations and a large body of data (Jevons, 1871a).

According to Jevons therefore, symbolic mathematical investigation in science and Economics in particular, had a cognitive and a heuristic function. The former was a sufficient condition for the latter to be efficient, but only the latter was typically scientific in Jevons' terminology.

SECTION 1@2: THE LIMITATIONS TO THE USE OF MATHEMATICS IN SCIENCE AND SCIENTIFIC PROOF

The latter view on the connection between science and mathematical symbolism must be contrasted with Jevons' view that mathematics have no demonstrative power, a statement that is correlated with the assertion that mathematical symbols are obscure. Both Jevons' alteration of Boole's logical system and his logical machine are illustrations of his views. He wrote that Boole "shrouded the simplest logical processes in the mysterious operations of a mathematical calculus" (Jevons, 1870, §10; 1864) and criticized Boole's system (Jevons, 1864; 1870). This led Jevons to alter the notation of Boole's calculus. In addition, he disagreed with Boole on how sentences connected by 'and' in common language had to be symbolised. Boole made the distinction between the exclusive and inclusive 'and'. It is admitted that he represents the former by a(1-b)+b(1-a) and the latter by a+b(1-a). Jevons held the view that this distinction should not be made in logic because logic had to stay close to general common sense and any symbolic result should be easily interpreted. Also Jevons criticized the fundamental Boolean law, namely x.x=x on the ground that it was not obeyed by all numbers. As far as the logical machine was concerned, he considered that:

> "The chief importance of the machine is of a purely theoretical kind. It demonstrates in a convincing manner the existence of an all-embracing system of Indirect Inference, the very existence of which was hardly suspected before the appearance of Boole's logical works." (Jevons, 1870, §56).

He also considered that his machine was a "visible proof".[8] Consequently Jevons' wish to achieve the "mechanics of utility and self-interest" (Jevons, 1871a) in economics proceeded from a broader mechanistic view of science, whose abstract findings, first are mechanically produced, and secondly have to be materially or technically embodied in order to be proved.

The influence of theological Unitarian doctrine, as well as Jevons' original training in engineering, partly explain the contrast in his opinions about mathematical symbolism as well as his mechanistic-technological view on science. The importance of such doctrine on Jevons' life is remarked upon by R. Könekamp (1972). The Unitarian doctrine was influential in the British Isles from the mid-19th c. to the early 20th c. and it was associated with social idealism and faith in scientific progress. This influence explains Jevons' trust in the laws of thought "on the habitual use of which our existence as superior beings depends" (Jevons, 1870, §10) and his mistrust of reasoning by symbols alone. The epistemological facet of the Unitarian doctrine is indeed agnosticism. In so far as thought is a superior and divine feature, its laws cannot be known by human beings if they are not materially embodied. In Jevons (1972, p.202), there is evidence that in 1866 Jevons had contacts with the Unitarian milieu. However, his commitment to agnostic ideas was expressed in 1857 (Jevons, 1972, pp.154-157) when he wrote against revealed religion that:

> "God is seen if anywhere in the wonderful order and simplicity of Nature, in the adaptation of means to ends, and in the creation of man to which everything refers, with power capable of indefinite improvement.(...)
>
> I feel no conviction of anything because it is the Bible and I examine Matter and Mind in order to found my conception of God.
>
> I perfectly comprehend everything that may be deduced from Nature, as to *design, order,* unity of conception &c of the universe, and I confess that both the theory of *Chances* and that of *Conditions of existence* are perfectly inadequate explanations."

This influence on Jevons' views on symbolism must not be overestimated, since he considers that symbols belong to material phenomena and that knowledge of the laws of thought is not out of the reach of human beings . In the sentence mentioned above he adds:

> "...only well-trained mathematicians could ever [if Boole's system were to retain its "peculiar mathematical form"] *comprehend the action* of those laws of thought..." (Jevons, 1870, §10, emphases added).

In addition, the use of machines to solve problems set in abstract or symbolic terms is not only a biographical feature of Jevons' work, it is also a general cultural feature of both physics and mathematics (cf. Rashed, 1991).

Furthermore, part of his mistrust of symbolism can be explained by pedagogical motives since Jevons was concerned with the vulgarisation of scientific ideas, both in the Theory of Political Economy and in Pure Logic.[9] The theological influence was greater on his overview of scientific method, for which he was accused of being a materialist (Mays, 1962).

SECTION 2: SCIENTIFIC PROOF, SCIENTIFIC METHODS AND ECONOMICS

Jevons' approach to science was not restricted to the view that qualitative algebra (logic) and quantitative algebra (mathematics) had a crucial heuristic function in science. This view on the method of scientific discovery was supplemented by his view on what he called the "logical method of science" and the "philosophical method of science". Let us consider these methods in the exact science of economics or, to use contemporary terminology, the empirical science of economics.

A science is exact to the extent that a confrontation with facts or data is part of the assessment and framing of its theories(Jevons, 1871a). In this respect, Economics must pay tribute to Cournot for grounding his theoretical assumptions on measurable observations instead of *a priori* notions, rather than to Canard, Whewell, Esmenard, du Mazet and Du Mesnil-Marigny did (Jevons, 1879, Preface and Introduction to the second edition).

The complete logical method of Economics consisted in observation, deduction and induction:

> "Possessing certain facts of observation, we frame an hypothesis as to the laws governing those facts; we reason from the hypothesis deductively to the results to be expected; and we then examine these results in connection with the facts in question; coincidence

> confirms the whole reasoning; conflict obliges us either to seek for disturbing causes, or else to abandon our hypothesis." (Jevons, 1871a and 1879, p.19).

Observation involved what we would call today introspection because the "laws of Economics", also called axioms were: "Known to us immediately by intuition, or, at any rate, they are furnished to us ready made by other mental and physical sciences." (Jevons, 1871a and 1879 p.19).

These laws were valid at the individual and the nation-wide level. There were three such laws. The first was that, confronted with economic choice, it was natural to choose the better apparent good. The second was that there were degrees of fulfilment of wants. The last was that the time allocated to work and the subjective experience of pain were proportional. It seems that, ideally, the observations were those of an individual, and that introspection was a particular case of such an observation:

> "I must here point out that, though the theory presumes to investigate the condition of a mind, and bases upon this investigation the whole of Economics, practically it is an aggregate of individuals which will be treated."(Jevons 1871a and 1879, p.16).

Consequently, Jevons pleaded for the statistical appraisal of economic laws alongside results deductively obtained from these laws.

According to Jevons, the use of statistics was, it seems, a specific feature of the social sciences. It provided an answer to specific methodological problems encountered in these sciences as opposed to physics. Because social facts deal with human action, it is hard to disentangle their causes. It is hard therefore to make trustworthy predictions to test social theories and to perform measurement of social variables. In both of these functions, statistical analysis ought to supersede direct observation of individuals. Statistical analysis involved social experimentation. Jevons held the view that institutions (e.g. Parliament) were experimental instruments for the social sciences and, conversely, that institutional measures were an unintentional form of social experimentation. Even though observation of statistics may reveal regularities in social phenomena that do not appear at the individual level, their significance was limited. Factors which limited statistically determined laws, and therefore scientific theories, involved large scale social changes, such as migration flows and social discontent(Mays, 1962).

Because social phenomena have characteristics that differ from physical phenomena, the method of observation differed in the social sciences and in the physical sciences. But the deductive mathematical method was common to all empirical sciences:

> "Many persons seem to think that the physical sciences form the proper sphere of mathematical method, and that the moral sciences demand some other method - I know not what." (Jevons, Theory of Political Economy, quoted from Mays, 1962).

As far as economics is concerned, one argument Jevons puts forward to warrant the use of mathematics and of differential calculus in particular, is that economic quantities are continuous. By continuous, he means that they change continuously with time, a position that rests on his view that economic phenomena are essentially dynamic phenomena.

The general scientific "logical method" was completed, in economics, by the "philosophical method", by which Jevons meant the historical method (Jevons, 1871a). In his view, any subject-matter had two aspects: a logical/formal aspect and a historical aspect(Jevons, 1864 and Mays, 1962). Even though he was designated as an opponent to the historical method in the 19th century's methodological controversy about the social sciences, he was not opposed to it and promoted it as the basis for a branch of social science. It is worth mentioning that, according to Könenkamp(1972) he viewed economics as part of anthropology. In addition, Economics *stricto sensus*, was only a part of economic science:

> "As I have previously explained, the present chaotic state of Economics arises from the confusing together of several branches of knowledge. Subdivision is the remedy. We must distinguish the empirical elements from the abstract theory, from applied theory, and from the more detailed art of finance and administration." (Jevons, 1879, pp.xxi-xvii).

As he subsequently mentioned, one can subdivide economic science from a methodological point of view into theoretical, empirical, historical and practical treatments of the subject-matter. The subdivision may well be subject-based (Economic Sociology, Fiscal Science, Mathematical Economics). The methodological unity of science must however be granted and Jevons suggested that the general principles of Mechanics play such a unifying role. In addition to the phenomenological argument (some economic phenomena are quantities) and the epistemological argument (calculus produces knowledge) for investigating economics mathematically and symbolically there was a 'political' argument to it. To a certain extent, using coherent and unequivocal terminology would eliminate unnecessary debates and makes Economics look a more coherent subject.

On the whole, and generalizing from Jevons' views, one can interpret the marginalist's advocacy of the use of mathematics as follows.
The marginalists who promoted the use of mathematics in economic theory identified this use as the most important feature that would make Economics a science as opposed to a form of Politics in particular. It seems that in previous centuries, this identification was not common (*cf* Cheix 1996, note 16). Both Walras and Jevons considered that using Mathematics results in separating

the assessment of political proposals from their consistency. In philosophical terms, they believed it was a way of removing teleological issues about knowledge from the scope of knowledge itself. They were also committed to the view that logico-mathematical structures could be assessed out of pragmatics, since they believed mathematics was 'neutral'.

Retrospectively, one can venture also the following view on the motivation of the marginalists for the mathematical and 'neutral' approach to economic ideas, particularly as applied to utility. The use of utility-maximisation techniques in order to explain achievable levels of social welfare and efficient trade levels, provided economists with the means to put forward alternative economic systems (e.g. socialism) and improvements to existing systems (e.g. by correcting market failure) without challenging existing economic and political organisations. In this connection, one must recall that in the European 19th c. there was a contrast between the refinements of Economic science, economic prosperity and technological achievements related to the industrial revolution on the one hand, and devastating poverty on the other.[11] Thanks to Mathematics, these alternative systems appeared as natural logical results or idealized frameworks endowed with authority but with no pragmatic consequence attached to them.

As for Jevons' view on the use of statistics in economic theory, there are, as Professor Reid has suggested to us, similarities between these views and later methodological views. For example, Jevons' view that statistics can be used to assess the conclusions of deductive reasoning was later used by members of the Chicago School of political economy from the mid-1930's onwards. These members were Milton Friedman (1912-), George Stigler (1911-), W. Allen Wallis, Henry Simon (1899-1946) and Aaron Director. As a group, they are known as empiricists, by which it is meant that they were in favour of testing economic theoretical propositions against statistical data.
Similarly, the idea that Economics does not consist merely in observing phenomena but also in social experimentation has been developed in the mid-20th c. Authors who are referred to in this connection as the originators of this approach are F. Mosteller, P. Nogee, E. Chamberlin for their studies of market behaviour. Later, similar studies were conducted to test game theoretical analysis. In addition societal experimentation are conducted in politics on the pattern of medical experimentation with control groups and groups that are being experimented upon. This is often the case in taxation policy.

SECTION 3: MATHEMATICS IN THE THEORY OF POLITICAL ECONOMY

SECTION 3@1: DESCRIPTION

Jevons identified his main achievement in the Theory of Political Economy as being the introduction of differential calculus into economic theory. However, Breton(1991), Etner(1987, p154) and Theocharis(1993, pp.178-197, esp.196-197) show that earlier authors had used marginal reasoning or differentials in connection to mathematical analysis Breton's(1991) reports on the works of Gustave Fauveau (cf. also chapter 1, note 4) suggests that this author might have used integration and differential calculus before or at the same time jevons did. More significantly, Etner(1987) quotes Jules Dupuit(1804-1865) who used a differential equation in 1844 in his attempt at assessing the utility of the building and the use of a bridge. The authors Theocharis mentions are Daniel Bernouilli and Paolo Frisi in the 18th c. as well as Georg von Buquoy , Charlemagne Courtois and Augustin Cournot in the 19th c[12]. In addition, he mentions von Thünen(1783-1850) as the first economist who has formulated the mathematical theory of marginal productivity, even though he did not touch upon marginal utility. He used the idea of the marginal productivity of a field in an attempt to work out the maximum revenue one can get from a field. And he stated that this problem ought to be seen as the mathematical problem of maximizing a function of two variables. To judge from Theocharis' (1993) report, Von Thünen did not use the symbolism of differential calculus. It is difficult to check this piece of information because, as Hall (1966) notices there are many versions of Der Isolierte Staat and they are not easily available. Generally speaking, the symbolism used in Hall (1966) and

Von Thünen (1850) falls within the province of bookkeeping rather than within the province of differential calculus. However, there are isolated occurrences of /x + dx/, /dx $\sqrt{ax}$ / (Von Thünen, 1850, p.538), / $d\left\{\frac{p-[a+y]y}{q(a+y)}\right\}$ / (Von Thünen, 1850, p.549) and also of / $(1+qz)\frac{pdz-pqzdz}{(1+qz)^2}-adz=0$ / (Von Thünen, 1850, p.551) in the study of labor in relation to capital and rent. The last two expressions, which are used to work out a maximisation problem, are explicitly identified with a differential functions and their algebraic calculation is actually carried out by Von Thünen. According to Theocharis, Walras, Menger and Jevons did not know the work of Von Thünen in this connection at the time they were building up their own theories, whereas Marshall did.[13]

The mathematical notations of the first edition of the Theory of Political Economy are "the limit of the fraction $\frac{\Delta u}{\Delta x} is \frac{du}{dx}$ where Δ refers to finite variation and $\frac{du}{dx}$ is the differential coefficient of u. They are used in the theories of rent, exchange, utility and labour. In addition, the first edition contains the ratios $\frac{F(t+\Delta t)-F(t)}{\Delta t}$ and $\frac{dFt}{dt}$ in the theory of capital. According to Professor Reid, Jevons got the idea of using differentials from his teacher De Morgan at University College London. It is clear from Jevons' Journal that he studied De Morgan's lectures thoroughly (Jevons, 1972, p.65 and p.69). Provided Boyer(1949) reproduces original symbolism (cf. Chapter 4, section 1@2), Jevons' symbolism is that used by Bolzano, except from the notation 'F't'. This is confirmed in Vojtech (1948).

In addition to Differential Calculus, Jevons uses thirteen two-dimensional diagrams. Some diagrams he uses (Jevons, 1879, pp33-34, p50, p53, p.136, p250, p257, p279) are of the kind which is usually used nowadays as a visual representation of Riemanian-integral of functions. Even though it is clear, in utility theory at least, that he identifies $\frac{du}{dx}$ with a line and u with an area so as to suggest that u is the integral of $\frac{du}{dx}$, he does not use the corresponding symbol of integration except in one chapter. Jevons (1879, pp.110-111) justifies this lacuna as follows:
lacuna as follows:

> "The process of integration, if I understand the matter aright, only ascertains other equations, the truth of which follows from the fundamental differential equation."

The exception concerns the theory of capital (Jevons, 1879, pp252-253) and the calculus of the total magnitude of repeated investments. He represents this total investment with the symbol $\sum t.\Delta p$ and remarks that it is 'the customary mode of expression'(Jevons, 1879, p.252) of total investment. He uses this kind of diagram to represent a magnitude that depends on two variables which he refers to as 'dimensions'[14]. Or else he uses them to illustrate this reasoning involving comparisons between a magnitude and its parts. In addition, in one case (Jevons, 1879, p.136) the reasoning involves a comparison of two derivatives through the comparison of two areas. When time or capital are involved as variables in the diagrams he is commenting on, Jevons does not justify the extension of his reasoning from finite variations of the variables or from finite divisions of the variables to infinitesimal ones. However, when physical quantities such as food are the variables, he does. The argument he gives for considering infinitely small quantities of a commodity can be put as follows. From an ontological point of view, these quantities represent finite quantities. From an epistemological point of view, the quantities they represent are infinitely small compared to the aggregate finite magnitude they make up. Jevons uses another diagram to represent the exchange of two commodities, even if he remarks that: "It is hardly possible to represent this theory completely by means of a diagram."(Jevons, 1879, p.104).

Two diagrams are also used by Jevons(1879, p.156) to represent the "functions of utility" of commodities in the theory of exchange as the area under a curve. In the theory of labour, the utility of producing labour and the utility of the corresponding output are compared geometrically on a diagram(Jevons, 1879, p.187). The theory of rent is similarly illustrated but the reasoning on the diagram(Jevons, 1879, p238) involves comparisons of derivatives. The Theory of Political Economy contains one geometrical figure (Jevons, 1879, p251) in the theory of capital. It is an isosceles triangle. It is used together with a reference to Euclid's Elements to justify the choice of relevant magnitudes in the calculus of investment.

Finally, the Theory of Political Economy contains a figure that illustrates the mechanical theory of the lever (1879, p.114).

In addition, the book contains numerical examples and tables. One of the tables is referred to in the literature (cf. Stigler, 1994) as the King-Davenant Law and it relates the price of corn to the harvest sizes. Jevons (1879, pp.169-174 works out a function that approximates the data of the table. In the theory of utility, there is an analysis of what we would call today 'expected utility'. In this analysis, Jevons uses probabilities that refer to the uncertainties of nature that might be estimated(Jevons, 1879, p.77-80). This analysis is not an attempt at introducing probability calculus into economic theory. It is instead an attempt at showing how practical economic problems actually dealt with calculus (e.g. insurance problems in maritime business which Bicquilley (1804) had also dealt with, cf. chapter 3 section 2@2), are consistent with the theory of

SECTION 3@2: SEMIOTIC ANALYSIS OF THE ANALOGY OF THE LEVER

In this subsection, we are concerned with defining isotopies in the Theory of Political Economy along the lines defined above (cf. Chapter 2, section 2) and with studying their relationships both within the text itself and in the light of the historical context (cf. Chapter 3 , Chapter 4). We shall focus on the 'Theory of Exchange' (Jevons, 1879, Chapter 4) in particular because in this chapter, one of the most important semiotic functions of the text is defined. This function consists in giving an economic content to symbols used in differential calculus. It is important with regard both to the historiography of economic thought and to Jevons' explicit purposes (cf. Chapter 4 and Chapter 5, Section 1@1). Evidence supporting this latter point are Jevons' introduction of the 'Analogy to the Theory of the Lever' in the second edition in order to reinforce this connection.

The first isotopy we shall identify is the 'notational and graphic isotopy'. We shall give an ostensive and non-comprehensive definition of it. The following are notations: /AA'/, /P/, / $\frac{AA'}{BB'}=\frac{AC}{BC}$ /, / $\frac{\Delta y}{\Delta x}$ / , /f/, / $\frac{dy}{dx}=\frac{y}{x}$ /, /PxArcAA'/, /Φ_1(a-x)/. Notations are a subclass of expressions, they contain typographical designs that do not belong to the vernacular alphabet, or else they are isolated letters with no obvious grammatical function. In Jevons' explicit terminology, notations are called 'symbols'. Amongst the figures are graphs of functions and also a diagram representing a lever moving away from equilibrium (cf. Chapter 5 , Section 3@1).

The second isotopy is the 'economic isotopy'. It is defined by the redundancy of expressions belonging to economic vocabulary in vernacular contemporary English.[15] Such expressions are /commodities/, /utility/, /exchange/, /market/, /supply/, /demand/.

The third isotopy is the 'mechanical isotopy'. It is defined by the redundancy of expressions which: (a) Jevons *identifies* as belonging to mechanics, by quoting and paraphrasing books from this corpus; (b) occur in 'Analogy to the Theory of the Lever' (Jevons, 1899, pp.110-115). Such expressions are /fulcrum/, /force/, /the law of energy/, /resistance/, /Theory of virtual velocities/, /the work done by P equals P x arc AA'/, /the angle of ACA'/, /finite arcs/, /work/, /perpendicular to/, /$\frac{W}{P}=\frac{AA'}{BB'}=\frac{AC}{BC}$/.

The fourth isotopy is the mathematical isotopy', it is defined by the redundancy of expressions that are used in contemporary mathematics, such as: /equations/, /integration/, /differential equations/, /infinitesimals/, /the angle ACA'/, /perpendicular to/, /finite arcs/, /algebraic sum/, /$\frac{AA'}{BB'}=\frac{AC}{BC}$/ as well as graphs[16].

The 'notational isotopy' concerns the level of expression. the 'economic isotopy' and the 'mathematical isotopy' concern the level of content. The 'mechanical isotopy' concerns both the level of expression and the level of content. It is defined indeed both by the occurrence of expressions *in a particular extract* of the text, and also by the membership of the corpus of mechanics, which gives a specific content to these expressions. It can be considered that this isotopy is a non-vernacular extra-textual semiotic function[17]. All isotopies are explicit isotopies. The identification of the first three isotopies requires no additional ability than knowledge of vernacular language. That of the 'mathematical isotopy' however, requires specialised mathematical knowledge.

The mathematical isotopy is directly connected to all other isotopies. Its connection with the 'mechanical isotopy' is part and parcel of the semiotic function defining this isotopy in other words, it is, for Jevons himself, a legacy from the history of physics.[18] Jevons defines the connection between the mathematical and the economic isotopy by using figures and symbolism to represent his arguments. Avowedly, the connection between these arguments and geometrical figures is partial, whereas it seems that he considers that their connection with symbols is comprehensive. As far as the connection between the 'notational' and the mathematical isotopy is concerned, we showed that it is to Jevons a legacy of the history of mathematics (cf. Chapter 4, and Chapter 5, Section 3@1).

The connection between the 'mechanical isotopy' and the 'economic isotopy' is achieved at the level of expression with the use of vernacular expressions: /equilibrium/ and /body/ and also with the use of one notational expression in particular: / $\frac{\Phi x}{\Phi y}=\frac{dy}{dx}=\frac{y}{x}$ / in the two isotopies. We shall now analyse the role of this latter.

At the mechanical isotopy levels, $\left[\frac{\Phi x}{\Phi y}\right]$ is defined as the ratio of two finite forces applied at the ends of a lever (1879, pp.113-114). According to the diagram used by Jevons (1879, p.114), he considers that these forces are functions of the position of the fulcrum. $\left[\frac{dy}{dx}\right]$ is a ratio of finite or infinitely small displacements AA' and BB' (1879, pp.111-113). $\left[\frac{y}{x}\right]$ is a ratio defining the position of the fulcrum (1879, p.113). In / $\frac{\Phi y}{\Phi x}=\frac{dy}{dx}$ /, [=] is the law of energy (1879, p.111). In / $\frac{dy}{dx}=\frac{y}{x}$ / [=] is the law of virtual velocity (1879, pp.112-113) , that is, the proportionality of the finite or infinitely small arcs ($Arc_R\alpha$) of a circle to the radius (R) (1879, pp.112-113). In modern trigonometric terminology, [=] is the equation $Arc_R=\alpha R$.

In the 'economic isotopy', $\left[\frac{\Phi x}{\Phi y}\right]$ is the ratio of finite, final utilities of the quantities x and y of the goods exchanged (1879, p.113). $\left[\frac{dy}{dx}\right]$ is explicitly a ratio of exclusively infinitesimal quantities of the commodities exchanged (1879, pp.113-114). $\left[\frac{y}{x}\right]$ is the ratio of the final and finite quantities exchanged (1879, p.114). In / $\frac{\Phi y}{\Phi x}=\frac{dy}{dx}$ /, [=] is the "general form" of the definition of economic equilibrium (1879, p.104 and p.113). In / $\frac{dy}{dx}=\frac{y}{x}$ /, [=] is the Law of Indifference (1879, p.103 and p.114). This law may be called the 'unique price hypothesis'. It states that on a market with perfect knowledge, no parts of given quantities of homogenous commodities can be exchanged in a ratio different from the ratio of the given quantities. Hence Jevons defines not only term by term correspondences between the equation of the notational isotopy on the one hand, and the mechanical and economic isotopies on the other hand; but he also defines correspondences between [=] at the notational level (i.e. the rule of substitution of symbols) and scientific laws. These correspondences imply a third implicit correspondence between the mechanical and the economic isotopies: (a) The idea of a 'force' and that of the 'utility' of a good (or 'value') play similar roles. This is a point Mirowski (1990) has justly noticed about 19th c. neo-classical analysis in general. (b) Similarly, $\left[\frac{y}{x}\right]$ may be considered at the characteristic feature of the phenomenon (machine, exchange) which is analysed. (c) As far as $\left[\frac{dy}{dx}\right]$ is concerned, the correspondence is less clear, which is confirmed by Jevons' subsequent alteration of the first edition and we shall go back to it later.[19] (d) The trigonometric law used in mechanics corresponds to the Law of Indifference. However, in the economic isotopy, this law, as Schabas (1990, pp.39-40) notices, is specifically an economic law, whereas in the 'mechanical isotopy', it is a mathematical law as well.[20] (e) It could be argued that the correspondence between the economic law of equilibrium and the 'law of energy' is limited At the mechanical isotopy level, there is a slight terminological shift in the application of this law to finite quantities (law of energy) and in the application of this law to infinite quantities (law of virtual velocity). This shift does not occur in the economic isotopy.

Consequently, the correspondence between the expressions of the equation / $\frac{\Phi_1 x}{\Phi_1 y}=\frac{dy}{dx}=\frac{y}{x}$ / at the economic and at the mechanical level is not a perfect substitution process. We shall now analyse this limitation in the above case (c) in particular.

Jevons definitely maintains that $\left[\frac{dy}{dx}\right]$ is a ratio of infinitesimal *distances*. He does not identify $\left[\frac{dy}{dx}\right]$ with a differential coefficient, nor with a virtual velocity coefficient, or to use his terminology, $\left[\frac{dy}{dx}\right]$ is not an "abstract number" (1879, p.90). However from a contemporary standpoint, the physical and trigonometric laws he uses involve such abstract numbers.[21] At the economic isotopy level on the contrary, even though Jevons (1871b, p.93) claims that "the ratio of exchange $\left[\frac{dy}{dx}\right]$ is really a differential coefficient", he expounds his economic theory in such a way that at first glance, it does not seem to use differentials nor infinitesimals. For example, he confirms criticisms addressed to him that he does not use the integration process associated with differentials (1879, p.110). He also heavily stresses both implicitly and explicitly that the use of infinitesimals can be avoided (1879, respectively p.114 and pp.106-108). In addition, he uses finite increments of quantities of goods exchanged when he first expounds the symbolic treatment of the theory of exchange (1879, pp.106-107) in such a way that the introduction in the subsequent passage (1879, pp.107-108) of /dx/ and /dy/ is not convincing at first. It seems that /Δx/ and /Δy/, which stand for finite quantities throughout the text, could be used just as well. No new element is added to the *equilibrium principle* used in the passage where /Δx/ and /Δy/ are used, when it is applied in the passage where /dx/ and /dy/ are used. Consequently, it can be considered that $\left[\frac{\Phi x}{\Phi y} = \frac{dy}{dx}\right]$ is the generalisation of the equation $\frac{\Phi x}{\Phi y}=\frac{\Delta y}{\Delta x}$ (which Jevons states in words only) to infinite quantities, on an *exclusive*ly symbolic basis. Following this interpretation, it can be argued that the formalism of differential calculus is redundant and that it only aims at making the law of exchange literally look like the law of mechanics. To summarize this argument, it could be pointed out that Jevons does not consider that / $\frac{dy}{dx}$ / in an indivisible expression or a singular concept, but that it is the compound of /dx/ and /dy/. In this connection, it can be argued that there is no proper economic concept corresponding to that of $\left[\frac{dy}{dx}\right]$ in the mechanical isotopy (i.e. that of velocity).

However, there is also evidence that Jevons (1879, pp.106-108) used two mathematical properties of a "utility function" which first are not consistent with the definition he gives of utility, and secondly, are two mathematical features defining the idea of the differential of a function according to today's standards. On these grounds, it can be argued that the mathematical entity occurring in this extract is the differential of the utility function. These properties are (a) that it is linear with respect to the second variable; (b) that according to Jevons, the (total) utility U of a good is not separable from the exchange of a finite quantity x of this good. Consequently, in the following quotation, the two occurrences of /utility/ do not refer to the same function.

> "Now the increment of beef, Δy, is $\frac{y}{x}$ times as great as the increment of corn, Δx, so that, in order that their utilities shall be equal, the degree of utility of beef must be $\frac{y}{x}$ times as great as the degree of utility of corn." (Jevons, 1879, p.107).

If it were the same function, in order to be correct, Jevons' reasoning would imply that the utility function is linear with respect to scalar multiplication. It would also imply that this function is not dependent on the fixed quantity exchanged. /U(Δx)/ would indeed refer both to the utility of an increment Δx in the exchange of x and also the utility of the exchange of Δx).[22] Similarly, we consider that $\Phi_1(a-x)$. dy = $\psi_1 y$. dx is an equality between differentials: given the initial endowments and zero of the individual 1 in the two good exchanged, given his utility functions Q_1 and ψ_1 for each good, the equilibrium of exchange occurs for the quantities x and y of these goods such that $d_a Q_1(x) = d_0 \Psi_1(y)$ where $d_u f(t)$ is the value at point t of the differential of f at u.[23]

NOTES TO CHAPTER 5

[1] However, Zylberberg(1990) and Zouboulakis (1993) suggest that as far as France is concerned the case did not differ from the British.
[2] According to R.D. Collison-Black (1970), Jevons received Walras' Principes d'une Théorie Mathématique de l'Échange in 1874. The latter helped the former with the compilation of the bibliography on mathematical economics. (Jevons, 1879, Preface to the second edition).
[3] R. KôneKamp (1972) reports that Jevons wrote a 'notice of a general mathematical theory of political economy'in 1862. She probably means the drafts of Jevons' (1866) article. R. Kônekamp adds that in 1857, Jevons was to write on 'Formal Economics', a study of which there is no trace.
[4] He mentions that in the literature, utility either means numerical ratio, mental state or mass of commodities. Value either means value in exchange or in use.
[5] This view is identical to G. Boole's belief that the Algebraic Calculus can be applied to disentangling theological controversies. He applied it indeed to the analysis of Clarke and Spinoza's work. (cf. An Investigation into the Laws of Thought, on Which are Founded the Mathematical Theories of Logic and Probabilities, Dover Publication, 1854).
[6] In this view, Universal Arithmetic does not recognise essentially negative quantities. This explains why in the second edition of the Theory of Political Economy, he adds comments on the appearance of negative values in Economic Calculus.
[7] These formula are commonly used in contemporary binary logic and also in mathematics as in integration theory and probability theory. 'a' and 'b' stand for propositions. '1-a' is the negation of the proposition 'a'; it is also written '¬a' in contemporary logic. The negation operator '¬' is defined by the truth-table:

a	0	1
¬a	1	0

'a + b' stands for proposition 'a or b' (inclusive); it is also written 'a v b' in contemporary formal logic. The '+' operator is defined by the truth-table:

b \ a	0	1
0	0	1
1	1	1

Consequently the two formula do not have the same truth-table.

In the first formula, 'a and b' can not be true at the same time, whereas it is possible in the second formula.
[8] This machine was constructed in 1869 and in 1962 it was displayed at the History of Science Museum in Oxford according to W. Mays (Mays, 1962).
[9] Cf. Jevons' own words (Jevons, 1879) and J. Passemore (1966, p.130).

10 The political argument in favour of the use of Mathematics in Economics may appear today as a response to the challenge facing Economics at a time when other social sciences began to be institutionalised and to compete with Economics (cf. chapter 3, section 1).

11 It seems that one of the main subject matter discussed by philosophers in the last two decades of the 19thC. in connection to Economic science was precisely this discrepancy (cf. *The Monist*, vol.1 to 6 and also chapter 8, section 1@4).

12 Some details on these authors can be found in Theocharis(1993) and in Theocharis. 1988. 'C. Courtois: an early contributor to cost-benefit analysis', *History of Political Economy*, 20, pp.265-273.

13 However, Von Thünen's writings as well as their translation into French occur in the bibliography on mathematical economics which Jevons (1879, Appendix I, pp.301-310) attached to the second edition of the Theory of Political Economy.

14 For a retrospective mathematical analysis of this aspect of Jevons' thought see Reid (1972).

15 The criticism could be addressed to us that the 'economic isotopy' ought to depend on 19th c. vernacular English categories instead of 20th c. categories. However, the linguistic analysis which this definition would imply is beyond the scope of this thesis. This limitation is not as great a lacuna as it may seem at first sight. The first reason is that, the economic expressions we are concerned with belong both to the 19th c. and 20th c. economic lexica since in particular, the corpus of Neo-classical economics to which this text belongs, is a general feature of 20th c. economics (cf. Chapter 2, Section 1). The second reason is that even though we do not study 19th c. economic lexica, we replace 19th c. economics into 19th c. context (cf. Chapter 3, Chapter 4).

16 The criticism could also be addressed to us that 19th c. mathematical categories ought to be used to define the 'mathematical isotopy'. The same answers hold here as that provided about the 'economic isotopy' (cf. previous note). First, the use of 19th c. categories is out of the scope of this thesis; and secondly 20th c. mathematical categories are grounded on 19th c. mathematics (cf. Joseph et al (1994)). In addition, we *believe* that, generally speaking, the reading of mathematical texts is *de facto* anachronistic since it relies significantly on the knowledge of contemporary mathematics.

17 Contrary to what is the case in the other text we analyse (cf. Chapter 6) it seems impossible to define a 'theoretical isotopy' that differs from the general semiotic function of the text.

18 Furthermore, it is considered in the History of Science that the bridging-in of connections between Geometry in particular and Physics is a defining feature of classical mechanics. This connection consisted both in considering physical space as a Euclidean space and in considering constructions and mechanical devices as providing solutions to geometrical problems (cf. Blanché, 1969; Grosholz, 1991; Molland, 1991). In addition, in Antiquity, according to Lefebre (1995) the analysis of the motions of the planets was often carried out on the pattern of the analysis of

CHAPTER 6

THE CONSOLIDATION OF MATHEMATICAL ECONOMICS: THE CASE OF DEBREU

"Rappelez-vous tout simplement qu'entre les hommes il n'existe que deux relations: la logique ou la guerre. Demandez toujours des preuves, la preuve est la politesse élémentaire qu'on se doit. Si l'on refuse, souvenez-vous, que vous êtes attaqués, et qu'on va vous faire obéir par tous les moyens. Vous serez pris par la douceur ou par le charme de n'importe quoi, vous serez passionnés par la passion d'un autre."[1]

P. Valéry, Monsier Teste quoted

from F. Rivenc, Introduction à la Logique. 1989.

"'Operations research' was born of the conditions of modern warfare; timidly undertaken in the First World War, it was widely developed in the second. The utilization of enormous means, and the manifestly economic character of the key factors of war, gave rise to the idea of a scientific treatment of military operations, which would provide commanders with the elements needed for rational decisions. It was originally in a very abstract and in large measure still speculative form that operational problems were approached. During the Great War a British officer, Lanchester, studied the advantages of the concentration of forces on a very summary mathematical model of the modern battle where a 'rate of exchange' measuring the relations of the average enemy losses appears as a unique parameter. In such an attempt one could see only the episodic and otherwise rough application of habits of scientific thought to the complex phenomena where man is involved. What is more, the on-going practice of engineers has no doubt always involved attempts to formulate problems of this type. But until the Second World War this was only a side-issue of knowledge: operation research developed and assembled a body of methods and knowledge from what had previously been only sporadic practices, stimulating the formation of specialities, learned societies, journals ... and consumers. The development of operations research certainly poses a psycho-sociological problem whose data are closely tied to technical progress and to the economic and social conditions of our time."

Granger Pensée Formelle et Sciences de l'Homme 1960, translated by Alexander Rosenberg.

INTRODUCTION

In this chapter, we are concerned with Debreu's Theory of Value from the external history perspective as well as from the semiotic perspective. In the first section a picture is given of how mathematical economics was viewed in the 1950s. This picture is based on the economic literature of that period. In the second section, definitions of 'axiomatisation', 'formalisation' and 'mathematization' are provided in a semiotic perspective in an attempt to substantiate the criticism of mathematical economic theory raised in the 1950s and still addressed today i.e. that mathematical formalism is 'empty'. The third section focuses exclusively on the Theory of Value. A genealogical presentation of its concepts and of its methods is given as well as a theoretical exegesis using semiotic concepts.

SECTION 1: POST-WORLD WAR TWO VISION OF MATHEMATICAL ECONOMICS

SECTION 1@1. ACCEPTANCE

In contrast to the marginalist revolution (cf Chapter 5, Chapter 6) and to the pre-1950s in France the activity of mathematical economics in the post-World War Two has become widespread. More economists and scientists in general are familiar with it and interested in it, than was the case in the 19th c. and at the beginning of the 20th c. Evidence of this familiarity and this interest, can be found in Allais (1949). According to Allais (1949, p.63, note 1) a study was carried out by M. Frechet in connection with the results of an international study published in the journal of the Institut International de Statistique in 1946. This international survey was conducted into the international scientific milieu. Its aim was to bring out the potential achievements, and the limitations of the use, of mathematics in the study of economic and social phenomena.

Further evidence that scientists were familiar with mathematical economics is that some mathematical theses presented at this time dealt with economic problems. This is true of André Nataf's thesis published in 1954 on the problems of aggregation in econometrics.[2] The examining committee, consisted of mathematicians, M. Frechet and A. Lichnerowicz amongst others. At the time of the marginalist revolution mathematicians were also aware of mathematical economics. However, Zylberberg (1990) shows that as far as French mathematicians are concerned, they displayed little interest in its development.

After the World War Two era, not only were mathematicians interested in the social sciences in general but social scientists were using Mathematics as well. To illustrate this statement, on June, 24th 1959, the first Stanford Symposium on mathematical methods used in the social sciences was held.[3] The sciences involved were economics, management science and psychology.

At that time, it was not clear whether economics and the social sciences had developed mathematics of their own or whether they were using the mathematics used in physics.

On the one hand, Koopmans (1957) held the view that since the mid-1930s the mathematics used in the social sciences had separated from the mathematics used in physics. He mentioned two mathematical concepts of the social sciences that have no equivalent in physics: the concept of preferences, in utility theory; and the concept of a strategic game. The development of mathematics specific to the social sciences is also what Morgenstern and Von Neumann were aiming to do (Morgenstern *et al*, 1944). On the other hand, Sebba (1953)'s report on the state of mathematical economics qualifies Koopmans' (1957) assessment. Sebba (1953) similarly pinpoints the 1920s and the 1930s as a critical period in the history of the mathematization of economics. He states that mathematization concerns a small part of economic theory but that it has generated "new thought" about economic theorising. Sebba (1953) identifies this new thought as an attempt to make models in economics on the pattern of "physical world models". In particular, he remarks that the view that economic science ought to be predictive developed together with the mathematization of economics. Predictiveness, he continues, is a feature of the assessment of knowledge in the natural sciences. One can retrospectively identify this new thought in economic literature, as in Morgenstern *et al* (1944), as well as in the historiography and the methodology of economic thought, as in Friday (1950). (cf Chapter 5, Section 2, @1; Part III).

Koopmans (1957), identifies the mathematical tools used in economics in the 1950s. He mentions diagrams, which have long been used in economics, and are still heavily used. In addition, he remarks that from the mid-1930s, the analysis of the variations of functions and linear programming as well as topology, symbolic logic and game theory have been used. At that time, mathematics was conceived both as a conceptual help to conduct reasoning as a device for clarifying and defining concepts, as in Koopmans (1957) and Morgenstern *et al* (1944), and as an empirical observational technique, as in the econometric tradition. It is not clear whether Mays (1962, p.233) is right when he remarks that: "(...)there would seem to be a tendency in modern economics to concentrate more on descriptive statistics, trying to disentangle economic tendencies without the aid of deductive models". Other authors such as Sebba (1953) consider, indeed, that the development of mathematical economics (as in econometrics) makes the difference between emprico-descriptive economics and conceptual-theoretical economics superfluous. In this connection he remarks that:

> "The new theory is driving towards full mathematization, not because of the advantages of mathematical over literary presentation, but because mathematization is the proper form of purely quantitative theory. It is driving towards predictiveness, not because of the supposed usefulness of economic prediction, but because a non-predictive quantitative science is non-verifiable. It is driving towards rigor and abstractness, not because it wants to escape logical and historical fallacies, but because rigor and abstractness are the essence of mathematical structures and the source of their explanatory power. In short, the new theory is what it is because it understands the true nature of a quantitative science and accepts what inevitably follows." (Sebba, 1953, p.263)

SECTION 1.2: ASSESSMENT

Even though mathematical economics was accepted amongst scientists by the 1950s, it was also critically assessed. Koopmans (1957, p.172) refers to: "The lively round of discussion of the role of mathematics in economics that we have witnessed in the last ten years". He makes reference to relevant discussions of this in economic journals (Koopmans, 1957, note 1, p.172). His argument is amplified by the following additional, and independent, evidence which validates his view on the reception of mathematical economics in the post-World War Two historical context.

One issue at stake in these debates is the "empirical foundation" of economic theories This is also referred to as the 'realism' of economic theories. General Equilibrium analysis was an important target in the discussion of mathematical economics as Shackle's (1958) criticism shows. He condemned this theory mainly for not being useful, in a pragmatic sense, and for leaving out the dynamic character of economic phenomena. According to Ahmed (1993), the renewal of interest in general equilibrium analysis occurred in the 1930s and 1940s, that is, prior to the 1950s discussions we are concerned with. As we shall now see many epistemological views were involved in this discussion as was the case in previous centuries in the debate about the conflict between mathematical economics and human freedom (cf. Chapter 3 , section 2 ●2, chapter 3, note 6) .

Friedman (1952) is well known for having contributed to this discussion, and his position in this debate, as many authors in the methodology of economics literature have pointed out (e.g. Caldwell, 1982) is not settled. For example, there is a contrast between his advocacy of anti-realism in the article entitled "The Methodology of Positive Economics" (Friedman, 1952) and his criticism of Oskar Lange's mathematical hypotheses. He proceeds from the grounds that Lange's hypotheses are not realistic in the article entitled "Lange on Price Flexibility and Employment: a Methodological Criticism" (Friedman, 1953a). Yet in the first article Friedman maintains the view, when he discusses the theory of the firm, that assumptions in economic theory about the motivation of economic agents could be 'unrealistic'. By this, he meant that assumed motivation could differ from actual motivation . In the second article, he argues that formalised assumptions ought to be 'realistic' in the sense of corresponding to what we have called economic 'communal sense' (cf. Cheix 1996, Section 4). On the one hand, he considers that an hypothesis, which he identifies with a theory, is a "conceptual world" so that there seems to be nothing in Friedman's view against the idea that mathematics can provide an admissible economic theory. On the other hand, he strongly criticises Lange's "abstract economic world" in the following terms: "The theory [proposed by Lange] provides formal models of imaginary worlds, not generalisation about the real world." (Friedman, 1953a).

There are two aspects in Friedman's apparently contradictory views on the realism of economic hypotheses. One is the *epistemological* view that economic 'communal sense' (cf. Cheix 1996, Section 4) supplies economic theory with both its concepts and its validation. It provides the theory with its concepts and assumption, according to Friedman in that it considers a theory to consist of a kind of Mendeleiev's nomenclature for economic phenomena. Economic communal sense also provides the theory with its validation since one of Friedman's arguments for supporting the antirealist view is that if economic agents do not use it in economic life, they won't "succeed". In this connection, Gutierrez (1966) remarks that Friedman's argument is ad hoc and that it is not a logical argument.[4]

The other aspect of Friedman's view on the cognitive status of economic hypothesis has been explored in rational expectation analysis since then, and it can be considered in two ways. Either it bears the following *meta-theoretical* epistemological interpretation: to assert a theory about human behaviour involves asserting that knowledge of the theory implies behaving according to the theory. Or the other interpretation of his view concerns *gnosiology* and *praxeology*, rather than methodology. This interpretation would be that knowledge of the means to achieve an end and, having the intention to achieve this end, implies the use of this means.

'Realism', in Sebba's (1953) conception, conveys two meanings. One can be expressed in terms of the actual use of theories in economic actions. In this connection, Sebba considers that the mathematical theory of programming such as Leontieff's, provides a more realistic view of the firm than the traditional theory of the firm because it has been actually used in international economic life.

In its second sense, realism of a theory means that it is comprehensive, and not that it is pragmatically used. In this connection, Sebba considers that game theory is more realist than other theories because it accounts for the relations of power between economic agents which other theories leave out.

As for Morgenstern *et al* (1944) and Von Neumann, it seems that in their view the "empirical foundation" of mathematical economics refers either to reliable statistical data, or, when it refers to axioms, to being in harmony with communal sense. The same idea is expressed in Shackle (1958, p.91):

"(...) our premises must bear some recognisable likeness to our impressions of the world which we wish to explain, the world which our theory is about."

Koopmans (1957) explains what he terms the "empirical void" of mathematical economics by pointing out that its lack of empirical foundation arises in part from one feature of mathematics, namely that mathematical symbols have no natural meaning. Consequently, to say that mathematical economic formalism is empirically void is just to say that it is formal.

In AER(1953) it was reported that at that time there were two answers to the lack of empirical foundations for mathematical formalism in economics. One was that logico-mathematical existence theorems provide formalism with some content.[5] The other, which we would identify today as Popper's falsification criterion, is to show with an example that the theory can be proved wrong. The debate about the realism of economic theories is still lively today (cf. chapter 2, note 11).

SECTION 2: LOGICAL STRUCTURES OF THEORIES

SECTION 2@1:.AXIOMATISATION AND FORMALISATION

In epistemological terms 'axiomatisation' applies to a sustained development of knowledge, or in equivalent descriptive terms, it applies to a piece of discourse. The defining feature of axiomatised discourse is that it contains explicit information about the logical status of the statements it uses. In other words, axiomatisation consists in compartmentalising the statements in a text. These statements may be identified as postulates, hypotheses, definitions, theorems or results. Inferential rules are those of intuitive logic, that is, they are left unspecified. The identification of the logical status of statements may be expressed using the means which grammar naturally provides or, using special statements which may be called 'meta-statements' in common language. Alternatively, it may be expressed by other signs which may be simply typographical. Axiomatisation does not necessarily imply the use of mathematical or logical formalism. However, if one has specific views on the foundations of mathematics, axiomatisation may imply the use of mathematical formalism. For example, if one considers that mathematical inference is the same as that of intuitive logic, and if one considers that intuitive logical categories are revealed in mathematical categories, then one can hold the view that axiomatisation is essentially mathematical. In particular, following this line of thought, mathematical proofs and results are valid in axiomatic systems. We shall call this kind of axiomatisation 'mathematical axiomatisation'. This view on axiomatisation is that of Debreu in the Theory of Value.

As opposed to the axiomatisation of a piece of reasoning, its formalisation necessarily requires that two different semiotic systems be involved. In axiomatisation, signs are needed to identify the logical status of statements, but it is not necessary that they form a logical system with specific inference rules. It is usually considered that formalisation consists of interpreting a piece of reasoning expressed with specific symbols governed by specific rules for constructing texts and specific inference rules, namely the formal structure. To this is applied another symbolic system, namely 'interpretation', which we shall call instead the 'object-system' to avoid confusing it with the linguistic acceptation of 'interpretation'. However, in this thesis we shall take up an even broader definition of formalisation.

Here, formalisation is regarded as the interpretation of a piece of reasoning expressed in one semiotic system by another semiotic system. With this definition, it is meaningful to regard the formal structure as defined previously as a formalisation of the object-theory and also to regard the object-theory as a formalisation of the formal structure. This unorthodox view aims at pointing out that formal structures are constructed first and foremost in an ad hoc fashion in relation to one particular object-system. In this sense, the formal structure is an interpretation of this object-system.

For example, Spinoza's Ethics provides an example of non-formal and non-mathematical axiomatised discourse, and Boole's algebraic reading of Spinoza's text (cf. Chapter 6, note 5) provides an example of a formal and mathematical analysis. The definition of axiomatisation and formalisation we adopt in this thesis is compatible with Stigum's (1990) view on William Nassau Senior's Outline of the Science of Political Economy (1836). Stigum (1990) considers that Senior's book is the first to introduce the axiomatic method in economics. At the same time, however, he points out that Senior was opposed to the idea that economics was a formal science. A third illustration of our definition of axiomatisation and formalisation is Debreu's Theory of Value. We shall identify it as a formalisation of general equilibrium analysis within the mathematical system. It goes beyond mere axiomatisation in that it uses mathematics, especially topology, as a system of inference. It does so in the sense that a topological proof is considered to provide valid economic proof of the existence of general equilibrium.

Finally, Mahieu (1989) provides a formalisation of Sraffa's axiomatised theory of value in orthodox, non-contradictory, homogenous deductive logic as well as in deontic logic.

It is worth recalling at this stage that by mathematics, we mean a particular semiotic system amongst other systems which has rules of inference and signs that do not belong to common language, amongst other systems. By rules, we do not mean 'rules of thought' as in a Bolean perspective on Algebraic Calculus, nor the logical foundation of mathematics, as in the perspective of Debreu. Instead, 'rules' are to be understood in a historical and pragmatic sense. The mathematical rules of inference we are referring to concern the pragmatic handling of mathematical symbols. We consider this handling to be regulated inasmuch as historically, one preoccupation of mathematics has been with, the definition and expression of these rules.[6]

Given this view on mathematics, we consider that mathematical formalisation is a particular case of formalisation.

SECTION 2@2: AXIOMATIC AND FORMAL EMPTINESS: GENERAL POINTS

From the outset, it is clear that neither axiomatised discourses nor formalised discourses as they have been defined previously (Chapter 7, Section 2@1) 'say something about the real world'. Instead, they 'say something about knowledge'. In semiotic terms, they are concerned with saying something about a system of expression of the continuum, rather than with saying something about this continuum itself.[7] Let us illustrate this view with the following interpretation of the Euclidean, axiomatisation of Geometry. Before the development of non-Euclidean geometries, Geometry was considered, it seems, as 'saying something' about physical space and about its properties as they were perceived visually by a normal human being. In the Elements however, Euclid's message does not concern physical space. Instead it concerns its geometrical representation. He identifies geometrical objects such as points and lines. He also identifies which properties of these objects are dependent on their being taken as representation of physical space and which properties are not dependent on this representation.

To the extent that they refer to knowledge, axiomatised and formalised discourses can be considered to be void. This is a drawback only if one considers that knowledge of knowledge, knowledge of the limitations of knowledge, and knowledge of its subjectivity, cannot contribute to "knowledge of the phenomena". What is at stake in this first kind of void is a definition of knowledge. This is why we identify it as a *gnosiological* problem.

The other kind of void we shall now identify in formalised or axiomatised discourses does not involve a theory of knowledge. Instead it concerns the idiosyncratic conditions necessary for a text to have a meaning. More specifically, it concerns the knowledge, or the cultural experience, which helps the reader for understanding a text. In this case, axiomatised discourses are said to be void in the sense that the discourse which is axiomatised is void. It also occurs when formalised discourses are said to be void, in the sense that the object-system is void. For example, a blind person may find that the geometry of Euclid is void as well as previous geometrical texts because this person is lacking the visual experience upon which this geometry is based. Similarly, equation systems used in formal general economic equilibrium analysis may seem void to an individual belonging to a community with a non-market and non-monetary economy. This is so, not so much because this person might be ignorant of the mathematics involved, but because the ideas of money, of exchange, of market, or even of an economy itself does not have an obvious counterpart in this person's cultural system.

The lack of meaning caused by the lack of idiosyncratic or cultural conditions for understanding a text can also be identified in formalised discourses at another level. For example, formal theories in mathematical logic (and this would apply to Bourbaki's system) may seem void to a person who is not used to doing mathematics. Similarly, mathematical formalisations of economic theories were considered to be void by some economists in the 19th c., and at the beginning of the 20th c., because the economists terming this view were not familiar with mathematics.

The appearance of such an idiosyncratic and cultural void is not connected to axiomatisation, nor to formalisation as such. In addition, it does not concern the nature of science as we approach it elsewhere (cf. Cheix, 1996 first and third landmarks). We consider indeed that science is a particular cultural phenomenon. We take for granted that scientific discourses have a function and a meaning which we do not challenge but which are explicit. The appearance of this void is an idiosyncratic or a micro-cultural problem rather than a cultural historical problem. It is for this reason that we shall not expand upon it further.

A third kind of void can be identified which is a distinctive feature of formalisation. Inasmuch as formalisation involves two semiotic systems with two kinds of rules of inference and two symbolic systems, formalisation may lead to statements with no meaning. Let us clear up a potential misunderstanding which may be caused by our own definition of formal system. From a popular point of view, formalisation involves a formal syntactic system and an interpretation of this system, as well as correspondence rules between the two systems, which are thought of as rules of substitution. From this perspective, these rules are conceived to be a kind of code (cf Eco, 1984, Chapter 5). However, we consider that these correspondence rules are not merely substitution rules, so far as reasonings are performed in one system or the other, and to the extent that as these systems are actually used in scientific practice. Another way to express this idea is to say that however fixed is the correspondence between the terms of the systems and the two sets of rules of inference, conducting a line of reasoning in one system does not guarantee that the interpretation in the other system of the conclusion of the reasoning is straightforward. In addition, following our subject-matter (cf. Chapter 2), we are interested in the interaction of the two systems. After this clarification we shall give historical examples of the occurrence of this kind of void in science.

One example is the crisis amongst mathematicians over the emergence of irrational numbers. It can be considered, retrospectively, as the result of attempts to construct formalisation of geometrical figures and magnitudes within the Pythagorean numerical system, or conversely to formalise the latter within the former. The correspondence rule between the two systems is broken as a result of reasoning about the diagonal of the square and the finding that the statement 'the magnitude of the diagonal' has no meaning.[8]

SECTION 2@3: AXIOMATIC AND FORMAL EMPTINESS: MATHEMATICAL ECONOMICS

We may be open to the criticism that by insisting on the third kind of void in formalisation mentioned in the previous paragraph, we miss the point of formalisation. It is indeed generally admitted that the function of formalisation is to separate the form and the content of a discourse and to ensure its consistency.

It is true that sometimes economists interpret the formal system, that is mathematical signs, in two ways. Sometimes they interpret it in a context-independent manner, which we shall call algorithmic, so as to obtain a result or a proof.[9] Sometimes economists interpret mathematical signs in a context-dependent manner, which is sometimes referred to in the literature by the phrase "economic meaning". This phrase refers to different situations, such as when mathematical signs stand for economic-theoretical notions or relations, or statistically estimated (actually or potentially), economic parameters. It also refers to the fact that their interpretation of mathematical results is constrained by methodological rules. For example, the criterion of rejection of a mathematical inequality on the grounds that it is refuted by standard statistical testing is an example of such a constraint.
Consider now the second system of signs which constitutes mathematical economics, namely vernacular language.

Economists either consider that they are interpreting it in a layman's or general common-sense manner, or they consider that they are interpreting it within the context of a particular economic theory. The seminal game theorists, for instance, consider that mathematical models are based on general common sense, natural relationships or as Morgenstern writes later on "what we believe to be common sense". (Morgenstern *et al*, 1944, p.6). By contrast, Friedman considers that the semantic support of mathematical economics is economic knowledge which could be described as a picture of the economy, so that it is theoretical and empirical. In most cases, the referential semantics of vernacular languages in economic theory is implicitly or explicitly "common sense". This sometimes refers to a kind of "intuition" that economists have. At other times it refers to something like an empirical "fact" or "evidence" which refers to everybody's presupposed experience of economic life. Finally, it sometimes refers to introspective or scientific psychology.

It is true that the historical trend towards the mathematization of economic knowledge has resulted in a disjunction of different levels of reasoning. This applies to the epistemological view of the interpretation of mathematical formalism that Debreu developed alongside his work on economic theory. Debreu (1986) considers that the mathematical form and the economic content of

economic theory can be considered separately. It seems that Debreu has adhered to the view that such a separation holds in cases where mathematical symbolism is used to represent concepts or to express relations. As the following quotation suggests however, it seems that he does not consider that this separation holds in cases where mathematical symbolism plays the algorithmic function of constructing a proof: "In *extreme cases* the proof of an economic proposition becomes so simple that it can dispense with mathematical symbols." (Debreu, 1986, p.1267, emphasis added).
The type of proof he offers to illustrate such an extreme case is a proof by contradiction and not a direct proof. Debreu also does not believe that the separation holds in the process of discovery. On the contrary, he emphasises the historical correlations between the construction of mathematical proofs and the building up of economic models. Debreu's view that the mathematical form and the economic content of mathematical economics are connected can be explained by his view on the relationship he establishes between axiomatic, mathematical formalism on the one hand, and logic on the other hand (cf. Chapter 6 , Section 2@1).

The interweaving of the economic content and the mathematical form is not solely a connection established *a posteriori* nor a connection established by economists when they reflect on their work. It can be identified in economic texts themselves. As such it is a matter for epistemology and methodology as we have defined it (Cheix 1996). Further we believe that one reason for the methodological debates opposing realist and anti-realist views on mathematical formalism is that such formalism can be alternatively or simultaneously interpreted in these two ways (cf Chapter 6 Section 1@2). We shall now illustrate this idea with examples in the history of the interpretation of numbers in economic theory.

The way some economists have used negative numbers in their theoretical reasoning suggests that the requirement that "economic meaning" should not to be divorced from mathematical computations, seems to be an important methodological feature of the *practice* of mathematical economics.

For example, according to Mahieu (1989) even Sraffa, who is committed to a formal and logical view about economic theory, takes the trouble to study the question whether he ought to admit negative numbers into his theory or not. The same question is also addressed by Tinbergen (1933a) in an article published in 1933. In this article, he is concerned with using the symbolism of the theory of oscillations in order to study trade cycles. After putting his analysis into the form of equations and solving them, he is concerned with selecting some solutions and eliminating others. Tinbergen (1933a, p.39, our translation) writes:

> "In combining solutions (...) [of a linear equation], one can get solutions that are real numbers, which is a necessary condition for a solution to have an economic meaning."

Because he is concerned with defining static demand functions, he consequently expunges the subset of mere complex solutions from the set of mathematically admissible solutions in order to get the economically admissible ones.

Economists seem to encounter problems not only in interpreting imaginary numbers, but in interpreting other kinds of numbers as well. In the second edition of the Theory of Political Economy, Jevons devotes a long paragraph (pp.137-145) to paraphrasing the utility equation of exchange in order to check the consistency of the interpretation of negative and zero values of utility functions as disutility. In this case, the definition of disutility seems to be *ad hoc* to ensure that no inconsistency occurs in the interpretation of the basic equation of exchange. For Jevons' reasoning to be consistent, one must apparently assume that two ideas are implied when he writes that a good has disutility: that its utility is negative and that it is given out in the exchange by the person to whom it is disutility.

According to Debreu (1959, note 1, p.35) who, in his theory considers that prices might be negative, the first theoreticians who used negative prices were

K.J. Arrow and T.C. Koopmans in two articles written in 1951. The first is concerned with welfare economics, and the second with allocation problems. As far as the representation of prices is concerned, Ahmed (1993) suggests that in the history of general equilibrium systems of equations, the choice of the mathematical set of possible prices and the economic meaning of prices have been connected. According to Ahmed (1993), some economists in the 1930s pointed out that the Walrasian equation system did not always have solutions. It seems that this led another economist, Schlesinger to modify the system in the following way:

> "Proofs of the theory of existence of equilibrium, by counting equations, led to the recognition that the special cases of negative or zero prices had to be barred from the equation system. This was recognized by Schlesinger(...)" (Ahmed, 1993, p.197).

> "Schlesinger's main objection against Walras and Cassel was that they did not distinguish in their models between "scarce" and "free" factors of production. They considered only the "scarce" factors. Schlesinger, in contrast, recognized that there are free inputs, like air, sunshine and sometimes water. To him, in order to have a meaningful solution to this system of equations, both free and scarce factors have to be considered. Thus, his equation system incorporated the notion that the supply of factors of production must be greater than or equal to the demand for the factors. If strict inequality holds for a particular factor, then the price of that factor is zero. If equality holds for a particular factor, then the price of the factor is positive or zero." (Ahmed, 1993, p.82).

However, in the 19th c., De Morgan (1862) had already pointed out that the signs of numbers indicates only an algebraic relation. They have only a relative meaning, and a positive number is not more "real" than a negative number. Similarly a maximisation problem can be considered as an equivalent minimization problem.

SECTION 3. AXIOMATIC AND MATHEMATICAL FORMALISM IN THE THEORY OF VALUE

SECTION 3@1: GENEALOGICAL PRESENTATION

The approach adopted by Gerard Debreu (1959) in his book the Theory of value: an Axiomatic Analysis of Economic Equilibrium can be considered to be representative of the 20thc. process of axiomatisation of the theory of general equilibrium. It was published in 1959 but, according to the author (Debreu, 1959, p.xi), its contents had been communicated earlier in lectures at the University of Chicago (from the spring of 1953 onwards), at Yale University, as well as in a doctoral thesis presented in 1956 in Paris. Debreu locates his work within the tradition of the School of Lausanne, which he associates with the figures of Walras and Pareto. The reason he gives for doing so is that he is dealing with a concept that is central to this tradition viz the concept of simultaneous equilibrium in all markets. As the subtitle of the work mentioned previously shows, Debreu is explicitly concerned with the axiomatization of the theory and expects that it will help to clarify the economic notion of equilibrium.

In the light of books dealing with the history of economic equilibrium analysis such as, for instance, those of Granger (1955, esp. Première Partie: "Un concept économique: l'Equilibre" pp.23-165), Perrot (1992) and Bruna Ingrao and Giorgio Israel's The Invisible Hand:

Economic Equilibrium in the History of Science (cf Backhouse 1994b), one might find arguments to contest the appropriateness of choosing Debreu as being representative of the axiomatisation of the theory of general equilibrium.[10] For this reason, we now present the reader with evidence that can be used to challenge our choice, from both an external and internal historical perspective.

It is true that in earlier centuries, economists had already attempted to clarify the idea of equilibrium, as we shall now see.[11] Perrot (1992) gives examples of such attempts in the French 18th c. tradition in economics. They are Turgot (1727-1781; 1759-1770 and 1753-1754) as well as the engineers Claude François Joseph d'Auxiron (1728-1778; 1766 and Achille Nicolas Isnard (?-1802; 1781). We now turn to their ideas of equilibrium . Our sources of information on these authors are the two articles by Perrot mentioned in the previous note and also Granger (1955, "L'Équilibre en Science Économique", pp.65-87).[12]

Turgot got the idea of economic equilibrium from reading of Hume's Political Discourses (1752) and Josiah Tucker's Reflections on the Expediency of a Law for The Naturalisation of Foreign Protestants (1751 and 1752). Turgot applies patterns of reasoning used in other sciences at his time to the study of the economy. In his earlier writings on this subject, the comparisons have a demonstrative power for the author because he believed the scientific laws of his time to be universal. He also believed that reality is unified, and that knowledge is produced by constructing analogies. Later, his beliefs underwent changes. Turgot gave up the idea that equilibrium had explanatory power in economics, and devoted himself to political decision-making instead of to economic science.

Subsequently, d'Auxiron and Isnard explored this comparison, which suggested to them for the first time a theoretical approach to economic phenomena in terms of equilibrium.

D'Auxiron, who adhered to a Hobbesian view of the nature of society, considered an isolated state of barter, the workings of which he described as interactions.[13]

D'Auxiron's method of theorising was as follows. He started with a simple theoretical framework, and applies to it deductive reasoning. He then studies whether the results he had obtained could be generalised, and he explored the extent to which his theoretical framework could be enhanced.

Isnard's theory dealt with "abstract" economic relations between goods in two ways. First, he did not consider that goods belonged to a particular individual; and secondly, he represented relations between goods with algebraic equations.[14] He first considered an economy in which there was no currency (i.e. a barter economy).

Within this economy, goods had relative values which were in inverse ratio to the quantities exchanged. Isnard showed how exchanges were interrelated, and put forward two views that became important to the historical development of mathematical economics. The first was that definitions of values for goods depend upon a system of equations, such that the number of equations must be the same as the number of goods. The second was that for such a system of equations one good must be continuously divisible. Then he introduces currency into his analysis, an innovation which was not used at that time. After pointing out that any good may serve as a unit for measuring the value of other goods, he defines a currency as any good which is actually used to measure the others, and which might serve as a security.

For these three authors, a theory is a rational representation of reality, the value of which is its ability to give a coherent account of apparently unrelated facts. The authors do not claim that it is out of the question for these relations be grounded in statistics, though they actually do not provide the reader with empirical verification. To conclude these remarks about the history of the concept of economic equilibrium, let us just mention that Granger (1955) provides evidence of even earlier occurrences of it in the History of Economic Thought from Aristotle onwards.

Because of their search for rigour and abstraction and because they are using alphanumeric symbols, it is right, as Perrot does, to consider these last two theories to be axiomatic endeavours. What then is the difference between Debreu and their own endeavours?

It could be said that Debreu is concerned with a central idea, that of "equilibrium", which is already theoretical, so that, in building on it, he is concerned with equilibrium analysis; whereas the pre-19th c. authors were building up, first and foremost, an economic theory, and in doing so introduced the idea of equilibrium into economics.

Even though we content ourselves with such an answer, it ought to be pointed out that it is not completely satisfactory, and that what we are faced with here is a historiographical issue. Why, for instance, could one not consider that in the 17th c. and the 18th c., the writings and the practices of the leaders of the states, of businessmen and tradesmen contained elements of economics theorising? Why not consider the fact that the idea of equilibrium was introduced even earlier by people doing accountancy? Why not consider, similarly, that Turgot, Isnard and d'Auxion got the idea of an economic equilibrium from accountants? It is not impossible that there are historical grounds for this. If Perrot is right, the first time Turgot uses the idea of equilibrium is about a flow of money from one country to another. Then, if "to be in equilibrium" a phrase used by Turgot was also a term applied to accounts in the pre-18th c. France, then one could give credence to this idea.

Because they demand rigour and order in reasoning, and because they use representations available in other disciplines in their day, such diverse authors have something in common. However, an issue that is arguable is whether these characteristics are evidence of a scientific or a mathematical method, as opposed to just *a demand for* meaning and cogency. Similarly, and if, as Bicquilley (1804) suggests, there existed a written tradition to transmit knowledge in accountancy, then it is hard to say whether it would be more natural to consider that Isnard, d'Auxiron etc..., as opposed to accountants, are contributors to economic theory.

Regarding the 20th c. the same criticism could be made of the choice of Debreu's Theory of Value as the representative of the axiomatisation of general equilibrium theory. Historical elements of an argument for this criticism can be found in the thesis by Ahmed (1993), on which we rely below unless otherwise specified.

Ahmed (1993) considers that the mathematicians and statisticians Abraham Wald (1902-1950) and John Von Neumann (1903-1957) were the first authors to use the axiomatic approach in their analysis of general equilibrium in the 1930s. By axiomatic approach Ahmed (1993) means, it seems, the historical combination of the logico-mathematical ideas developed at the Mathematical Colloquium of the University of Vienna and the epistemological ideas developed at the meetings of the Vienna Circle in the 1930s. There is historical evidence for a cross-fertilisation of these ideas, in general, since the audiences of the two meetings overlapped. For example, Karl Menger (1902) - the son of Carl Menger (1840-1921) who is the founder of the Viennese School of Economics , who organised the Mathematical Colloquium and Oskar Morgenstern (1902-1977) attended both meetings and had a particular interest in economics. In all probability this combination has influenced the development of mathematical economics since:

> "It was at Menger's Colloquium where the works of mathematical economics were presented by mathematicians and discussed by an audience of mathematicians. It appears that it was a big step forward towards an unprecedented development of mathematical formalism in economics. Many of the papers in general equilibrium theory in the 1930s were presented at Menger's Colloquium. It was Menger himself who invited the economist Schlesinger in 1931 to present a modified version of the Walras-Casselian system of equations and it was at this seminar that Wald's existence proof was presented. This may have been possible because of Menger's own interest in the problem of general equilibrium. It was also at this Colloquium that von Neumann, in 1937, presented his paper on the equilibrium of a growing economy, and also where for the first time the problem of existence was solved by applying Brouwer's fixed point theorem." (Ahmed, 1993, p.77).[15]

Ahmed (1993) then mentions the fundamental contribution of Koopmans to the axiomatisation of the analysis of the production process in the early 1950's.

Similarly, there are proofs of the existence of general equilibrium prior to that of Debreu in 1959. There is no proof of existence of general equilibrium in the works of Leon Walras (1834-1910), Vilfredo Pareto (1845-1923), Leon Cassel (1864-1944), Piero Sraffa (1898-1983), nor in those of Karl Schlesinger (1889-1938) and Hicks (1904-1989), neither according to the mathematical standard of their historical era, nor according to today's standards. However, these authors contributed to the development of the theory to a great extent. As for Walras and his followers, they considered no more than the equality of the number of equations with the number of unknown in the system, which is inadequate, as a proof of existence. The first existence proof according to contemporary standards was provided by Wald. His rigorous proof was set out at the Mathematical Colloquium already mentioned, in the 1930s.[16] The system of equations to which this proof was applied is derived from Walras in the version that Gustav Cassel (1864-1944) made popular and most importantly in the version by Schlesinger discussed in an insightful manner:

> "The first Wald proof of the existence of equilibrium required that there be a single good and no production choice. It required an assumption akin to the (aggregate) weak axiom of revealed preference." (Ahmed, 1993, p.198)

Whereas Wald is not known for having introduced a particular mathematical tool into the theory of general equilibrium and into the body of theorems , Von Neumann is known for his use of topology and for having generalised Brouwer's fixed point theorem in his proof of existence of general equilibrium.

If we analyze the genealogy of Debreu's Theory of Value, our choice might be contested, not only from a general historical perspective, but also from a biographical point of view. Before the publication of this book, Debreu collaborated with K.J. Arrow in the analysis of General Equilibrium. This collaboration resulted in the publication in 1954 of a paper, first read in 1952, at a meeting of the Econometric Society. This contained what is now termed the 'Arrow-Debreu model'. According to Ahmed (1993, p.166) this paper contains:

> "the first theorem of existence of general economic equilibrium formulated without Wald-type restrictive hypotheses (...). Together with the theorem, we have the first rigorous mathematical formalization, with considerable generality, of the fundamental concepts of the theory developed by Walras so many years before."

Although there were a number of precursors of Debreu's theory, it is possible to consider, as Ahmed (1993) does, that the method and the content of the Theory of Value differ from similar analyses of general equilibrium which preceded it. These differences are important with respect to the topics we are interested, in and with respect to the criteria we choose for selecting theoretical pieces (cf Cheix1996 ; chapter 2).

One difference is that relatively new mathematical techniques are introduced in the 1959 work which *coincide* with both the introduction by Debreu of the idea of 'excess demand' and with the proof of the existence of the general equilibrium point. The use of these mathematical techniques, namely algebra, topology and convex analysis in economics was indeed not new. According to Ahmed (1993), Von Neumann had introduced topological analysis, fixed point theorems and convex analysis into theoretical economics earlier on. But since we are interested in how concepts, proofs and tools change in connection to one another, this theory as it is perceived by historiography is an example adequate to our study.

Another difference between Debreu's theory and earlier approaches to the same subject is their epistemological foundations. Ahmed (1993) holds the view that there is a difference between Debreu's view on axiomatics and that of Wald and Morgenstern which refers to the difference between axiomatics developed by Hilbert and axiomatics as the Nicolas Bourbaki group conceived it. This difference is that:

> "The characteristic peculiar to 'Bourbakism' was that of pushing the Hilbertian

> axiomatic approach to its extreme consequences, particularly with reference to the relationship between mathematics and the empirical sciences." (Ahmed, 1993, p.171)[17].

Let us now compare the history of mathematics with their use in economics. It is explained below (Chapter 6, section 3@2) why it is hard to identify the mathematical tools that are important for the <u>Theory of Value</u>. It is consequently hard to reconstruct the genealogy of the mathematical tools of the theory. However, the development of topology, of convex analysis and that of fixed point theorems are relevant. Convex analysis developed in the 19th c. and was probably based originally on geometrical analysis. The development of topology and fixed point theorems took place later, from the end of the 19th c. to the 1950s, even though the mathematical problems they were answering had been set earlier.

So far as fixed point theorems are concerned, Herreman's (1996b) bibliography suggests that the study of fixed points in the mathematical community began in 1885 with an article by Henri Poincaré (1854-1912) entitled 'Sur les coubres défines par des équations différentielles' in the <u>Journal de Mathématiques Pures et Appliquées</u>. Then there is a gap in this bibliography until the 1910s. Brouwer published a series of articles on the subject from 1910 to 1921. It seems that by the beginning of the 1940s, the major aspects of the topic had been studied. The publication of the proof of Kakutani's theorem was

published in 1941 in an article entitled 'A Generalisation of Brouwer's Fixed Point Theorem', in the Duke Mathematical Journal. However, its significance for economics was not immediately realised.
Consequently, so far as the connection between the history of mathematics and the history of mathematical economics is concerned, it seems that the development of the mathematical tools preceded their use in economics by a time interval that we would estimate as being (approximately) up to thirty years. Border (1985) implies that in the period post-1950s, the refining of the mathematical tools, and their subsequent use in economic theory, tended to take place with a smaller time lag.

As far as the genealogy of axiomatic theories is concerned, Ahmed (1993)'s inquiry suggests not only that the axiomatisation of mathematics, of economic theory and the logical study of axiomatised systems underwent simultaneously comparable developments in the first half of the 20th c. and especially in the 1930s, but in addition, that evidence exists of historical connections between the contributors to these developments in different scientific fields and their connection to the Vienna Circle.

SECTION 3@2: DESCRIPTION

The first chapter of the Theory of value (Debreu, 1959, pp.1-27) is devoted to presenting, for the benefit of non-mathematicians, the mathematical objects, notions and results that are used later in the book. They are given without proofs but mathematical and economic examples are attached. The notions belong to set theory, topology and algebra. Debreu emphasizes the fact that such tools are uncommon in mathematical economics since differential calculus is usually used instead. (Debreu, 1959, p.x). To the first subfield (set theory) belong theorems and concepts concerning ordering, the difference between a function and a correspondence (also called a multi-valued function) and that of a set. The difference between a function and a correspondence is, according to Debreu himself, of particular importance to economic interpretation. The topological notions involved are those of: connectedness; compactedness and semi-continuity of a correspondence; and continuity of a function. The major algebraic notion involved is that of convexity. The topological and algebraic definitions and results are given only for particular spaces, namely finite Cartesian products of the real line (R^m) considered as real vector spaces, with the standard norm-sup metric topology.
As we shall now see, it is hard to list the mathematical results, either mentioned or omitted in this first chapter, that are important with regard to the role they

play in yielding the economic results, even from a mathematical point of view.[18]

This is so firstly, because the author does not report proofs and does not always refer to his sources. In order to extract the mathematical arguments on which the theory and its mathematical results are based, one ought to be able to trace back these proofs from the bibliographical references. Secondly, retrospectively and from a mathematical point of view - there is a discrepancy between the level of generality of some of the mathematical notions used in the first chapter and in proofs of the theory, on the one hand, the mathematical spaces to which they are applied (typically R^m), on the other. A consequence of this discrepancy is that it is not clear whether the proofs of the mathematical results that are important for this chapter, rely mostly on general non-metric topological properties, or whether they rely on properties of the real numbers. In other words, it is not an easy matter to identify the level of topological generality which is sufficient to derive the mathematical results of the theory. For example, because of the topological properties of R, in which the connected subparts are identified with intervals, and, further, because there is a characterisation of the compact subsets of R^n - as the closed and bounded sets, one can construct topological arguments referring only to the *form* of R-intervals, without using set-theoretic topological *concepts* in their whole generality. Similarly, since only *finite* Cartesian products of topological spaces or subspaces are considered, most of the proofs of the theorems useful for Debreu's theory generalize easily, compared to the infinite case, from R to R^m. It is true that a theorem about sets in R^m may be considered to be more important than the corresponding theorem for m=1, because it is more general. However, since frequently the *proof* about properties of R^m are straightforwardly deduced from both the particular case of R and the axiomatic definition of a finite product of sets, the particular case could just as well be considered as more important than the general. The purpose of the above remarks is to suggest the difficulty of determining "the important underlying mathematics for the theory", and of *identifying in detail* constraints imposed on the economic content of the reasoning by the mathematics deployed by Debreu. Conversely, in giving an account of the mathematics, it seems impossible to avoid adopting an anachronistic point of view and, so to speak, to "forget" the state of mathematics today (cf Chapter 1, section 4, second 'landmark').

To conclude the matter sharply, we would say that there are just two notions of mathematics that are important for Debreu's formalisation of equilibrium analysis, compactness and convexity, as opposed to results.[19] So far as both the main interpretation of the theory and the proof of existence are concerned (e.g. of equilibrium in case of the private ownership economy) the mathematical theorems which Debreu appeals to are basic indeed.

Because of these exegetical problems, we limit our task here to reporting on what Debreu considers to be important. From this perspective, there are two important mathematical results for his theory: Minkowski's theorem on separating hyperplanes; and either Brouwer's or Kakutani's theorem, on fixed points for functions or correspondences. In the latter case, it depends on which proof is more central.[20] The relevant theorems are quoted by Debreu in the following form (Debreu, 1959, pp.25-26):

Minkowski's theorem:

> Let K be a convex subset of R^m and z a point of R^m. There is a hyperplane H through z and bounding for K if and only if z is not interior to K.

Brouwer's theorem:

> If S is a non-empty, compact, convex subset of R^m, and if f is a continuous function from S to S, then f has a fixed point.

Kakutani's theorem:

> If S is a non-empty, compact, convex subset of R^m, and if φ is an upper semicontinuous correspondence from S to S such that for all $x \in S$ the set $\varphi(x)$ is convex (non-empty), then φ has a fixed point.

The axiomatic features of Debreu's theoretical developments are as following. The first is an explicit terminological segmentation that follows different lines.

The first line is that the language used to express the theory is clearly divided into terminologies which are mathematical theoretical and economic theoretical, respectively. This segmentation is made clearer by their being explained in different chapters. Both terminologies are theoretical in the sense that they result from an analysis of mathematical and economic 'realities' or, to avoid using an ontological line of reasoning, in the sense that they differ from their common usage in natural languages. This is a difference that Debreu goes out of his way to mention and he emphasizes it by using a specific typography to designate economic theory *stricto-sensus*. The second line of terminological segmentation is this opposition between theoretical and non-theoretical terminology. According to the introductory philosophical landmarks (*cf* Cheix 1996 section 4, fourth 'landmark') it is a scientific feature. One example of it is the concept of a commodity. A singular commodity is defined as a point in a space that bears a temporal dimension, so that a good considered at two different times is *formally* two different commodities.

There are limits to the first division between economics and mathematics for at least two reasons.

First, there is a correspondence between these two theoretical terminological realms. And it is of this that Debreu's axiomatization of economic theory ultimately consists. Sometimes, the economic interpretation of mathematical symbols amounts to duplicating a single mathematical object by two different economic objects. For example, a given real vector, whether numerical or algebraically written, may denote the action of an economic agent or a system of price. Sometimes the difference between two economic concepts (e.g. producer, consumer) that are instances of a more general concept (e.g. economic agent) is defined by a mathematical property (e.g. by definition of a producer, his input $\in R^-$, output $\in R^+$, and the converse convention is used for a consumer).

Sometimes the economic interpretation results in specifying mathematically the structure that is being considered. For example, individual consumption and production sets are generally contained in a subset of R^1 with a "relatively small number of dimensions" because inputs and outputs involve a small number of commodities. Similarly, it might be considered that it follows from the economic demand that an economic agent is an optimizing agent, and that the preference relation on consumption sets have particular properties. These properties allow one to define partitions with special topological properties, which in turn enable one to define a function (utility) for consumers to maximize. A third example of the influence of the economic on the choice of specific mathematics is the mathematical condition (d) (cf. pp.39-40) on the structure of the production set. This is given an economic origin both formal (in the theory, a commodity is dated) and intuitive (production takes time). A fourth example is provided by the use of correspondences. Debreu (1959, 1.8k, p.19, 1.8a p.17, 1.3h p.6) asserts that it plays a particular role on the economic side of the theory. This is true indeed, since it is mostly used to represent *possible* economic actions, which implies the idea of economic choice. Not only is the idea of a correspondence important with respect to the economic interpretation of the theory, but it is equally important from a purely mathematical point of view. It is clear from a note that Debreu (1959, note 1 p.27) considers that a function and a correspondence are two different types of mathematical objects. This is confirmed by the conceptual difference between the idea of continuity he defines for a function on R^m and the one that he defines for a correspondence on R^m. In the first case, the topology involved is metric; it is the standard norm sup topology on R^m, both on the set of variables and on the set of image-objects. In the second case, there is no topology on the images because Debreu does not consider the collection of the image-objects as a topological space. To have a homogeneous metric definition of continuity for functions and correspondences one needs to have a topology on $P(R^n)$ - the set of subsets of R^n, and a distance between R^n sets. This is not trivial. Today, Hausdorff distances and spaces might provide one way in which mathematicians would consider this problem and some other approaches may be found in game theory.[21] Still, the existence of such a distance is not obvious, since it may give birth to paradoxes concerning the power of the continuum.

Secondly, one could support the view that the economics and the mathematics of Debreu's theory are not separated because the mathematics used are chosen so as to get a proof of the key existence theorem. It is true that Debreu is concerned with eliciting the properties of an economic idea, that of equilibrium, which *was suggested to him, by the tradition of Cournot (1801-1877), Walras (1834-1910) and Pareto (1845-1923) as a mathematical problem.* Eliciting these properties does not involve a straightforward "translation" of economic ideas into mathematical concepts or symbols. It requires seeking the *deductive cogency* of equilibrium *analysis*, through a redefinition of basic economic concepts, involving a selection of adequate mathematical concepts, and the adoption of the logical rigor of mathematics. In this sense, the axiomatized theory itself, and *not just the existence of proof of the equilibrium*, can be considered as a construct that *validates* equilibrium *analysis* by identifying its hypotheses and their links with the results. In this respect, it is scientific.
One could argue, along the following lines, that these proofs may be conducted without this economic meaning, on

a pure mathematical-symbolic basis. Were it the case, then one could support either the view that the economic meaning is redundant or that the economic meaning attached to the mathematical symbols bears the particular epistemological function of giving the mathematical symbols some content; and that without some kind of content or another, no reasoning can be performed on mathematical symbols.

Let us turn now to a second axiomatic feature of Debreu's theory: the use of symbolism and formalism in the economic theory.

The symbolism involved is mathematical, by which we mean the use of alphanumerical symbols in expressions containing operators such as "Σ", "+", "$\in$", "U", "x^n" etc... The use of this symbolism is not restricted to the mathematical chapter and occurs, along with geometric diagrams, in the rest of the book. Even though Debreu uses concepts and reasonings belonging to first order logic (e.g. in his definition of the empty set), he does not use the corresponding symbolism which is used today such as the universal and existential quantifiers and which was known in 1959.[22] The reason may be that he is concerned with mathematical symbolism only inasmuch as it is a means for formalizing *economic* notions. This consists in identifying, at the end of each section, the economic-theoretical notions or hypotheses that have been explored with mathematical notions so as to build a proposition that can be mathematically treated. In a first analysis, such a proposition is formal, not because it is mathematical, but because it can be used for different "[economic] interpretations of the [economic] theory", to use Debreu's own words. This means that the economic-theoretical notions and hypotheses that are formally expressed have been thoroughly selected. Let us exemplify this comment with the input and output notions. Today, these notions are economic-theoretical notions. Arguably, in 1959 they were also, since canonical mathematical and non-mathematical studies on the subject, by Leontief and Georgescu Roegen (whom Debreu mentions in the acknowledgements) had already been published by 1937. In the latter tradition, these are formally expressed, whereas they are not in Debreu's theory (*cf* Debreu, 1959, p.30).

These notions are not in themselves formal in Debreu but are used in his formal definition of consumers and producers.

A third explicit axiomatic feature of Debreu's economic theory is that it is general.[23] What Debreu means by general is closely related to formalism as the following quotation shows.

> " It [the dichotomy between the logical side of the theory and its interpretations] also makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretations of concepts; this is

explored with mathematical notions so as to build a proposition that can be mathematically treated. In a first analysis, such a proposition is formal, not because it is mathematical, but because it can be used for different "[economic] interpretations of the [economic] theory", to use Debreu's own words. This means that the economic-theoretical notions and hypotheses that are formally expressed have been thoroughly selected. Let us exemplify this comment with the input and output notions. Today, these notions are economic-theoretical notions. Arguably, in 1959 they were also, since canonical mathematical and non-mathematical studies on the subject, by Leontief and Georgescu Roegen (whom Debreu mentions in the acknowledgements) had already been published by 1937. In the latter tradition, these are formally expressed, whereas they are not in Debreu's theory (*cf* Debreu, 1959, p.30).

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> " It [the dichotomy between the logical side of the theory and its interpretations] also makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretations of concepts; this is

repeatedly illustrated below, most strikingly perhaps by Chapter 7 on uncertainty" (Debreu, 1959, Preface, p.x).

SECTION 3@3. SEMIOTIC ANALYSIS

We shall now be concerned with describing the connection between the mathematical side and the economic side of Debreu's theory as it occurs in the mere reading of the text. For this purpose we shall use the semiotic approach defined above (chapter 2, section). We shall attempt to identify isotopies that appear in the Theory of Value and also at studying the relationships between these isotopies, with particular emphasis on the fifth chapter. This chapter, which is devoted to the study of economic equilibrium, is particularly important for the semiotic analysis for two reasons. Firstly, in the specific semiotic system of Economics (cf. chapter 2, section 2), the Theory of Value is associated with (cf. Ahmed, 1993) the proof of existence of equilibrium in terms of the concept of excess demand, which both appear in this chapter (chapter 6, section 2; section 3 @1, @2). Hence this chapter contains elements that are considered to be 'problem-solving' within the community of economists. Secondly, it is important from a general semiotic point of view, because we consider that the main role of proofs in a text is that of establishing a 'semiotic function' (cf. chapter 2 , section 2). Generally speaking, proofs strengthen the connection between the expressions of a text and their content by increasing the perceived truthfulness of the theorem which they prove. This role is even more important in axiomatized systems since to a great extent, proofs define the content both of axioms and definitions of the system.

The first isotopy we shall identify is the 'theoretical isotopy'. It is created by the redundance of the use of a particular typography, namely italics, in part of the text. It is an isotopy intended by Debreu (1959) since he explains that the theory proper is stated in italics so as to separate it altogether from other matters, including from its justification, and also from the heuristic discourse about the theory. In order to enable the reader to identify this isotopy, it is enough that he can visually recognize different typographical patterns and that he understands Debreu's meta-theoretical instructions on how to read his theory. The second isotopy is the 'notational isotopy'. We shall give an ostensive, but (for reasons of space) less than comprehensive definition of the notations displayed. The following are notations: /$\mathbb{R}^1$/; /E/; /$i = 1,...,n$/; /ω/; /$\leq$/; /$((x_i),(y_i))$/; /$\sum_1^m w_i = w$/; /θ_i/; /$Y \cap \Omega = \{0\}$/; /(x_i)/; /$z = x-y-\omega$/ ; /$\precsim_i$/. Notations are a particular subclass of the set of expressions; they contain typographical designs that do not belong to the vernacular alphabet, or else they are isolated letters with no obvious grammatical function. One can identify a notational isotopy because Debreu's use of notations is very 'regular' in the sense of being consistently applied. Not only are the same notations used throughout the text, but also a close connection is established between some of them and other expressions. For example expressions containing the letter 'x' (e.g. /X/; /X_i/; /x_i/) are associated with the concept of the consumer throughout the text, and expressions containing the letter 'y' are always associated with the concept of the producer. Consequently, this regularity results in the idea of the consumer being part of [X], [X_i], [x_i] and the idea of the producer being part of [Y], [Y_i], [y_i]. The identification of the notational isotopy does not require skills on the part of the reader other than those of reading and identifying definitions.

The third isotopy may be called the 'economic isotopy'. It is defined by the repeated utilization of expressions belonging to economic vocabulary in vernacular contemporary English language. Examples of such expressions are /consumer/, /producer/, /market/, /input/, /labor/, /exchange/, /output/, /international trade/, /economy/, /demand/, /price/, /supply/. Linguistic abilities in English are enough for recognising the economic isotopy. Whereas it might be the case that the recognition of the theoretical isotopy involves mainly the exercise of grammatical and visual faculties, the recognition of the economic isotopy requires another sort of linguistic ability, namely the recognition of linguistic categories. There is indeed an economic linguistic category in vernacular English.

The fourth isotopy shall be called the 'mathematical isotopy'. It is defined by the repeated utilization of notations and words to which Debreu deliberately attributes a mathematical content in his chapter one. In principle, the recognition of this isotopy requires no more than linguistic abilities. However, they are not of the sort required in identifying the 'economic isotopy', but of the sort required in identifying the 'notational' and the 'theoretical' isotopies. Because there is a mathematical chapter in Debreu's text, there is indeed no need to assume that there is a mathematical linguistic category as a prerequisite for recognising the mathematical isotopy.

Whereas the first two isotopies concern the level of the expression of the text, the last two concern the level of the content of the expressions.

The economics and the mathematics of the Theory of Value are not separated in the sense that the theoretical isotopy does not contain any expressions that also contribute to defining the economic isotopy. The theoretical isotopy contains expressions such as /the number of consumers is a given positive integer/, /Given an economy E, a consumption for the ith consumer (respectively. a production for the jth producer) is attainable if it is the component corresponding to him of some attainable state./ In these expressions, /consumer/, /consumption/, /production/, /producer/, /economy/ are items that define the economic isotopy.

We would argue that the criticism that the economic-theoretical isotopy would match better the intended theoretical structure of the Theory of Value than the economic isotopy we defined is not flawed. All the expressions that belong both to the theoretical isotopy and to our economic isotopy are economic theoretical expressions, that is, they are economic concepts explicitly defined by Debreu. In particular, /Input/, /output/, /international trade/, /transport/, /labor/, /exchange/, /international trade/ are some of the few expressions of the economic isotopy that do not belong to the economic theoretical isotopy. They neither belong to the theoretical isosopy. We shall now see that even though this criticism appears to be fair, the opposition of an economic theoretical isotopy to mathematical isotopy proves less fruitful than do the isotopies we defined when it comes to understanding how the correspondence between the mathematics and the economics of the theory is established.

We shall now study the correspondence between the economic content of notational expressions and their mathematical content in connection with /x-y/.
At the notational isotopy level, x-y is the compound of /-/, /x/ and /y/.

As previously noted the difference at the economic isotopy level, between /x/, /Xi/, /xi/ and /y/, /Yi/, /yi/ corresponds to the difference between [consumer] and [producer]. Both belong to 'economic agent], but the former is defined as an economic agent whose only output is labor and whose inputs are items which contribute to the physical survival of the individual (e.g.: food , clothes, housing...), whereas there is no restriction on the nature of the input and the output of the producers. There is another difference between [consumer] and [producer] at the economic isotopy level. The theoretical isotopy associates [consumer] with [demand] and [producer] with [supply]. Consequently, at the economic isotopy level, [x-y] is a comparison between the demand the supply for goods and services. This economic content of /x-y/ is confirmed by the definition of a market equilibrium as a state of an economy where x-y - w=o, with w being the resources of an economy. [x-y-w=o], therefore is a non-monetary form of the traditional economic law of supply and demand. Following this line of analysis, the law of supply and demand is part of [-] in /x-y/.

The difference between /x_i/ and /y_i/ at the economic isotopy level and at the notational isotopy level is echoed at the mathematical isotopy level thanks to the sign-convention concerning the representation of input and output (cf. supra). The sign-convention also enables us to identify a subspace of R^e containing all the x, namely the subspace where the components of x corresponding to the non-labor commodities are positive.

Consequently, at the mathematical isotopy level, the sign-convention sets the same difference between /x_i/ and /y_i/ on the one hand and between /x/ and /y/ on the other also holds for the difference between /X/ and /Y/. However, the sign-convention does not enable us to consider [x-y] as the compound of [x] and [y] at the mathematical isotopy level. We can identify [x-y] with [x-(y)] and with $\sum_i \begin{pmatrix} x_{1i} \\ \vdots \\ x_{\ell i} \end{pmatrix} + \sum_j \begin{pmatrix} -y_{1j} \\ \vdots \\ -y_{\ell j} \end{pmatrix}$ since $x = \sum_i \begin{pmatrix} x_{1i} \\ \vdots \\ x_{\ell i} \end{pmatrix}$ $y = \sum_j \begin{pmatrix} y_{1j} \\ \vdots \\ y_{\ell j} \end{pmatrix}$. This latter identification makes sign-convention meaningless. Another way of formulating this point about the sing-convention is to say that it provides a unique mathematical "representation" for an economy where producers do not produce any good - that is, they only produce different kinds of labor, and for an economy where the only economic agents are consumers. Not only does /-/ in /x-y/ make meaningless the sign-correspondence between the economic isotopy level and the mathematical isotopy level as far as [x] and [y] are concerned. In addition, one may identify [-] in /-y/ at the mathematical isotopy level as that which annihilates the difference at the notational isotopy level between /x/, /x_i/ and /Y/. Debreu's theoretical definition of /x-y/ as the 'excess demand' lends weight to this *prima facie* reading of /x-y/. It has indeed been shown that at the economic isotopy level, /demand/ belongs to [consumer] and /offer/ belongs to [producer].

NOTES TO CHAPTER 6

1 "Just simply keep in mind that only two relationships exist between human beings: either logic or war. Always insist on being provided with evidence; evidence is the basic courtesy a human being owes to another human being. If somebody declines to provide you with evidence, remember that you are being attacked and that you will be compelled to obey by fair means or foul. You will be carried away by anything gentle and entrancing, you will have a passion for somebody else's passion" (Our translation).

2 André Nataf. 1954. Thèses présentées à la Faculté des Sciences de l'Université de Paris pour obtenir le grade de Docteur ès Sciences Mathématiques. Paris: Faulté des sciences de l'Université de Paris (série A, no.2657, no d'ordre 3529). 60p. We are grateful to Bernard Bru of the Centre d'Analyse et de Mathématiques Sociales (CNRS, Paris) for mentioning this document to us and providing us with it.

3 *cf.* Arrow, Kenneth; Karlin, Samuel; Suppes, Patrick (eds). Mathematical Methods in the Social Sciences: Proceedings of the First Standford Symposium. Standford: Standford University Press (Standford Mathematical Studies in the Social Sciences, 4). 365p.

4 Friedman's argument can be symbolized as follows. If m stands for the sentence "in real economic life, the firm maximizes profit", if s stands for "in real economic life, the firm survives", and if T refers to Friedman's theory, validation rules included, then his argument is:

if (non-m => non-S) then $\vdash_T m$.

Unless this may be formalized into an inference rule of T, this influence, as Gutierrez (1966) notices, is accidental.

5 Thus, instead of discussing the role of "tâtonnement" in searching an equilibrium that one does not know, for certain, to exist, one can assert the existence of general equilibrium provided certain conditions on excess demand functions (e.g. homogeneity) are satisfied. We are grateful to Professor Reid for mentioning to us these examples. They show that one might have to make a distinction between the cognitive content and the mathematical content of mathematical formalism. In the case of discussion of "tâtonnement", the formal-mathematical definition of the equilibrium has a cognitive "economic" content (the horizon of economic actions) but not a proven mathematical content (its existence might not be proved mathematically). In the second case, it has a "full" mathematical content but because the hypothesis may be considered to be "unrealistic", one could argue that it lacks "economic" cognitive content. Further discussion of this delicate subject is beyond the scope of this thesis since it belongs to the philosophy of mathematics.

6 It is not expected that the view on the relationships between mathematics, formalisation and axiomatisation we hold will be valid for Early Indian, Greek or Chinese mathematics since, these mathematics not always use a symbolic system different from the corresponding common language, from a typographical point of view (cf our working definition of science, Cheix 1996).

7 Continuum is altogether a basic semiotic term and one whose definition raises unanswered fundamental metaphysical and

gnosiological questions. According to Eco (1984, p.81), semiotics in its current state does not yet answer these questions but ought to do so in the future. It ought also both to confront itself with contemporary gnosiological theories and contribute to them. Eco 1984) justifies using this concept still and postponing the gnosiological problems it involves by considering that semiotic analysis has up to now been more fruitful than many philosophical debates. The 'continuum' is also called 'matter' and 'world'. Eco (1984 , pp.60-61 and pp.79-81) describes it as being at the same time "what signs are saying something about and that through which they speak" (our translation). He also describes it as "amorphous matter as a whole, or similarly the universe, prior to any semiotic system" (our translation). For example, spatial relationships are part of the continuum. A sign such as a diagram is a spatial relationship and it may also express tri-dimentional spatial relationships. Because the continuum is known only in so far as it is being expressed, and because in order to express oneself (e.g. cognitive contents), one uses parts of the continuum which are already structured by culture (e.g. tongue), the question can be asked whether the cognitive content of a sign (or a behaviour) is the same as its semantic content. These issues also meet issues about the existence and the status of linguistic universals (cf. Eco, 1968, pp.390-392).

The issues which are raised in this note are not as disconnected from issues in the methodology of economics as they might seem at first sight. For example, one may use the view that cognitive contents (e.g. intentions and motivations of economic agents) are identical with semantic contents (e.g. economic theories) to justify Friedman's (1952) claim that the 'maximising agent theory is rightfully "anti-realist". Put under this light, questions can be asked such as: Is the agents' understanding of these actions more "realist" than Friedman's understanding of the actions of economic agents?; to what extent does the knowledge of theories of individual and social behaviours (and the spreading in vernacular languages of terms coming from theories in the social sciences) influences these behaviours (and the individuals' perception of himself/herself as a social agent)? These are basic epistemological and gnosiological issues in the social sciences which we have mentioned already (cf. Cheix 1996 note 12 ; Chapter 3 note 1). They also often involve praxeology. However it is out of the scope of this thesis to deal with them.

8 Such critical situations may appear when the two systems seem *a priori* close to one another, as it is the case with Sraffa's theory of value and formal logic. According to Mahieu (1989) in Saffra's system, hypothesis and deductive steps are clearly identified so that this system seems predisposed to be formalised in that way. So Mahieu (1989) attempts at expressing Sraffa's system first in standard first and order logic with no quantifiers or predicates, countable universes, and unitary or binary predicates. He shows that some of the conclusions of Sraffa's reasoning can not be obtained as interpretations of the corresponding formal reasoning. Similarly, he shows that formal reasoning leads to results that are meaningless in Sraffa's system. The same correspondence problem occurs in formalisations using quantified and deontic logic. For

example, this is the case of Mahieu (1989)'s analysis of a text by Arrow on social choice and of Sraffa's text.

9 The idea that reasoning is context-independent ought to be distinguished from the idea that it is context-free. An algorithmic reasoning is context-independent in the sense that what matters is not how it is causally or empirically performed, but that results are performed.

10 According to Perrot (1992) the idea of equilibrium in the French language belonged to aesthetics then it moved to politics at the turning point between the 17th c. and the 18th c. It then reached the psychological and economical sphere in the 18th c.. The term is used to describe either a matter of fact or norms, either imposed on something by an agent, or resulting from natural trends. It is in this latter sense that the following economists use it. Granger provides a detailed survey of the concept of equilibrium in scientific thought from the ancient Greeks onwards (Granger, 1955, "Spectre épistemologique de l'équilibre", pp.23-87).

11 (cf. especially: "Equilibre économique et déterminisme au XVIII siècle. Etude de cas." and "Premiers aspects de l'équilibre dans la pensée économique français" respectively pp.237-255 and 257-273). Reprints of original texts are included in these articles. Perrot (1992) being a reliable specialized historical source, and the history of the different versions of the books mentioned being complicated, we shall rely on his studies on the works of the 17thc. and 18thc. authors. Consequently, we refer directly to his excellent book for more information on authors such as d'Auxiron, Isnard and Turgot.

12 Further information on the history of the idea of equilibrium in economics can be found in Granger (1955)'s bibliography and Ingrao *et al*(1987, ch2 and ch3). Theocharis (1983, 1993) contain information on Isnard but not on d'Auxiron and Turgot.

13 Later, in 1826 H. Von Thünen published Der isolierte Staat in Beziehung auf Landwirtschaft und Nationalökomie, Berlin, 1st part. (Referenced in Perrot, 1992, p.263 n.15) which involves a similar kind of economic theorizing and which is referred to as an important stage in the history of mathematical economics (Stigler 1965a; Theocharis, 1993, 1983).

14 He uses symbols for goods, quantities and ratios.

15 Ahmed (1993)'s sources on this matter are Baumol and Goldfeld (1968) and Weintraub, 1983, "On the Existence of a Competitive Equilibrium: 1930-54", Journal of Economic Literature, 21, 1-39.

16 This information is confirmed in Dorfman, Robert; Samuelson, Paul A.; Solow, Robert. 1958. Linear Programming and Economic Analysis. London/Toronto/New York: MacGraw-Hill. p.527, note 4 pp.366-367. In this latter book, the publication of the proof is dated 1935 and 1936.

17 It is not completely clear what Ahmed means by this difference. However, it is likely that he refers to Debreu's view on the connection between axiomatics, intuitive logic and mathematics which we mentioned earlier (cf Chapter 6, Section 2@1). By contrast with what we termed Debreu's 'mathematical axiomatisation view', Hibert considered that an axiomatic system comprises an explicit formal system of inferences. What Ahmed (1993) might meant by 'pushing the Hilbertian axiomatic approach to its extreme consequences' is the consideration of mathematics as the formal logical system of

inferences associated with, so to speak, the object-system of the empirical sciences.

18 It is very likely that to a mathematician such an explanatory detour to demonstrate how hard it is to be definite about what is *the* important mathematics and what is not, is unnecessary. It might, however, be of some interest to non-mathematicians.

19 One can refer to Berger (1959, note 1, p.114), one of Debreu's mathematical references to be persuaded of their importance for the mathematics used by Debreu.

20 In the weak form of these theorems given by Berger (1959) in Chapter 6, Brouwer's is explicitly a corollary of Kakutani's. In the generalized form used by Debreu and expressed in Berger (1959, chapters 8 and 9), it is less clear. Deciding this question requires further mathematical investigation, which is beyond the scope of this study. Let us mention only that Border (1985) views Kakutani's theorem as the corollary of another theorem for whose proof Brouwer's theorem is used. In this case, Brouwer's theorem, rather than Kakunai's, is central.

We are grateful to Professor Reid for drawing our attention to the following historical remarks. Historically, Brouwer's theorem was discussed before Kakutani's, e.g. by Von Neumann in his growth model, and by Wald in his discussion of general equilibrium. The latter had been used in 1958 in theoretical economics by Robert Solow, Paul Samuelson and Robert Dorfman in Linear Programming and Economic Analysis, which appeared in Debreu's (1959) bibliography.

21 Let (X, d) be a metric space such that $\sup \{d(xy), x \in X \text{ and } y \in Y\} < \infty$, and let $\mathcal{F}$ be the family of all closed subsets of X. For $r>0$ and $F \in \mathcal{F}$, let $V_r(F) = \{x, \text{dist}(x,F)<r\}$ where $\text{dist}(x,F) = \inf_{y \in F} d(x,y)$. The Hausdorff distance Δ between $F_1 \in \mathcal{F}$ and $F_2 \in \mathcal{F}$ is defined by:

$$\Delta(F_1, F_2) = \inf \{r, F_1 \subset V_r(F_2) \text{ and } F_2 \subset V_r(F_1)\}.$$

$(\mathcal{F}, \Delta)$ is a metric space whose topology is not determined by the topology on X.

22 According to Marcel Guillaume (1994, p.192) these quantifiers were introduced by R.S. Peirce.

23 *A priori*, the difference between the generality of a science theory and its formal aspect is not straightforward, even though it seems reasonable to believe, on the grounds of the history of science, that formalization has *de facto* an essential role in scientific generalisation. A possible difference could be the following. From a logical point of view, both the feature of being formal and that of being general involve a relationship between expressions and contents. They seem to differ, however, in that in the case of formalism, two sign systems are involved, whereas only one is, if any, in the second case. Another way to express this difference is to say that the general-type of relation insists on an

identification process between two items, the expression and its content, and that it involves the idea of the "number" of contents of an expression, whereas the formal type of relation rather points to a *correspondence* between a system of expression and a system of the content.

SUMMARY AND CONCLUSION OF PART TWO

The second part of the thesis contributes to the 'external' history of mathematical economics and to methodological/epistemological analysis of mathematical formalism in the Neo-classical school of economic thought. The study of two seminal texts of this corpus, and the study of the historical context in which they were published, indicate that the epistemological status of mathematics in this theory changed from 1870 to 1959. In addition, these studies qualify the commonly held idea that mathematical formalism is unambiguous.

According to contemporary historiography and to historical events themselves, it is not clear whether the "marginalist revolution", by which is meant the simultaneous publication of Menger's Grundsätze der Volkswirtschaftslehre, Jevons' Theory of Political Economy and Walras' Eléments d'Économie Politique Pure is a revolution. Differential calculus, involving both marginal reasoning and the corresponding mathematical formalism, had indeed been introduced into economic analysis earlier (cf. chapter 5, section 3@1). This lack of specific features for defining the "marginalist revolution" has led historians to consider that this event is a sociological and institutional event rather than an event in the 'internal' history of economic thought. For example, Stigler (1964) suggests that mathematical economics developed from the end of the 19thC. because economists had become trained in modern mathematics. This was the case for the two authors whose work we have surveyed, Jevons and Debreu. In connection with the British context, Stigler's (1964) sociological view on the marginalist revolution can be supported by Passmore's (1966, note 8, p.550) remark. Passmore points out that after Boole's discoveries, logic and economics were taught together in British universities. Regarding the French context, the importance of sociological causes for the development of mathematical economics in general is suggested by Perrot's (1992) view that because the academic milieu was disconnected from that of economists in the 19thC., the latter were not aware of statistical techniques developed by the former, which they might have found useful. In the historical literature on the subject, mathematical formalism is described as the "language barrier" which precluded the development of mathematical economics. Similarly, it might be considered that mathematics established a "privileged access" to the scientific community of economists.[1]

Putting aside sociological views on the marginalist revolution, the results of our investigation into the external history of mathematical economics suggest that this event is a version of the foundational crisis that affected mathematics at the end of the 19thC. (cf. chapter 4, section 1). We showed that in the early 1870s, mathematical articles were published that mark the evolution that occurred in mathematics in the 19th c. and that laid the foundations of 20th c. mathematics. By the end of the 19thC., the mathematical intuition based on a physico-geometrical representation of space had given way to an intuition based on algebraic rules and on the handling of symbols. The analytical definition of the limit of a function and of a derivative contributed to modifying this intuition. Consequently, one could argue that by applying differential calculus to economic theory as a whole, Jevons echoed in economics the revolution which was taking place in mathematics at the same time. Firstly, he was strongly influenced by Boolean logic (cf. chapter 5, section 1). Secondly, he used the same mathematical symbolism as that which was used by mathematicians who were prominent figures of this revolution. Thirdly, Jevons gave an analytical definition of the derivative of a function. Fourthly, he made use of differential coefficients in a way that shows, retrospectively, that he considered them as algebraic operators (chapter 5, section 3@1). However, this view on the connection between the history of mathematics and the history of mathematical economics needs qualifying since Jevons' use of the "new" differential calculus is limited and it has other origins than theoretical mathematics alone. In addition, even though his views on the use of mathematics (especially statistics) are modern to a certain extent (cf. chapter 5, section 2), his use often remained tied to the "old" Euclidean geometrical tradition in mathematics.

Jevons' use of the "new" differential calculus is limited since it was shown that Jevons did not use differential calculus evenly in the various parts of his theory. In particular, he did not use differential *formalism* (cf. chapter 6, section 2@1)[2] often: he used differential coefficients as algebraic operators in the Theory of Utility only; and in spite of his claims, he used the integration operation but only in connection with the computation of the rate of interests. On this latter ground, the view can be supported that Jevons' mathematisation also had an empirical/practical origin. This does not conflict with our results, namely that some evidence suggests that generally speaking, mathematical formalism used in economics has empirical and practical origins (cf. chapter 3, section 2@2). Another well known origin of Jevons' use of the formalism of differential calculus is theoretical physics (cf. chapter 5, section 3@2). When Jevons does not consider differential coefficients as algebraic operators, he uses the corresponding *symbolism* as a means of extending reasoning involving originally finite quantities to reasoning on infinite quantities. In this connection, it will be suggested below (cf. General Conclusion), that the use of mathematical symbols allowed Jevons to avoid considering in details any epistemological problems arising from such an extension of his reasoning.

Features of the Euclidean geometrical intuition remain in the Theory of Political Economy firstly in that it contains direct reference to Euclid. In addition, it can be argued that both Jevons' claim that literary economics already contains mathematical reasoning and the deliberate lack of mathematical proof with symbols in his treatise (cf. chapter 6, section 1@2) are typical of the Greek tradition in mathematics.[3] Gardies (1993) considers indeed that there are two characteristic features of Greek mathematics compared with other traditions. One feature is that it did not use many symbols or ideograms. The second is that not only were mathematical proofs expressed in vernacular language, but the inferential steps of reasoning relied on (and followed) the phonetic structure of the written discourse expressing the proof. By contrast, symbolic algebraic proofs also rely on the visual and typographic aspects of writing.

On these grounds, one can summarize Jevons' contribution to mathematical economics as follows and in doing so contribute to a non-sociological definition of the marginalist revolution. Retrospectively, Jevons may be characterised as having developed mathematics as a conceptual tool: he applied the *symbolism* of differential calculus so as to unify at the same time reasoning in different subparts of economic theory into a unique "marginal reasoning" drawing together mathematical formalisms with different origins. In addition, he argued for the use of statistics as an empirical tool for heuristic purposes, as well as for validating the theory.

The evolution of neo-classical mathematical economic theory from Jevons' Theory of Political Economy to Debreu's Theory of Value concerns both the theory itself and how it was generally perceived. The *use* of mathematics in science, and in economics in particular, had become more widespread in the 1950s than was the case in the second half of the 19thC. It was also less controversial (cf. chapter 6, section 1@1). However, controversies concerned the *assessment* of mathematical economics, and especially the role of economic "reality" in assessing theories (cf. chapter 6, section 1@2, chapter 6, section 2@3). It was considered that knowledge of economic "reality" was provided by econometrics, which developed in the first half of the 20thC. (cf. Morgan, 1990), or that "reality" referred to logico-mathematical possibility. Alternatively it was considered that "reality" referred to pragmatic and political decision-making. For his part, Debreu was concerned first and foremost with the logico-mathematical cogency of General Equilibrium theory. He achieved and proved this cogency using metric topology and axiomatics (cf. chapter 6, section 3@2). These tools enabled Debreu to provide a non-constructive proof of the existence of a General Equilibrium under certain specific conditions.

From a biographical standpoint, the developments in logic by the Bourbaki group and by the Vienna Circle had an influence on Debreu (cf. chapter 6, section 3@1;

chapter 6, section 2@1). Contrary to Jevons however, Debreu considered that logic departed essentially from communal sense and vernacular language. This provides an explanation for their giving different status to symbolic-mathematical proofs in their theories. Debreu considers that they are essential for the logical validation of the theory. Jevons does not and considers that logical arguments are valid only if expressed in vernacular language. In addition, from an 'internal' historical perspective, Debreu's contribution to Equilibrium Analysis is identified with this application of logic, whereas Jevons' contribution is identified with the application of differential calculus.

NOTES TO THE SUMMARY AND CONCLUSION OF PART TWO

1 From this perspective, mathematics bears the same socio-linguistic status as Latin did in the French society of Descartes' time. At that time, literate publications were mostly written in Latin. Descartes often used vernacular French instead.

2 Briefly, mathematical *symbolism* refers to the use of typographic symbols commonly used in mathematics, whereas mathematical *formalism* refers to the use of both mathematical *symbolism* and rules for handling symbols.

3 However, Jevons admits mathematical proofs in the sense of statistical proofs (cf. chapter 5, section 2).

PART THREE

MATHEMATICAL ECONOMIC MODELS

INTRODUCTION TO PART THREE

This third part deals with the second topic of the thesis, namely mathematical economic models. It intends to show that, even though epistemologists and methodologists agree with the view that mathematical models are an essential feature of the production of contemporary economic theories, it is difficult to identify precisely in which way they contribute to science. In order to bring these points into light, we shall attempt at defining mathematical economic models from different stanpoints.

We shall first give an ostensive definition of what is called today mathematical economic models, and also of applied models (chapter 7). This is for the purpose of clarity: when the views and the remarks held in this thesis differ from the views of other methodologists, this definition enables the reader to identify whether it is a difference in interpreting and understanding scientific objects or whether it is a difference in the range of objects that are been considered.

Then we are concerned with the historical origin of the term model in economic literature (Chapter 8). The bibliographical starting point of this inquiry is an unpublished article by Martin Zerner (1993). The first section displays the results of a backwards chronological inquiry into the origins of the term. The methodology and the scope of the second section differ from those of the first, even though they both have the same subject-matter. The second section is not focused on the chronology but on the institutional context. We identify 'terminological sources' i.e. individuals or institutions a) that were amongst the early users of the term in economics, and b)that are recognised now as having played a primal role in the circulation of the term within the community of economists. It goes without saying that because of its very nature this chapter has no claim to be comprehensive.

The next step in the apprehension of mathematical economic models consists in extending the scope of the previous analysis (Chapter 7, Chapter 8) in two ways: to non-mathematical models and to non economic models (Chapter 9). This is justified first by the methodology of the thesis which is concerned with external history. Secondly, it is justified by the nature of the available methodological and epistemological literature about scientific models. Most authors approach models either from a perpective concerned with science as a whole or with physics in particular, but not with economics specifically. In this respect, Hausman (1992), Van Parijs (1990) and Granger (1955) seem to be exceptions since they are strictly concerned with economics. Thirdly, the extension of the scope of the study to other sciences is justified by three features of contemporary economics. The first feature is that one possible origin of the use of the term in economics is its use in mathematical physics (cf.

Chapter8). The second is the belief held by some economists, both in the past and at present, that economics is the paradigm of social sciences. Consequently, as Van Parijs(1990) notices, the study of the methods used in economics is *de facto* a study in the methodology of other social sciences. The third feature is the view held by some economists that their mathematical structure is not essential to mathematical economic models. The first section is concerned with the question whether models are a defining feature of 20th c science in general, or whether they are specific to economics. The second section is devoted to analysing what models reveal of the epistemological structure of economic science.

CHAPTER 7

OSTENSIVE DEFINITION

"Du remède à la confusion qui naît dans nos pensées et dans nos discours de la confusion des mots; ou il est parlé de la nécessité et de l'utilité de définir les noms dout on se sert, et de la différence de la définition des choses d'avec la définition des noms."[1]

Antoine Arnauld, Pierre Nicole, La Logique ou l'Art de Penser, I, 12 (1662).

SECTION 1: OSTENSIVE DEFINITION

The argument below refers to items that are *named* mathematical economic models today, either in economic textbooks or in specialised economic literature. The motive for the selection of these models is to exemplify the fact that they appear in most subparts of economics regardless of the principles adopted for subdividing the discipline.[2]

A widely held view is that the use of mathematical models is a characteristic of neo-classical economics. However, they are not specific to a particular school of economic thought. Models derived from the Debreu-Arrow model are usually referred to as being examples of the 'microeconomic neo-classical equilibrium model' (cf. Guerrien, 1992) The equilibria concerned may be general or partial equilibria, the economic environment concerned is either certain or uncertain and the "competition" is either perfect or imperfect. The mathematical structure related to this object is the finite dimensional real vector space with its dual space. It is represented either with algebraic symbolism or geometrically. The mathematics in use are general set theory (cf. Debreu, 1986) differential calculus and topology, linear algebra (convex set analysis included), measure theory and non standard analysis (cf. Debreu 1986). Probabilities are also used in these models in the neo-classical vein. For example, this is the case for the asset pricing models in financial economics Debreu terms these 'theories', 'models' in Guerrien's (1992) terminology. He considers that they are 'true' instances or interpretations of mathematical formal structures. In this respect, they are not only models in the popular sense of representations of the economy, but also in a sense similar to that used in the corpus of mathematical logic. The mathematics is considered to be the form of the models. This form allows characteristic features of the models to be deductively obtained with mathematical arguments and symbolism. In this case, it is considered that mathematics validates the model by endowing its statements with necessity. There are some cases where it is considered that the necessity of statements is independent of the symbolic system (vernacular language/mathematical symbolism) in which these statement are argued for. One such case is a proof by contradiction mentioned by Debreu (1986). There are applications of these models such as computable general equilibrium models which are used in international economic institutions (cf. Debreu, 1986).

There are examples of mathematical economic models in the Western Marxian school of thought as well.[4] There are first Marxian microeconomic models.[5] Some of them have been built recently by American authors such as John Roemer (cf. Guerrien, 1992). Roemer's model deals with the determination of prices. Just as the neo-classical model deduces the characteristics of a state of equilibrium from a price system, Roemer similarly deduces class exploitation. In this sense, his model is explanatory. In addition, there are Marxian macroeconomic models. One example is Farjoun *et al* (1983)'s analysis. The authors refer to this analysis as 'mathematical modelling' and label what they are building as a 'probabilistic model'. They are using both probabilistic and statistical analysis to reframe the basic theoretical concepts of political economy expressed in input-output models. Basically, their approach consists in turning economic laws into probabilistic distribution laws.

The assessment of whether the mathematization of the propositions of specific schools of thought alters these propositions is an important and unsettled issue for the methodology of economics . On the one hand it is striking that in the two Marxian examples, the mathematization of the theory is on a par with its passing over schools of thought barriers. In this connection, Guerrien (1992) is doubtful about Roemer's analysis being Marxian at all, and Farjoun *et al* (1983) themselves point out that the results of their model are of relevance for economics in general. On the other hand, Guerrien writes that the mathematization of neo-classical theory has a merely pedagogical role and that it does not have the role of generalizing the theory. Similarly, authors differ in assessing whether the mathematics is an essential feature of the model or not. Guerrien (1992) holds the view that it is not, whereas Debreu (1959, 1986, 1991) holds the view that it is.

We have just illustrated the point that mathematical economic models are not specific to a particular School of Economic Thought. Let us now indicate that they are not specific to other sub-divisions of economic knowledge, such as the level of abstraction of a theory, theoretical knowledge as opposed to practical knowledge, large scale theories or small scale theories.

In abstract economic theory, apart from the Debreu-Arrow model already mentioned, there are also game theoretical models. The latter compete with the former, first, because they have the same theoretical status, the same level of generality and also the same axiomatic form. Second, they are both concerned with equilibria.[6] Important contributors to game-theoretical models are John von Neumann (1903-1957), Oskar Morgenstern (1902-1977) who contributed to the analysis of co-operative games in particular, and John F. Nash who suggested considering co-operative games as special case of non co-operative games. In the same way as there are less abstract models of the Debreu-Arrow general equilibrium model involving specific production and utility functions, there are also less abstract models of, say, probabilistic one player games, with a specific probability distribution.

According to Franklin (1993, pp.517-518), there are applied models of axiomatic game theoretical ones, just as there are applied models of the general equilibrium model. For example, Schwartz (1978) refers to the Wharton School's model of 1967, which has been applied 'to give account of' statistical data for the US economy collected between 1948 and 1964. In particular, this model identifies recession patterns as sets of sub-optimal equilibria.

Similarly, models are not scale-specific: there are general models of the economy and also models for specific markets such as financial markets. Consequently, the size of the empirical extension of a model in itself does not justify its being called a 'model'.

The mathematical economic models that have some connection with the economy are not scale-specific in a geographical or thematic sense (a particular region, the market for a specific commodity...).

Finally, the diversity of examples already mentioned is sufficient to show that mathematical economic models are not using a unique mathematical technique. The use of some branches of mathematics, such as differential calculus or linear algebra, does not seem to be controversial. The use of others, such as probability calculus, is. This was true when Haavelmo (1944) published one of the earliest econometric models, and both Farjoun *et al* (1983) and Davidson (1991) suggest that it is still true today.[7]

It is also hard to identify methodological and epistemological features of mathematical economic models so as to define the difference between models and, say, theories, hypothesis or conjectures. Even within the sub-group of models called 'error-correcting' models, which, to use Morgan's (1990, pp.192-258) terms, aim to bridge the "data-theory gap", models have different functions. For example, some of them estimate the bias of statistical computational methods such as regression analysis, resulting from these computations being performed on one particular set of data. We consider that these models are part and parcel of statistical analysis and it may be considered that their function is merely methodological. Others aim at accounting for inconsistencies between the theory and statistical data, such as "errors-in-variables" models (cf. Morgan, 1990). These models have an epistemological function since in order both to identify a "data-theory gap" and to assess what does explain it and what does not, one must have an opinion about the cognitive status of a theory. It is difficult to identify methodological and epistemological features defining models first because given a particular model, it may be difficult to identify its cognitive status. For example Boland *et al* (1986) criticise the Debreu-Arrow model on the grounds that few economics articles which are concerned with empirical inquiry make use of it. This criticism might be misleading since Debreu, at least in the Theory of Value is concerned with the logical cogency of an analytical framework rather than with its explanatory power. Second, it is also difficult because the same models may play different roles. In addition, as Morton (1990, pp.257-261; 1993, esp. 671-673) and Hausman (1992, pp.70-82) notice, 'model' and 'theory' often stand for one another in economic literature.

SECTION 2: COMMENTS

In the previous section, the multi-dimensional diversity of mathematical economic models has been brought into light. We shall now make further comments on the particular aspect of this diversity, namely on applied mathematical models. After Israel (1996) and Franklin (1993), whose scopes of analysis are wider than ours, we shall point out the ambiguity of the phrase 'applied mathematics' as applied to mathematical economics in particular. Then the claim that the conceptual cluster mathematics/formal is not ideal to account for contemporary mathematical economics is explained (cf. chapter 2, Section 2).

There are at least three ways in which mathematical economic models are said to be applied. In the first accepted use of the phrase, mathematical models are applied because they are specific to a particular economic reality (a particular nation, a particular market, a particular sociological group...). This accepted use encompasses Bergstrom's(1967) definition of a "model" as follows: "Any set of assumptions that approximately describes an economy or a sector of an economy can be called an economic model", and also applies to models constructed to account for time-series. In this first accepted use, the function of the models is to reach something that is relevant to a *particular* economic situation, by contrast with something relevant to the economic phenomena in general. Let us give further examples. Guitton's (in [5], "Science Economique") definition of positive econometric models is a straightforward example of this first accepted use of "application". He draws a distinction between models in positive econometrics ("économétrie positive") and models in rational econometrics "économétrie rationnelle"). Positive models are generally linear or non linear sets of equations with parameters estimated from statistical data. They differ from rational models, whose "statistics are derived from understanding as opposed to reality" (our translation). Examples we can give of "positive models" are the models used by civil servants described by Tinbergen (1967) after having been laden with national statistical data. There are 'general rational models', that correspond to these applied models namely those expounded by Tinbergen (1967)which are not nation-specific. However, there being a correspondence between an applied model and a more general epistemological framework is not a defining feature of 'applied' models in this sense.

The second sense in which mathematical economic models are said to be applied is when they are used for specific practical purposes. By practical purposes, we mean actions *purporting* to transform the outside world and to physically organise it. The realms of such applied models are: national - governmental policy-making; and conjectural analysis for individual firms, planning, accountancy, trade. 'Applied' models in the second sense may be 'applied' in the first sense as well, but this is not necessary. For example, it may be a quality for a model aiming at modifying consumers' behaviour to be consistent with people's representation of the economy instead of with statistical data on consumption. Conversely an applied model in the first sense may not be 'applied' in the second sense, that is practically used. In this case, applied is more or less opposed to 'theoretical' or 'causationless' models.

A third accepted use occurs, amongst other things, when one is considering the history of model-building. A model, whether abstract or not, might have been built for a particular phenomenon and may later be 'applied' to something else, that had not been thought of beforehand. This something 'else' might be a different discipline, a different subfield of the same discipline as the original, a different phenomenon, within the same discipline or not, or a different historical era. An example of such an accepted use can be found in the material explored by Hausman (1992, pp71-72). For example, MacKay's scoring model for multiathlon sporting events is an applied version of Arrow's model.[8] MacKay interprets Arrow's axioms for economic individual preference orderings as axioms for scoring athletes in a competition. Another example of the same accepted use occurs when one speaks about mathematical economics as applied mathematics in the following manner. Mathematics that is, mathematical theorems and definitions rather than mathematical signs alone (i.e. mathematical formalism rather than mathematical symbolism) provides ready-made logical tools that are either interpreted in economic terms or used as computing devices in economic discourse.

One could argue that our example about consumers' behaviour (cf. definition of the second accepted use of 'applied') does not truly illustrate the difference between particular 'applied' models and practical 'applied' models. This remark is even more accurate if addressed to models of behaviour in financial markets. They are 'applied' (practical) models used for economic decision-making. In addition it is often considered that the agents' representations of the market are inserted in the models in the guise of anticipation variables or probability distributions. It is true that these two senses of 'applied' both refer, to a certain extent, to what Israel (1996, note 1, p.207) calls the vernacular accepted use of 'applied mathematics', namely the use of mathematics in empirical contexts. This use of 'applied mathematics' is vernacular, according to Israel (1996) because its appearance is not an important event in the history of science. However, these two first sense of applied are different from a philosophical point of view because the first refers to epistemology and the second to praxeology. We shall also retain the first sense of 'applied' (particular) mathematical models as a way to draw attention to an aspect of the use of mathematics in contemporary science.

Assuming that what we are surveying at present is a *scientific* practice, then to appleal to the existence of applied (particular) *mathematical* models may appear to be debunking the widespread Aristotelian idea that science deals with generality. In addition, as it was noted above (Chapter 2, Section 2) 'mathematics' usually connotes 'formal', which often connotes 'generality' as opposed to 'the particular' as well as a 'gap' between 'the formal' and 'the particular'. Franklin's (1993) study as well as those of Israel (1996), Morgan (1990) and Morton (1990) indicate concerns that mathematical models do not entirely conform to these connotations. For example, Israel (1996) holds the view that a defining feature of 20th c. science is that mathematics are used to study *particular* aspects of phenomena as opposed to aspects they share with one another, such as their conformity to general laws. Franklin (1993) who is a mathematician, attempts to draw the attention of philosophers to the aspect of the 'Formal Sciences' (including game theory, control theroy, network analysis etc).[9] He holds the views, first, that these sciences cannot be described as an 'idealisation from' or as 'applications to' reality. He also holds the view that in these sciences there is often no 'difference' between the mathematical necessity of the theories and the practical certainty of observations. Morton (1990) considers that the role of mathematical models in contemporary science is to bridge the 'gap' between general scientific theories, and particular observations. Morgan (1990) shows that this is true so far as econometrics is concerned. Morton (1990) also holds the view that this gap-filling role of models has similarities with the role of folk psychology. Hence he suggests that this role might be psychological rather than scientific.

The reader might rightly consider that given the evident relevance of the above remarks to the subject-matter of this thesis, they could be explored further. They are indeed the concern of an epistemology which is connected to the approach embraced in this thesis, but are still out of its scope . Nevertheless it is hoped that these remarks are of assistance in showing how epistemological questions emerge out of the descriptive study of scientific methods or techniques.

NOTES TO CHAPTER 7

[1] "On how to remedy the lack of sharpness in our thoughts that is caused by the ambiguity of words; it is discussed whether it is necessary and useful to define the names which are used, and the definition of a thing is contrasted with the definition of a term." (Our translation).

[2] We are all grateful to Professor Reid for drawing our attention to the fact that one subpart of economics, namely institutional economics generally eschews mathematics.

[3] We are very grateful to Professor Reid for mentioning to us this example.

[4] Because we are interested in the relationship between mathematical tools and economic content in economic theory, it is a matter of interest to us to investigate the Marxist East European mathematical economics in order to compare it to the Western Marxian tradition and to the neo-classical tradition. Both mathematics and economics are important academic subjects in the former Soviet countries, but the economy in which they developed and to which they apply is not a market economy. However, this investigation is beyond the scope of this thesis. We are grateful to Professor Reid for mentioning to us the following references related to the analytical treatment of Marxian economics: the French economists Benassy and Bonnetti, the Italian Pasinetti and the German Scheffold, the Russian Dimitriev and also Sraffa.

[5] Hausman (1992 p.49 note 59) mentions the following authors: H. Morishima, Marx's Economics: A Dual Theory of Value and Growth, (Cambridge: C.U.P. 1973); I. Steedman and P. Sweezy (eds), The Value Controversy (London: New Left Books 1981); J. Roemer, Analytical Foundations of Marxian Economics (Cambridge: C.U.P. 1981)

[6] It could be argued that these two analyses do not entirely compete since Arrow *et al* (1952, pp.272-274), hold the view that games with finitely many players are a special case of their 'abstract economy'. They complete however, in the sense that historically, Arrow-Debreu models have been concerned with defining a uniform concept of equilibrium and with explaining the derivation of empirical data from equilibrium points (cf.

Benetti, 1995), whereas game theoretical frameworks are rather concerned with defining different concepts of equilibrium so as to have a taxonomy of various empirical situations.

[7] In this connection, it is worth noting that the use of probability calculus in economic theory in the 20th c. succeeds the mathematical axiomatisation of the theory provided by Kolmogorov from the late 1920s to the early 1930s, and precedes the English publication in 1950 of his Foundations of the Theory of Probabilities.

[8] MacKay A. 1980, Arrow's Theorem: The Paradox of Social Choice. A Case Study in the Philosophy of Economics, New Heaven: Yale University Press, referenced in Hausman (1992).

[9] By 'Formal Sciences', Franklin (1993) means operation research control theory, the body of techniques used to find emerging patterns in large sets of statistical data (e.g. descriptive statistics, signal processing), network analysis, tame theory, theoretical computer science, artificial intelligence, statistical mechanics, fluid mechanics. We are grateful to Adam Morton of the Philosophy Department of the University of Bristol (UK) for drawing our attention to this article.

CHAPTER 8

HISTORICAL DEFINITION

"Folly speaks: [...] What difference is there, do you think, between those in Plato's cave who can only marvel at the shadows and images of various objects, provided they are content and don't know what they miss, and the philosopher who has emerged from the cave and sees the real things?"

Erasmus, Praise of Folly, XLV (written between 1509 and 1522).

SECTION 1: CHRONOLOGY OF THE ORIGINS IN ECONOMIC LITERATURE[1]

SECTION 1@1: 1936

According to Zerner (1993), the term 'model' first occurs in economic articles' titles in 1936 in an isolated context.

This remark is confirmed by our survey of tables of contents as well as a reading of articles of the first four volumes of the journal Econometrica.[2] 'Model' first appears in an English article by Victor Edelberg entitled "An Econometric Model of Production and Distribution" (Edelberg, 1936)[3]. In the core of the text, the word is scarcely used and it is used to assess the author's input-output theory of production. It designates an equation entailed from others. According to Edelberg, this theory is statistically useful but is not a good model for production.

The occurrence of the word Zerner reports next is also dated 1936. It appears in a presentation by Tinbergen to the Vereenigung voor Staathuishondkunde. From the analysis of Zerner's references, the term seems to be Dutch. In 1937, Tinbergen defines it in the following way:

> "Before enumerating the equations adopted to describe Dutch economic life, I must stress the necessity for simplification.

> Mathematical treatment is a powerful tool; it is, however, only applicable if the number of elements in the system is not too large. Subjects, commodities and markets have, therefore, to be combined in large groups, the whole community has to be schematized to a 'model' before anything fruitful can be done. This process of schematization is, of course, more or less arbitrary. It could, of course, be done in a way other than has been here attempted. In a sense this is the 'art' of economic research, depending partly on the attitude in which the approach is made."[4]

The word does not appear in an article by Tinbergen published in 1933 (Tinbergen, 1933a). For the present *terminological* survey and it is important to know whether this article is a translation or not. This article, published in French, contains anglicisms such as '*component* réél', '*component* imaginaire' (emphases added). It could therefore by a translation of an earlier article written in English or Danish which could contain the word 'model'. However, it is likely that it is not a translation since according to Alain Desrosieres to whom personally we are grateful for this piece of information, Tinbergen was fluent in French (cf. also Desrosieres, 1994, p.3 note 1)[5]. In addition, according to Desrosieres (1994, p.19), Tinbergen wrote a book in French in 1938 entitled Les Fondements Mathématiques de la Stabilisation du Mouvement des Affaires, which strongly suggests that he was able to write an article in French in 1937.

After having reviewed the author's works, Zerner remarks that 'model', in Tinbergen's terminology, is restricted as a rule to cases where the coefficients of the equations are statistically determinated. Not only is his use specific but also it is scarce. The economic context, identified by Desrosieres (1994), is that of Dutch macroeconomic policy which aimed to relieve the 1930s Depression. Our inquiry in Chapter 8, Section 2 partly confirms Zerner's view.

SECTION 1@ 2: 1937

Thanks to Morgan (1990, p.81) we notice the next occurrence of the word in an English article by Eugen Slutsky in 1937. This article is a revision by Slutsky of the English translation by Eugene Prostov of a 1927 article Slutsky wrote in Russian on business cycles.[6]

> "Any concrete instance of an experimentally obtained chance series we shall regard as a *model* of empirical processes which are structurally similar to it. As the basis of the present investigation we take three models of purely random series and call them the first, second, and third basic series. These series are based on the results obtained by the People's Commissariate of Finance in drawing the numbers of a government lottery loan. For the first basic series, we used the last digits of the numbers drawn; for the second basic series, we substituted 0 for each even digit and 1 for each odd digit; the third basic series was obtained in the same way as the second, but from another set of numbers drawn." (Slutsky, 1937, p.108).

At first sight, it seems that Slutsky is concerned with finding a well defined mathematical function which matches statistical data on business cycles, as Tinbergen (1933a) does in the tradition of Econometrics. We suggest that, in addition, and in the second section of the article in particular (pp.107-114), Slutsky is addressing not only a *technical* economic problem, but also the general *epistemological* status of mathematical accounts of random events, necessary events and events produced by a known or unknown cause. What supports this suggestion is that it is striking that in this section, (esp. pp.107-108; note 7 p.108; note 14 p.114), Slutsky is extremely careful about the terms he employs and about their origin.[7] Slutsky is concerned with showing that a simple mathematical treatment of what he calls an "incoherent" or "random" chance series generates a "coherent" series. By "incoherent" series, he means series with no connection between the terms of the series, and by "coherent" series, he means series with a connection between the terms. Slutsky implicitly asserts that mathematical computation first may provide unstructured (random) series with a structure and secondly, that it provides a way of analysing different empirical events within the same symbolic framework.

The concept of model used in this article is not totally clear even though it is well defined. Whilst the aim of the article is clearly to show that the mathematical treatment of random processes can generate mathematically regular patterns, and despite Slutsky's attempt to develop a clear terminology, some details of this article remain obscure. For example, the random series on which the whole article relies is based on "the results obtained by the People's Commissariate of Finance drawing the numbers of a government lottery loan"

(Slutsky, 1937, p.108). There is no detail on how this series has been drawn. Most certainly, these numbers were drawn at random, but it might also be that the Commissariate of Finance used a mathematical computation to draw these numbers, so that this series might be "random" only in a limited sense.

SECTION 1@3: 1944

Thanks to Morgan (1990, p.245) and Zerner (1993, p.5) we notice the next occurrence in Trygve Haavelmo (1911-). In 1944 he defines a model as an *a priori* form of knowledge:

> "Theoretical models are necessary tools in an attempt to understand and 'explain' events in real life. In fact even a simple description and classification of real phenomena would probably not be possible or feasible without viewing the reality through the framework of some scheme conceived a priori." (Haavelmo, 1944, p.1).

they:

> "will have an economic meaning only when associated with a design of actual experiments that describes - and indicates how to measure - a system of 'true' variables (or objects) X_1, X_2, ..., X_n that are to be identified with the corresponding variables in the theory...
>
> The model thereby becomes *an a priori hypothesis* about real phenomena, stating that every system of values that we might observe of the 'true' variables will be one that belongs to the set of value-systems that is admissible within the model. The idea behind this is, one could say, that Nature has a way of selecting joint value-systems of the 'true' variables such that these systems are as if the selection had been made by the rule defining our theoretical model. Hypotheses in the above sense are thus the joint implications - and the only testable implications, as far as *observations* are concerned - of a theory *and* a design of experiments." (Haavelmo, 1944, p.8).

According to Zerner, 1944 is a landmark

in the history of the word. Von Neumann and Morgenstern's Theory of Games and Economic Behaviour, dated 1944, originated contemporary usage of the term.

> "At this stage, the reader will observe a great similarity with the everyday concept of games. We think that this similarity is very essential; indeed that it is more than that. For economic and social problems the games fulfill - or should fulfill - the same function that various geometrico-mathematical models have successfully performed in the physical sciences. Such models are theoretical constructs with a precise, exhaustive and not too complicated definition; and they must be similar to reality in those respects which are essential in the investigation at hand. To recapitulate in detail: The definition must be precise and exhaustive in order to make a mathematical treatment possible. The construct must not be unduly complicated, so that the mathematical treatment can be brought beyond the mere formalism to the point where it yields complete numerical results. Similarity to reality is needed to make the operation significant. And this similarity must usually be restricted to a few traits deemed "essential" *pro tempore* - since otherwise the above requirements would conflict with each other.(1)
> (1) E.g. Newton's description of the solar system by a small number of 'masspoints'. These points attract each other and move like the stars, while the enormous wealth of the other physical features of the planets has been left out of account." (Author's footnote)[8]

The connection between model-building and theorizing in Physics is also unambiguously asserted by Morgenstern (1944, note 8 p.21): "We do not want to give the misleading impression of attempting here a complete picture of the formation of mathematical models i.e. of physical theories."

From then on, the use generalises in economics according to Zerner so that the period after 1944 goes beyond the scope of this section.

SECTION 1@4: IN SEARCH OF EARLIER OCCURRENCES IN ECONOMIC LITERATURE

One reason for selecting early issues (1890-1896) of the periodical The Monist as a potential source of information on the origin of the term 'mathematical economic model' is that it was concerned with bringing science and its methodology into all areas of human thought, implying a synthetic view of science. This suggests that issues related to economics were within the scope of the journal. A second reason is that if, as historians and philosophers of science tend to suggest, and as we shall see later (cf. Chapter 9) mathematical model-building is a *general* feature of 20th c. scientific theorizing one might find in the *unitary* views on science expressed in The Monist roots of this contemporary common scientific practice. The third reason is related to emerging contemporary knowledge of the history of mathematical modelling. Israel (1996, p.19) holds the view that:

> "In fact, the phrases "mathematical models" and "applied mathematics" began to be used exactly when a crisis hit the mechanist view on science and especially the idea that science provided a united approach to Nature" (our translation).

Since Ernst Mach is known as one of the first scientists to have challenged the methodology of classical mechanics, and since he was a regular contributor to the periodical, it is a potential source of information for our inquiry. Books that are landmarks in the history of mathematical economics and in neo-classical economics in particular such as Jevons' Theory of Political Economy and Marshall's Principles had already been published by the 1890's. Hence it can be considered that by that time, the knowledge they contained had reached an academic milieu wider than that of economists. Consequently, if economic mathematical model building is historically connected to the neo-classical tradition and to the development of mathematical economics, evidence for this might be found in the periodical. After describing the place of economics in the issues, we shall describe the place of the concept of model herein.

A survey of the table of contents and of some articles of the first six issues of The Monist (1890-96) reveals that Economics was not an important subject-matter. The subjects dealt with were the 'science of human mind', in a broad sense, such as psychology, ethics, anthropology and religion, as well as the physical and mathematical sciences.

The only article which focused on an economic topic occured in the 1893-94 issue (4, pp.533-544): 'Philosophy and Industrial Life' by J. Clark Murray. The author does not refer to 'Economics' but to 'Economical science' instead. In this article, he is concerned with showing that Economics and Ethics are connected to one another. He considers that since morality is dependent on human economic conditions, there is no such thing as *purely economic* actions. The author does not use the term 'model', nor similar terms, nor does he comment on the use of mathematics in what he calls "economical science".

In addition to this article, there are a few articles dealing with the economy as a secondary topic in the 1890-91 and the 1891-92 issues. These articles are presented as dealing with theories of the evolution of species.[9] Therein, the evolution of economic systems is considered as a component of Evolution in general.

Similarly, there are not many reviews of books on economic and related subjects in the issues under survey. However they are more abundant than articles per se on this subject. Such book reviews first appear in the first issue under the heading 'social philosophy'.[10] The next similar book reviews appear in the third issue.[11] In the last three issues, that is, from 1893 onwards, their numbers increase.[12] The main question the authors are concerned with is how to combine the economic organization of society with both the progress of the human species and justice; and the tone of their analysis is critical of the state of society of their time.

Articles and book reviews dealing with topics that are the concern of other Social Sciences such as

Anthropology or Sociology occur more often in the periodical and earlier than economic topics. The theoretical framework which is used to approach social and economic phenomena is that of the sciences of life and not the framework of Mechanics. Society is identified to 'a social organism'. The conceptual framework used in Mechanics is indeed discussed in the periodical, but only in connection with individual psychology.

The articles and the book reviews pertaining to Social Science referred to above do not make mention of the term 'model' nor of similar ideas. The term 'model' does not occur in the table of contents either. There are articles concerned with Mathematics. They focus on the analysis of the subdivision of the subject into highly specialised sub-disciplines rather than on the use of mathematics in other disciplines. Israel (1996, pp.311-312) provides a treatment of this which explains that even though there were articles in the periodical on Mathematics, Economics and on the methodology of classical Mechanics, there were no articles on the Mathematisation of Economics. Israel holds the view that the mathematisation of theoretical biology in the 19th c. is less important than the mathematisation of theoretical economics. Since the approach to Economics that prevails in the periodical is a biological approach to the economy it is therefore not surprising that the mathematical nature of economic science is not discussed in the articles and book reviews on economics published in the periodical.

One article concerned with the philosophy of perception however, shows concerns that are familiar to those expressed in contemporary epistemological and methodological literature about scientific mathematical models. This is an article by Paul Carus and Ernst Mach (Carus *et al*, 1890) entitled "Some Questions of Psych-Physics. A Discussion." In this article, the relationship between both the awareness and the possible material cause of a feeling, and the representation of this feeling are analysed. The first section in particular, focuses on the link, termed "parallelism" in the article, between abstract representations of reality and what allegedly they represent. It is argued along the line of what would be termed today an instrumentalist view on scientific theories, that: "In a certain sense all words and concepts are tools for dealing with the realities they represent. But some words are tools in a special sense. They have been invented for acquiring a *proper* representation." (The Monist, 1, p.410 emphasis added). This quotation insists on the essential normative feature of language and of cognitive frameworks which have been mentioned above (cf. Chapter 2 , note 12). Even though this quotation originally applies to issues in the philosophy of mind, it can be read in connection with the topic of this chapter, mathematical economic models. These models can indeed be considered as the conceptual tools which are considered today as the *proper*, i.e. socially admitted, *scientific* representations of reality. This understanding of contemporary scientific model-building, which is that of Morton (1990, 1993), tends to emphasize its gnosiological features rather than its epistemological features, (cf. Cheix 1996 , Section 1).

SECTION 2: ECONOMIC INSTITUTIONAL SOURCES

SECTION 2@1: TINBERGEN (1903-1994)[13]

The first terminological source is Jan Tinbergen. He is considered as a terminological source because it is clear from Desrosieres (1994, p.4) that he played an institutional role in Economics and Political Economy. He indeed worked for the League of Nations and the United Nations, and was also the Director of the Central Planning Bureau at The Hague. In addition, he was awarded the first Nobel Prize in Economics with Ragnar Frisch in 1969. In his early writings and possibly up till 1944-45 Tinbergen's use of the word model is generally restricted to that which Zerner (1993) identifies as characteristic of Tinbergen's work (cf. Chapter 8 , Section 1), but in addition he uses it in a different context in his later work.

In the first usage which is to be found in Tinbergen (1945), Bos *et al* (1962), Tinbergen (1964, 1967), it qualifies a class of representations of "real" economic relations with a cognitive function. For Tinbergen the representations are not mathematical in essence but the cognitive functions they are involved in are essentially mathematical.

Zerner's quotation of Tinbergen seems to indicate that the common characteristic of these models is that they involve simplifications of economic reality; they are not mathematical in essence, but rather present conditions for using a mathematically "powerful" treatment, which cannot be applied to too large a set of data.

In this context, "model" bears the same meaning as "theoretical scheme" in Tinbergen's (1935) article on business cycle where the connotation is economic-theoretical scheme. It expresses relationships between economic variables which influence business cycles. Tinbergen's idea is that once these relationships are expressed with mathematical equations defining implicit functions, one can derive mathematical characteristics of the functions and obtain the amplitude of the business cycles over time. Depending on how well the function is specified, Tinbergen writes that the economic theory whose scheme has been mathematized is "non-mathematical", "semi-mathematical" or "fully mathematical". The example he gives of a non-mathematical theory is Hayek's.[14] Tinbergen assesses Hayek's "literary" (his emphasis) theory in the following way:

> "An attempt at a mathematical "translation" [of Hayek's theory] leads the reader to many unsolved questions. This is not to deny that many original and valuable suggestions are made and, by stating the various assumptions and relations with mathematical precision, one may contribute to a better understanding of the unsolved problems" (Tinbergen, 1935, p.264).

Keynes' Treatise on Money is an example he gives of a semi-mathematical theory. Even though the mathematical relationships between economic variables at a given point in time are more straightforwardly obtainable than in Hayek's case, the intertemporal relationships between these variables leave the ultimate function unspecified. Examples of fully mathematical theories are Kalecki's, Frisch's and others, as well as his own. Not only are the theoretical schemes they are using mathematical schemes but they also fall within the set of functional relationships that can be mathematically treated so as to get qualitative characteristics of the functions they involve.

In later writings, the definition of models in the first sense is worked out in more detail and thereby altered. The mathematics definitely become an essential feature of a model itself. In addition, they are presented in connection to their political use, which was not the case in earlier writings. An entire book is devoted to their presentation (Bos *et al*, 1962) and in another book a chapter is dedicated to their taxonomy and history ("Scientific planning", in Tinbergen 1967, pp.218-233).

In the books under survey (Tinbergen 1945, 1964; 1967; Bos *et al* 1962) the models are considered as conceptual designs for implementing the phenomenon of economic growth.

They are theoretical instruments for economists either to explore economic reality, and build up causal relations, or to forecast future economic trends, as they are in earlier writings. They are "instruments" for economic observation, just as telescopes are for astronomy, and barometers are for meteorology. Further, they are theoretical, because they result from economic science just as optical instruments are the result of the history of science: they are sharpened as the theory progresses. Such a sharpening occurs throughout Tinbergen's work itself. A model is, however, not theoretical in the sense that it would reflect the view of a particular economic "theory" or school of thought. This view already occurs in Tinbergen's earlier writings:

> "In this connection it is important to remember that "different theories", in the economic sense of the word, may often lead to the same mathematical formulas, the only difference being in the economic sense of some of the constants." (Tinbergen, 1935, p.302).

Besides being instruments for economists, models are instruments for political decision-making as well and for framing economic policies. To turn to Tinbergen's words, they are a "hard core of programming" (Bos *et al*, 1962, p.4). For example, the models in Mathematical Models for Economic Growth are aimed at civil servants for the practical purpose of planning economic development, either in the short or long term, at a macro or at a micro level, in Western or Eastern, or in developed or underdeveloped countries.

The difference between the economic and the political instruments is merely teleological:

> "It is not entirely satisfactory that these models [econometric and decision making ones] should be called by a different name, since they may in principle remain exactly the same as before, that is to say, they may include the same equations.(...) [But] a different role is assigned to the variables (Tinbergen, 1967, p.229).

Models in the first sense have a formal side and a content. This distinction appears not only in later writings of Tinbergen but also in earlier writings as well (cf. previous quotation of Tinbergen, 1935).

The formal side of the design, which is mathematical, consists of equations (linear or not linear) whose coefficients are indeterminate or are statistically estimated:

> "A model consists of a number of elements, now to be considered from the formal side only. The economic contents will be discussed in Sec. 1.5. A model consists of (1) a list of variables, to be subdivided into known and unknown or exogenous and endogenous variables in the analytical sense used above; and (2) a list of relationships or equations specifying the links of any type that exist between the variables, to be subdivided into definitions, balance equations, technological and institutional equations, and behavior equations. Each equation represents a set

> of *links* or *reactions* with a causal direction, sometimes symbolized by arrows directed toward the variable affected. The links represented in one equation are those meeting in one variable at one point of time and together responsible for the size that variable will take at that time. In each equation, therefore, those variables occur which influence the variable in which the links meet. There are other elements too in the equations, representing a third element of the model: (3) coefficients. They describe the intensity with which one variable affects, through one particular link, another variable. (Additive constants will be considered coefficients too.) They are typical of what is sometimes called the structure of the mechanism or organism.
>
> The elements of a hypothetical ideal model of society can be used to analyse the process of planning and the coordinating task of the macro-economist in it." (Bos *et al*, 1962 p.6)[15]

In programming, the mathematical and abstract feature is essential. It is first a tool for checking that "no inconsistencies creep into the *system of figures* which ultimately constitute the plan" (Bos *et al*, 1962, p.5, emphasis added). Then, it combined knowledge into a manageable, though sophisticated object that helps to handle interdependent variables:

> "It [the ideal co-ordination method] is essentially of a mathematical or, if that term is preferred, of a logical character. The complete operation, in all details, of an economy and the human beings populating it can be described by a mathematical model of a much more complicated and sophisticated nature than anything we know in reality. To deny this is equivalent to denying the possibility of the scientific treatment of the operation of society. It does not imply the

> assumption of determinacy in the old sense, since we have the tool of stochastic variables, representing - in physics as well as economics, to cite only two examples - those elements of "freedom" sometimes invoked against the assumption of determinacy. The essence of a model is precisely that of *an orderly and, in a sense, complete administration of knowledge*. It is for this reason that the model supplies us with the tools of coordination." (Bos *et al*, 1962, pp.5-6, emphasis added).

In addition, the design has an economic content which corresponds to the schematized picture of the economic phenomenon which is referred to as "model" in Zerner's early quotation of Tinbergen [16]. Such a scheme is grounded partly on the economist's intuition to which Tinbergen refers to now and again and partly on communal economic sense (cf. Chapter 4, note 2). By intuition, the author means a judgement motivated by economic theoretical evidence and statistical data about what is relevant and what is not relevant to the analysis of a particular economic reality. For example, in Mathematical Models for Economic Growth, the economic contents of the models that aim at picturing the material side of the process of development (the phenomenon of production) consist of the following (Bos and Tinbergen, 1962, pp.10-14). The first type of economic content consists of selected economic *phenomena* integrated into the model (e.g. labour, capital, land, investment, prices, intertemporal/interfactorial/international and substitutions of final products). These phenomena are theoretical economic notions. The second type consists of *assumptions* about *relationships* between economic phenomena (e.g. proportionality between inputs and outputs, importance of capital/output ratio in investment, comparative length of investments processes) and *assumptions* about *the variations* of these variables over time (e.g. the quantity of land in constant over time). The third type is meta-theoretical and methodological. It consists first of *justifications of mathematical assumptions* (e.g. prices of products "can be conveniently put equal to one, meaning that value figures and volume figures are identical", Bos and Tinbergen (1962, p.12), thanks to economic history arguments and *assumptions on the nature* of development. Secondly, the third type consists in the enumeration of cases in which the previous assumptions are *appropriate* (e.g. the assumptions on prices holds if the world-market dominates the regional-market of the product and if its transportation costs are low).

Considered as economists' theoretical instruments, Tinbergen's mathematical/economic models bear similarities with mathematical equations used in mechanics. Such similarities are most obvious in Tinbergen's early writings on business cycles (Tinbergen 1933a, 1935) even though the author does not specifically refer to "models" therein. For example, Tinbergen explicitly uses the symbolism, the methods and the mathematical treatments used in studies of mechanical oscillations. The interrelation between these mechanical and economic studies does not reflect a merely biographical feature of Tinbergen's work. It is true that according to Zerner (1993, p.4), Tinbergen studied physics before he studied economics and that his doctoral thesis was devoted to minimum problems in physics and in economics. In addition, according to Desrosieres (1994, note 12, p.18), Paul Ehrenfest, one of the originators of quantum mechanics, influenced Tinbergen. But it is also true that theoretical approaches to business cycles using the formalism and the methods of the study of mechanical oscillations were commonplace in the 1930s and in Econometrica articles in particular. Further, the influence of physics seemed to fade away in Tinbergen's later writings. These are the reasons why we venture to say that similarities between Tinbergen's practice of economics and those of contemporaries or previous practices of physics does not hinge on a philosophy of science view about the achievements of physics compared with that of economics as is the case in Jevons' writings (cf. Chapter 5). Tinbergen's practice of economics rather hinges on the acknowledgement of the role of mathematics in building, testing and applying knowledge. His view seems to be that mathematics imposes various *controls* on knowledge which constitute its scientific feature.

We first recorded the second usage of the term model in the 1945 translation of International Economic Co-operation in the following sentence: "The best kind of planning is that in which *free trade is taken as a model*" (Tinbergen, 1945, p.21). This judgement is presented as the result of the appraisal of, what would be called today, different institutional economic systems for achieving the goals of international relations. These goals are to produce as much as possible at a regular pace, to allocate production, justly through persons, classes and nations, to guarantee freedom and to restrict both national and international conflicts. It is important to recall the pre-war shutting down of national economies, the post-war reconstruction context and the cold war to understand Tinbergen's advocacy. In his terminology, free trade is contrasted with free competition, which is exemplified by the Japanese case. In his sense, free competition refers to unscrupulous and ruthless trading, and free trade refers to harmonious exchanges. Free trade is not associated with financial profit maximising in the private sector nor with economic deregulation. On the contrary, it is a regulation process which does not exclude state intervention. The sole alternative to this "principle for the regulation of trade relations" is socialisation, which is equated with less liberty at every level of social order, and rejected for this reason. An additional point he puts forward in favour of free trade is that there *is* imperfect competition in world trade in terms of the elasticity of national shares in world trade. This imperfection is not justified by structural economic features of national economies but by an economically non-competitive distribution of wealth *within* national economies. This argument seems to be aiming at playing down critics of his who would point out that the regulated free trade he promotes is synonymous with the hindrance of natural-competitive actual trade.

This second usage must be read in parallel with Tinbergen's later view that welfare economics should be considered as the foundation of planning. By this he means that the criterion for choosing between global plans must be of the utilitarian kind, deployed to choose between social orders; even though the link between personal and social utility is ill-defined (Tinbergen, 1967).

The first usage of the term "model" by Tinbergen is constant throughout his work it seems. This concept of a model can be traced back to 1933 . It refers to economic problems expressed with equations to which *mathematical treatment* is applicable.

The formal mathematical aspect is part and parcel of the model which bears a heuristic and a methodological role, but is also closely related with "literary" economic theory. This close interrelation is due to the cognitive power of implicit functional relationships whose literary expression can be symbolised in a way that is at *once* mathematically meaningful. Because they involve natural numbers and because economic data are numbers, deductively obtained functional relationships can be controlled with empirical evidence. Conversely, empirical evidence suggests mathematical relationships. The second usage is part of the terminology of welfare economics and we record its occurrence in 1945. By contrast with the first usage, this one does not appear in early writings. For example, it does not appear in an article published in 1933 in which Tinbergen (1933b) uses welfare economic concepts such as ophelimity and expectations but he used them solely insofar as they help to give a mathematical account of data. In this sense, "model" refers to a norm or reference for the agency of world trade or of national economics and it is a guide for political and economic actions at a national and international level.

SECTION 2@2: THE COWLES COMMISSION[17]

Another terminological source is the Cowles Commission for Research in Economics. The origins of the information on the Cowles Commission that follow are Morgan (1990, p.55, note 11), Desrosieres (1994), the epigraph to the Cowles Foundation Monographs in Debreu (1959) as well as Armatte (1994, pp.870-871) and reference books. It was set up in 1932 and founded by Alfred Cowles at Colorado Springs. It was intended to develop econometrics in conjunction with the Econometric Society formed in 1929, and the journal Econometrica which was launched in 1933. It moved to Chicago in 1939 and was affiliated with the University of Chicago until 1955. Because the research staff of the Commission was appointed at the University of Yale, the Cowles Foundation superseded the Commission in 1955.

In a 1953 publication, Milton Friedman defines the specific meaning of the term 'model' in the Commission's terminology. At that time, Tjalling Koopmans (1910-1985) had been the director of the Cowles Commission since 1948; he held this position until 1954 and also from 1961 to 1967. The methodology promoted by the Commission to frame hypotheses

> "Consistent with known evidence is divided into two substeps: first, the selection of a class of admissible hypotheses from

> all possible hypotheses (the choice of a "model" in their terminology); second, the selection of one hypothesis from this class (the choice of a "structure") (...) As noted above, if one hypothesis is consistent with available evidence, an infinite number are. But, while this is true for the class of hypotheses as a whole, it may not be true of the subclass obtained in the first of the above two steps - the "model". It may be that the evidence to be used to select the final hypothesis from the subclass can be consistent with, at most, one hypothesis in it, in which case the "model" is said to be "identified"; otherwise it is said to be "unidentified" (Friedman, 1953, p.12 note 11).

'Model' is a descriptive-methodological term that refers here both to *a priori* schemes as in Haavelmo's terminology and to a pattern reflecting empirical data as in Slutsky's quotation. In this context, the term designates a particular scientific practice and also it defines a class of theoretical objects. Friedman concedes that this practice may have a heuristic value. Unlike the proponents of this practice, however, he denies it an appraising - methodological function inasmuch as the selection between alternative hypotheses "must be decided by some such arbitrary principle as Occam's razor" (Friedman, 1953, p.13). For its proponents, who are considered here as a tradition, this practice lays a unique basis for assessing a hypothesis and comparing hypotheses. Since this assessment is grounded first and foremost on the theory of statistical inference originated by Haavelmo in the 1940s, we consider that the methodological view of the Cowles Commission reported by Friedman deals with the *logical* foundation of scientific knowledge. In the perspective of the Cowles Commission models are used to *define* the correspondence between theories and the empirical data.

SECTION 2@3: VON NEUMANN AND MORGENSTERN

It can be considered that Von Neumann and Morgenstern's use of the term 'model' influenced economists as a whole since, according to Israel (1996, p.312), "The birth of modern mathematical economics (also in the 1920s) was implied by the works of von Neumann, who was not an economist himself." (our translation). It is clear from their definition of 'model' (cf. Chapter 8, Section 1@3) that it is a methodological term that describes a practice as in the Cowles Commission's terminology (cf. Chapter 8 , Section 2@2) and that it does not designate an object as it does under Tinbergen's terminology (cf. Chapter 8 , Section 2@1). This practice is the generalisation of theorizing in physics to science in general. In this terminology, models in physics and models in economics as similar in the sense that they play the same function in the two subjects. This similarity between models in physics and models in economics differs from this similarity in Tinbergen's terminology. In Tinbergen's case, it refers to the same kind of mathematical functions being used in the two subjects.

NOTES TO CHAPTER 8

[1] It seems that the terms 'structure', 'structural' and 'schemes' were used in the economic literature before the term 'model' was used. For example, R. Frisch and F. Waugh in "Partial Time Regressions as Compared with Individual Trends" (Econometrica, 1, pp.387-407) define a "structural relationship" as the *a priori* ideal and theoretical relationship that defines the subject-matter of empirical approximation. In this article, "structural relationships" bear an *essential* methodological role for the authors in the sense that as they rightly point out, when economists *compare* different methods of approximation (which are referred to as "econometric theories" in the article), they implicitly postulate the existence of such a referential structure. According to Morgan (1990, p.150) the term structure was later adopted via Haavelmo by the Cowles Commission (cf. also Chapter 8, Section 2); it is to be found in Slutsky (1937) as well. As for the term 'scheme', it occurs in 1933 as well in Tinbergen (1935) (cf. Chapter 8, Section 2).

[2] In addition to the article referred to in this thesis, these articles are: in Volume one:

"Partial Time Regressions As Compared With Individual Trends" by Waugh and Frisch; "Editorial", by Frisch; "L'Utilité d'une Théorie Générale des Ensembles Renouvelés" by Divisia; "Time Series: Their Analysis by Successive Smoothings" by Maverick; "Pseudo-Scientific Method in Economics" by Mayer.

[3] Victor Edelberg is not recorded in the New Palgrave Dictionary of Economics. In Econometrica (1939, pp.89-91) he also wrote two abstracts on 'L'Hystérésis de la Demande' and 'Discussion of "L'Influence de la durée d'emploi sur la Productivité du Travail"'.

[4] This quotation is from Zerner (1993, p.4). It is also quoted by Morgan (1990) but not in full. Its full reference however, is taken from the latter, it is: J. Tinbergen, An Econometric Approach to Business Cycle Problems, Paris: Hermann et Cie, 1937, p.8.

[5] Alain Desrosieres is conducting research for the Institut National de la Statistique et des Etudes Economiques, Paris.

[6] The original article by Slutsky in unavailable in the major British and French libraries as well as in Parisian libraries specialised in Russian publications.

[7] In addition, it is worth noting that the style of this section and the philosophical concepts it deals with such as 'whole', 'part', 'model', 'chance', 'process', are very similar to those of Aristotle in Physics, II, 3-6. In this section, Aristotle is concerned with defining what we mean by a 'cause' and how cause differs for 'randomness'. The connection we draw between these two authors is not a biographical conjecture about Slutsky's readings in philosophical literature but it is evidence that general epistemological questions occur throughout history at the occasion of technical scientific problems. This justifies retrospectively our claim (cf. Chapter 1) that methodology is an unavoidable stage in an epistemological study and also our interdisciplinary approach to the reading of economic theoretical texts.

[8] J. von Neumann, O. Morgenstern, Theory of Games and Economic Behaviour, (Princeton: Princeton University Press), 3rd edition, 1953 p.32, quoted from Zerner (1993, p.5).

[9] These articles are:

Joseph Le Conte, "The Factors of Evolution", The Monist, 1, 1890-91, pp.321-335; Editor (Paul Carus), "The Continuity of Evolution", The Monist, 2, 1891-92, pp.70-94.

[10] The book reviewed is John S. Mackenzie, An Introduction to Social Philosophy (The Monist, 1, 1890-91, pp.601-604).

[11] The book reviewed is Gustav Engel, Die Philosophie und die Sociale Frage (The Monist, 3, 1892-93, pp.478-479).

[12] These books are: James Bonar, Philosophy and Political Economy in Some of Their Historical Relations (The Monist, 4, 1893-94, pp.316-317); Emile Durkheim, De la Division du Travail Social, (The Monist, 4, 1893-94, pp.279-280); J. Shield Nicholson, Principles of Political Economy (The Monist, 4, 1893-94, pp.474-475); J. Novicow, Les Luttes entre Societes Humaines et leurs Phases Successives (The Monist, 4, 1893-94, pp.121-125); Th. Ziegler, La Question Sociale est une Question Morale, (The Monist, 4, 1893-94, pp.447-448); J. Novicow, Les Gaspillages des Sociétés Modernes (The Monist, 5, 1894-95, pp.439-440); Julien Pioger, La Vie Sociale, La Morale et le Progrès (The Monist, 5, 1894-95, pp.436-437); G. Tarde, Logique Sociale (The Monist, 5, 1894-95, pp.434-436).

[13] It is reported (Sellekaerts, 1974) that the most comprehensive bibliography on the author is collected by L.H. Klaassen, L.M. Koyck and H.J. Witteveen (eds.) in Selected papers, Amsterdam: North Holland, 1959. This bibliography can be supplemented by: J.B.D,.

Derksen in De Economist, CVII, 1959, 798-799; by J.P. Pronk in De Economist, CXVIII, 1970, 156-73; De Economist November 1994.

[14] We are grateful to Professor Reid for suggesting to us that Tinbergen is probably referring to Hayek's Prices and Production published by Routledge in 1931.

[15] Exogenous variables concern "phenomena of an extraeconomic character, that is, natural, psychological, technical, or institutional phenomena". It seems that Tinbergen considers that they are specific to the economy under consideration. Endogenous variables on the contrary, concern economic phenomena of a wider scale than that of the economy under consideration. (Tinbergen, 1962, p.1).

[16] J. Tinbergen, An Econometric Approach to Business Cycle Problems, Paris: Hermann et cie, 1937, p.8. (cf. supra Chapter 8,Section 1).

[17] The history of the Cowles Commission is available, according to Morgan (1990) in the following. C.F. Christ, 1952, 'History of the Cowles Commission 1932-1952', in Economic Theory and Measurement, Chicago, Cowles Commission for Research in economics; C. Hildreth, 1986, The Cowles Commission in Chicago, 1939-1955, Berlin, Springer-Verlag; R.J. Epstein, A History of Econometrics, Amsterdam: North-Holland.

CHAPTER 9

EPISTEMIC-COMPARATIVE DEFINITION

"And the Lord said, Behold, the people *is* one, and they have all one language; and this they begin to do: and now nothing will be restrained from them, which they have imagined to do.
Go to, let us go down, and there confound their language, that they may not understand one another's speech."

Holy Bible, Genesis, 11.

SECTION 1: (MATHEMATICAL) MODELS IN CONTEMPORARY SCIENCE

SECTION 1@1: AN INTER-DISCIPLINARY PHENOMENON

Nowadays, the use of models is not confined to the economic domain. It belongs to "hard" sciences such as logic, physics and biology and also to the social sciences as a whole. Philosophers (e.g. Morton, 1990, 1993) together with scientists (e.g. Zerner, 1993, Klein, 1991) and historians of science (e.g. Israel, 1996) notice this fact. According to Etienne Klein (1991) there are periods during which "a particular type of problem-setting and problem-solving prevails, so that approaches to knowledge are unified" (our translation) and this is the case for the 1990s. Hausman (1992, p.81) remarks similarly that "The fact that theoretical economics is devoted to the exploration of models does not distinguish economics from other sciences. In theoretical work, *all* scientists attempt to exclude the complications of reality".

According to Badiou (1969) this way of conducting scientific inquiry was current in 1969. In all probability, it was also current practice at the beginning of the 1950s since at that time, there were already attempts at classifying models (cf. Maldonado, 1970, pp.139-140, note 37).[1]

This way of doing science is model building, which is often mathematical model-building. It originated before the 1960s (cf. Chapter 8 and Israel, 1996). Zerner (1993) considers that, that from a terminological point of view, 'model' is more widely and more loosely used in the social sciences than in the physical, mathematical and logical sciences. So far as physics is concerned, this is suggested by Zerner's (1993) subject index inquiring into physics abstracts up to 1990, upon which we shall now report. Among applied mathematicians, the term appears in the 1960s but is not widespread in English-language journals until the mid-1970s. It is scarcely used in France, and when it is, it occurs at a later date.

Zerner (1993) mentions the use of 'model' in psychology in 1951, in sociology (1950-51), in demography in 1958 and in operational research in 1951. Badiou mentions its use in 1958 by Levi-Strauss in Anthropologie Structurale. It also occurs in linguistics as it does in Muller (1979).

In post-war physics, the term is scarcely used. It can mean a simplification of o well determined theory for the purpose of approximation and practical use. This is the case in meteorology. The theory is supplemented with a series of models whose role is to approach real atmospheric conditions step by step. This usage of 'model' in meteorology is confirmed by Morton (1993). Alternatively, it can refer to mathematically simple problems set up to study a physical phenomenon which is not well known and whose theoretical mathematical description is not easily manageable. This is the case in studies of turbulence phenomena, and such models are known as Burger's models. It is not expected that these models will be approximations of reality; it is enough if they share some qualitative features with it. Later in the 1960s, field theory models provide example of such simplified theories. They differ from Bruger's however, because the corresponding theories are axiomatized, and they may function as a check on the models' consistency. In addition to these fields, models are used in life sciences. In palaeontology, Brunet-Lecomte *et al* (1992) refers to the 'Cox model', the 'model of the Red Queen' and to the 'Stationary model'. These models are theoretical hypotheses about the dominant causes of the extinction of species, or the evolution of species over time. They can be confronted with empirical data thanks to modern mathematical 'methods' like fractal analysis. Another usage of 'model' in this article by Brunet-Lecomte *et al* (1992) is that of a paradigm in the following sense. To illustrate, the radiation of the particular species of the Arvicolid family is considered today as a model for the studies of evolution. This is because of the good quality of the available data on this species. Hypotheses on evolution in general are framed from this case-study, tested on it and then tested on other data sets. This article is not an isolated case in biology. In his speech at the annual conference of the European Society for Philosophy and Psychology held in Paris in September 1994, the neurobiologist J.P. Changeaux pointed out that in the life sciences in general 'model' has such a double meaning, that of a theoretical hypothesis and that of a paradigm.

The term 'model' is used also in logic. Today, in accordance with the Carnap and Tarski tradition in logic, a model is an object that instanciates or realizes a formal structure called a theory. It belongs to a conceptual realm where one can distinguish between truth and falsehood. "An interpretation constitutes a *model* of the theory if and only if it makes all the sentences come out true." (Hausman, 1992, p.71; cf. also Krivine, 1989, Lassaigne, 1989). We rely on Guillaume (1994) for the following historical information on this matter. The earliest occurrence of the term in the philosophical logic and mathematical logic literatures which Guillaume (1994) mentions is in an early article on game theory by Von Neumann published in 1925 in the Journal für die reine und angewandte Mathematik entitled 'Eine Axiomatisierung der Mengelehre'. The German term 'Modell' is used in 1929 by Zermelo in 'Über den Begriff der Definitheit in der Arithmetik' and in 1930 in 'Über Grenzzahlen and Mengenbereiche' both published in Fundamdenta Mathematicae. According to Guillaume

(1994), there are numerous occurrences of the term in these articles. By contrast, the next occurrence in Tarski's writings is a rarity. It occurs in a paragraph written in 1933 for the Logische Syntax der Sprache (1934) but included only in the English edition of the book (1937). However, until 1935, it does not occur in Tarski's writings concerned with meta-mathematics. In 1935, Tarski uses it in Grundzüge des Systemenkalkül. Tarski also uses it in 1937 in his address 'Sur la Méthode Déductive', at the 9th International Congress for Philosophy, in which he draws differences between realization, model and interpretation. Then Gödel uses it in 1938 in 'The Consistency of the Axiom of Choice and of the Generalized Continuum Hypothesis' in the Proceedings of the National Academy of Sciences of the U.S.A.

In short, it seems that by 1948, the term was still not widely used in titles in the literature of logic. As far as the genealogy of the concept of model is concerned, Guillaume(1994) considers that the modern logical concept of model originated from the separation between the science of formal languages, also called "symbolic algebra", and the interpretations of such formalisms in "applied sciences". This separation culminated in a report by George Peacock before the British Association for the Advancement of Science held in Cambridge in 1833. Then three notions of model developed fairly independently in geometry and axiomatics, in algebraic logic and in universal algebra. These three notions are definitely unified between 1945 and 1950 so as to result in the contemporary logical and modern concept of a model. It seems that 'model' has not always referred to a *true* structure, but that it has also referred to the weaker idea of a structure interpreting another structure. In addition, it seems that in the fields mentioned above it has never referred to an idealized structure.

Not only are objects called 'models' used in many sciences, but also the terminology regarding them in one science is not always consistent with the terminology in another science. The main inconsistency, which is often mentioned in the philosophical and methodological literature, is between the meaning of 'model' in Logic and its meaning in Economics. Hausman (1992, p.71) remarks that: "This logician's notion of a "model" is *not* what economists mean when they talk of models." It is true that at first, the common phrase 'mathematical model of the economy' seems paradoxical compared with the use of 'model' in 20th c. mathematical Logic. Granger (1994, p.245, note 1) remarks that this paradox occurs not only in connection with economics but also in connection with the social sciences as a whole. In the Social Sciences, 'model' refers to something abstract or formal, and to an idealization or a representation of the *real* economy. It is often considered as a theory or a hypothesis which can either be false or true. In Logic, a model is less abstract than a theory. It is the reality to which the theory applies and sometimes it is the object about which one theorizes. It also plays the role of assessing formal systems since according to Badiou (1969), models can be used as a criterion for selecting and comparing formal logical theories. For instance, if the formal logical structure does not apply to basic mathematical structures (e.g. the set of numbers N) then the formal structure is doubtful and the axioms undergo a revision. Therefore logical models are on the side of "the empirical" and they have a methodological role in meta-mathematics. As Granger (1994, p.245, note 1) remarks, however, there is a similarity between these two contradictory definitions of models. In both cases, the model is involved in the representation of one system by another system.

SECTION 10.2: A TOPIC DEALT WITH BY PHILOSOPHERS OF SCIENCE

References to the terminological diversity surrounding models in science seem to be commonplace in the philosophy and methodology of science literature. In the methodology of economics literature, bibliographical references giving evidence of such an interest can be found in Hausman (1992) and Armatte (1995). The features of *mathematical* economic models that have been brought into light previously (cf. Chapter 7 , Chapter 8) are also, according to Hausman (1992, p.80), features of economic models in general: "(...) models in economics serve many purposes and are of many kinds". According to Hausman (1992) some models are 'special case' models, that serve as crutches or pedagogical devices, and resemble the scale models used by engineers. Their methodological function is not unique either. The question as to whether they serve to test theories or whether they are "occasions for the acquisition of new perceptual beliefs" is controversial. This view on the methodological status of mathematical models is also Morton's (1990, 1993).

We shall now turn towards reporting some epistemological attempts at defining scientific models. The attempts we shall report upon are due to Hausman(1992), Badiou (1969) and Morton (1990, 1993). They are particularly relevant to this study because contrary to most philosophy of science studies, they are concerned exclusively or at least partially with Economics. The aim of these reports is not to review fully the literature on this subject. The aim, rather, is to explore whether models are as diverse in the philosophical literature as they are in the scientific literature.

Hausman (1992, esp. pp.70-82) identifies the major accepted use of the concepts of "theory" and "models" in the methodological and the philosophical literature, with a particular emphasis on the views that influenced him. These views include Suppes, Sneed and Stegmueller on the one hand, and Giere on the other.[3] The first set of authors consider scientific theories to be predicates. In particular, "Suppes argues that scientific theories are set-theoretic predicates, because he hopes to provide set-theoretical formal restatements of scientific theories." (Hausman, 1992, p.74). Sneed attempted to fulfil Suppes' desiderata so far as economics is concerned.[4] Giere considers that "scientific theories are *definitions* of predicates themselves" (Hausman, 1992, p.74, emphasis added) and that they are but one part of science. The other part involves "proposing *theoretical hypotheses*, which assert that the new term is true of some actual system" (Hausman, 1992 p.75). The view can be held that the diversity of philosophical definitions only reflects the diversity of scientific terminology. The view can also be held that the plethora of philosophical definitions is the cause of many scientific terminological confusion, especially since philosophers often change the definition they give of a 'model' with time, as Giere does, according to Hausman (1992, p.75, note 3). Be this as it may, we consider that these conflicting attempts at defining the different kinds of theoretical structures are a sign that there is some work to be done by the epistemologist (cf. Chapter 9 , Section 2@2).

Hausman introduces his own work in connection to this context as follows:

> "Although terminological changes court confusion, this one is worth the risks, for it *brings* the language of this abstract discussion of scientific theories into close accord with the usage of economists and avoids the paradoxical denial that scientific theories make claims about the world" (Hausman 1992, p.75, emphasis added).

So far as economics is concerned, Hausman solely considers neo-classical economics. However, because he is adapting positivists' concepts of the philosophy of science to economics, the definitions he gives of model and theory apply to a certain extent to science in general and not just to neo-classical economics. Hausman's (1992, p.75, note 4) work is not intended to reflect what econometricians call models. To him, a 'model' is either trivially true or it is neither true nor false; whereas a 'theory' is either true or false. According to his view, another difference between a 'theory' and a 'model' is that a 'theory' is a set of *lawlike* assertions, that is, assertions of regularities in the appearance of phenomena, whereas the postulates defining a 'model' do not have *a priori* a specific form. A 'model' is a definition of a predicate which applies to a system. This definition consists of a set of postulates. The cognitive function of a model is to abbreviate properties of a system and to help understanding of this system. For example, a model of agents' maximizing behaviour does not describe actual behaviours, nor is it a hypothesis about actual agents themselves. It is a definition of what it is to maximize behaviour. What connects models to actual behaviours is what Hausman calls the 'closure of the postulate' of the 'model'. A 'closure of a postulate' is an assertion that this postulate is true of a particular system. Finally, a 'theoretical hypothesis' is the assertion of the truth of some postulates that define the model.

Badiou's interpretation of the diversity of models in science is interesting for several reasons. First of all, even though a philosopher, he has a good knowledge of mathematical logic. Secondly, some of the results of his analysis of the state of the sciences coincides with Morton's later results, upon which we shall report shortly. Thirdly, he concerns himself with economic models (p.27). Badiou distinguishes three types of model . One he calls *ideological*, the other *scientific* and the third *epistemological*. He blames the notion of model defined by the positivists for being ideological (cf. Chapter 9 , Section 2). Badiou means by this that they make use of a scientific concept, namely that of a model as used in mathematical logic, in order to describe scientific practice, from the social sciences to the mathematical and natural sciences. For Badiou, it seems that epistemology is *descriptive* of scientific practice; and mathematical logic *is* such a practice. So far as economic models are concerned, Badiou (1969, p.16, our translation) testifies that:

> "(...) there is an increasing use of the so called "mathematical models" in economics (...)"

and defines their function as follows:

> "(...) a model endows an economic policy with superficial coherence, it justifies it and overshadows both its origin and its logic"

Under this interpretation economic models are mostly examples of ideological models. Scientific models are logico-mathematical models (cf. Chapter 9 , Section 1@1). They apply to mathematical structures, and they are the concern of the theory of models or of the theory of categories. It seems that Badiou holds the view that the diversity of models is the result of scientists holding (explicitly or implicitly) a realist/representationalist view on scientific theories. This view is that scientific theories are manageable and plausible images of "the world" as opposed to instruments for transforming it. He grounds his criticism on the analysis of what scientists and philosophers supporting this view consider to be features of a "good" model for a phenomenon: simplicity, comprehensiveness with regard to the phenomena concerned, and independence from other views on these phenomena, as well as independence from other phenomena, which we would call 'internal consistency'. According to this view, a consistent model, together with an interpretation connecting it with reality, has the main features of scientific knowledge. Badiou (1969) considers that this view on scientific theories is a variant of "common sense empiricism".

The scope of Morton's (1990, 1993) inquiry as well as Badiou's (1969) is the philosophy of science. However, the range of models he is considering is narrower than those of Badiou. Morton is indeed referring to *mathematical* models exclusively and he only considers models in physics and economics. Morton considers, as Badiou does, that mathematical models are a feature of *scientific* practice, but he is concerned with extracting the features they share with both common-sense representations and common-sense explanations. In this respect, therefore, his analysis of the diversity of models is grounded on the same observation as Badiou's, namely that there are some similarities between these scientific objects and common sense. Morton's (1990, 1993) inquiry is two-sided and it is essentially taxonomical. Morton's (1990) attempt to define the types of imperfect explanations provided by quantitative and qualitative mathematical models in science is terminological and methodological. This attempt consists indeed in defining a terminology ('width', 'depth', 'contrastive') in order to assess the cognitive value of a scientific explanation. To consider explanations as 'contrastive' explanations is to consider them "as saying not why something happened but why it happened in one way rather than another" (Morton, 1993, p.265). For example, in connection with this thesis, Slutsky's (1937) model (cf. Chapter 8), which represents time series mathematically, does not provide a 'contrastive' explanation, since it does not say why the curve does not have another shape. According to Morton, 'contrastive' explanations are the best type of explanations. The 'depth' of the explanation of a phenomenon refers to its contrastive force, that is, to the extent to which it explains why *this* phenomenon occurs rather than another. The 'width' of the explanation of a phenomenon refers to the extent to which it provides a comprehensive description of the phenomenon. Morton (1990, 1993) uses this terminology to assess, amongst other models, models of the preferences of economic agents, using utility functions. He considers that their 'width' is satisfactory, whereas their 'depth' is *quasi*-nonexistent. Further, according to Morton (1990), economists do not aim at 'contrastive' explanations. The methodological terminology Morton defines is as diverse as that of scientific mathematical models, but it provides a means of comparing the explanatory power of models.

SECTION 2: MATHEMATICAL MODELS AND THE STRUCTURE OF ECONOMIC SCIENCE

SECTION 2●1: INTERPRETATIONS OF THE WIDESPREAD USE OF MATHEMATICAL MODELS IN ECONOMICS

We shall now report philosophical interpretations of the widespread use of mathematical models in economics. The first is the interpretation of the positivists. In the next subsection, we shall assess whether it is a sign of the unification of economic knowledge, and also whether it is a scientific feature.

The presentation of the interpretation of the widespread use of scientific models from a positivist point of view is necessary because according to Hausman (1992) and Caldwell (1982) this tradition has strongly influenced 20th c. philosophy of science and also the methodology of economics. The presentation of logical positivism that follows mainly relies on the following sources: Piaget (1976), Hausman (1984 Editor's Preface Introduction; 1992, Introduction, chapter 5, Appendix 1), Neurath (1983), Nagel (1961) and Brandt *et al*(1965). To these sources must be added the study of contemporary mathematical logic, which bears the marks of the logical works of Carnap and (since Carnap also shared their viewpoints) of the Vienna Circle (cf. Guillaume, 1994). The positivist philosophy we are presenting here, also referred to as 'neopositivism' if one wants to distinguish it from Auguste Comte's philosophy began in the 1920s and 1930s in Vienna and Berlin and became less influential from the mid-1970s onwards. It is conventional to call it 'logical positivism' when referring to its beginnings and to call it 'logical empiricism' in its 1950s American and more sophisticated version. The epistemology this tradition developed is *scientific* epistemology, by which is meant that they have been mostly and originally concerned with explaining scientific knowledge. According to Brandt *et al*(1965, Introduction):

> "The central task of epistemology is to provide a generalized critique of the grounds on which claims to knowledge are supported, by constructing a systematic account of the principles by which the truth of statements may be properly assessed, as well as of the rationale of these principles. A theory of knowledge so understood is indistinguishable from a theory of logic that is general enough to deal not only with the formal validity of arguments, but with the basis on which cognitive claims of any sort can be judged to be warranted, either as cases of knowledge or as instances of probable or reasonable belief. Accordingly epistemology is a normative discipline, in the sense that it provides standards for measuring the worth of cognitive claims, along with a systematic defense of the reasonableness of those standards. But it is in part also a descriptive discipline."

This tradition was influenced at the same time by the physics of Einstein (1879-1955), the development of formal logic at the turning point between the 19th c. and the 20th c., the empiricism of Hume (1711-1776) and

Mach (1838-1916) and finally Kant (1724-1804). Positivists consider that scientific knowledge has two sources, one is the empirical-experimental and the other is the formal logico-mathematical. Formal logico-mathematical judgements are tautological and have no truth or falsity value. Empirical judgements by contrast, are either true or false. A scientific theory consists in a set of syntactic propositions that may be expressed in first order logic, so as to appear as a set of propositions closed by deductions. Formal axiomatization is therefore the ultimate form of scientific theories. In order for them to have cognitive content, which is identified it seems with having truth-value, they must be endowed with empirical interpretations called 'models'. The notion of model of the positivists seems to be identical with the logician's. According to Hausman (1992, p.75): "Note that this sense of "model" [Hausman's] is distinct from the logical positivist's notion. In their notion, a model is an interpretation of the sentences of a theory such that they all come out to be true." One of the aims of the positivists is to separate science from non science, for example from metaphysics, magical knowledge and theology. What is important for the present purpose is that positivists consider that the feature that distinguishes scientific theories from common sense is their being translatable into the logical-mathematical language. This language is considered to be the best for the purpose of interpersonal communication of objective facts. This translation is a task, and the unification of the sciences is its result.

From a positivist point of view therefore, the importance of *mathematical* models for contemporary science and for Economics in particular, is a testimony to the fact that the process of unifying sciences is at work. The fact that some models are not mathematized may be interpreted in this perspective as a call for further logico-mathematical translation. This perspective does not provide the means to explain why models, in a large sense, are so popular in science.

The multifarious usage of mathematical economic models within Economics can be put forward to support conflicting views on the methodology of economics. On the one hand, one may express the *descriptive* statement that economic practice is unified thanks to mathematics. One may point out that, to a certain extent, economic knowledge is unified in a stronger sense similar to the positivist's view on science. One can point out that axiomatized models such as the Debreu-Arrow model or the Morgenstern-Von Neumann-Nash model are generic models in the following sense. First, these models have a deductive structure to a certain extent. This is because they consist of hypotheses and axioms from which results and theorems are deduced. The deduction may be mathematical deduction, or it may rest on the semantics of either Economics or of vernacular language. This uncertainty about the nature of the deduction explains why, as Mahieu 1989) has shown in the connection of other models, it is difficult to express the deductive structure formally (cf. Chapter 6, Section 2@2 note 8). However, it is not certain that this difficulty justifies the claim that these models do not have a deductive structure. Assessing this matter fully is beyond the scope of the present work, since it requires deeper philosophical investigation than is required in this work. Secondly, these models serve as paradigms for other models (cf. Chapter 7). Finally, they provide general and formal definitions of the 'maximizing economic agent' which has been the core of economic theory for a long time, and remains so today (cf. Friedman, 1953, Van Parijs, 1990; Rosenberg, 1983). In addition, if one adheres to the contention about physics - which, as Zerner suggests, was made by Morgenstern and Von Neuman in 1944 (cf. Chapter 8, Section 1@3), namely that model-building is the core of scientific theorizing in physics; and also to the contention that physical theories are the paradigms of scientific theories, then one can infer that economics is a fully formed science.

On the other hand, one can focus on the fact that mathematical models are very different from one study to the other, and also from one school of thought to the other, and consequently that the economic field is not unified (cf. Chapter 7 , Chapter 8). In addition, this lack of unification and of fixed meanings for concepts may either be considered (as the positivists and Jevons contended) to be a lack of features characteristic of science, or (as Morin ,1982, would argue) to be a proof of the dynamics of science:

> "As a matter of fact, ideological and metaphysical conflicts, whether they be conscious or not, are a sine qua non of scientific dynamics (...). Under no circumstances ought conflicts be eradicated because they are the very condition of the liveliness of science, which is a game regulated by empirical and logical rules." (Our translation).

Lakatos, according to Bailly et al. (1990), remarked in Proofs and Refutation that such a lack of agreement also occurred in mathematics. Edgar Morin (1982), whom we conjecture to be influenced by Lakatos and Feyerabend (1979), supports the view that it is a feature of science in general. The views of Morin and Lakatos on the creative role of diversity and debates in science seem to imply a view on significance or meaning that is akin to the Wittgensteinian (1958) view exposed in the Philosophical Investigations. This view is that social practice, whatever the angle from which it is approached - from anthropology as Latour et al (1988) do, or from linguistics as MacCloskey (cf. Backhouse, 1991a) does - generates significance. This is to say, scientific methods and results are not important in themselves, or because they 'reveal reality'. They are significant in connection to the particular historical and sociological background. Meaning as social practice in this sense is opposed to meaning as a reflection of 'truth' or 'reality'.

By contrast, methodological diversity is sometimes considered to be a negative feature of economic knowledge. If one believes that the significance of phrases is inversely proportional to their usage, one can argue, after Morton (1990, 1993), Zerner (1993) and Badiou (1969) that the term 'mathematical economic model' does not refer to an epistemologically *specific* scientific practice. Instead, it is a rather general and misleading common-sense term. From a methodological point of view concerned with the appraisal of economic knowledge, it can be considered that this terminological looseness is an impediment to the advancement of knowledge. If one considers that the basic methodological question to ask about a logically structured set of arguments is: "Does it play the role for which it is designed?", and also if one considers that such roles vary from one subpart of economics to the other, then not having a terminology suitably for discriminating between mathematical economic models is a flaw. No general statement can indeed by stated about models. This flaw can be relieved by building a methodological terminology in a way which is explored by Adam Morton (1990, 1993) (cf. Chapter 9, Section 1@2), or else it can be relieved by historical investigation into the causes of the looseness of the terminology, or else by the assessment of particular models. These last two ways are explored by Zerner (1993).

SECTION 2@2: THE VAGUENESS OF THE CONCEPT OF MODEL: A RESULT OF PHILOSOPHICAL INQUIRY?

It was previously mentioned in passing (Chapter 9 , Section 1@2) that the terminology used by philosophers to study scientific mathematical models was as confusing as the object to which they apply. An example of how vagueness might result from philosophical inquiry is provided by Hausman's (1992) definition of models (cf. Chapter 9 , Section 1@2). He begins with pointing out that 'models' as defined by the positivists and by the logicians, does not match the usage of the term by the economists. Then he uses logical and positivists' concepts to define a new concept of theoretical model which is in accordance with the practice of economists. At the same time, he leaves out one aspect of this practice, which is closer to the positivists' concept of model than the theoretical models he defines. This aspect is mathematical model-building in econometrics. *Prima facie*, it is close to the positivists' concept of model since it concerns the relationship between theoretical and empirical investigation.

Consequently, one can ask the question whether the diversity we pointed out (Chapter 7 , Chapter 8 , Chapter 9) is the result of our analysis and whether it is a phenomenon that deserves being studied.

From the outset, let us point out that such question is not relevant to this thesis only, but to other epistemological/methodological enquiries as well. For example Otto Neurath (1935, p.115) in 1935 considered the question of whether the structure of science which positivists were attempting to build was a mere consequence of the methodology of "scientific philosophy" or whether it could be based on ontological foundations. His answer was that the unity of science had ontological foundations:

> "As *scientific* people, we are prepared to check all our tenets by observation statements, but also - far removed from every absolutism - to alter the principles on which the checking is based, when this seems necessary. But for our attempt at a common procedure *uniformity* is needed. Is this uniformity the logical consequence of our program? It is not; I stress this again and again; I see it as a *historical fact* in a sociological sense."

Beyond this provisio , there are at least two questions deserving consideration. The first, is whether the difficulty in defining mathematical economic models is one that pertains to definitions in general or only to mathematical economic models. It seems indeed that mathematical models in economics and in science in general perfectly illustrate the Wittgenteinian idea of family resemblance (Wittgenstein, 1958). Basically, the idea is that there is nothing common to all such models but only resemblances within subsets of each object. These are not partitions of such designated objects, so attempts at defining them which seek to extract their essential features, are bound to fail. The second question is whether mathematical economic models and mathematics are essential features of contemporary economic knowledge or whether they simply denote a fashion. Further, statistical empirical studies such as those of Stigler (1965a), Anderson *et al*(1986) and Leontief (1982) might be used to show that they are now falling into disuse.

The first question recalls a long debated philosophical issue about the correspondence between words, things and the rationale of definitions. Suffice it to say that we follow in Aristotle's wake, and more recently in that of Eco (1984, pp.12-13), in that we consider not only that the identity of things which bear the same name is a postulate, but also that making such identifications is a defining feature of philosophical inquiry. And we consider that the difficulty in defining mathematical economic models pertains to definitions in general. The question is not whether one ought to identify objects as we are doing here; but rather why one does want to do so, and what is the purpose of definition in this context. Our answer to that question, which is also an answer to the previous second question - that about the importance of mathematical economic models in contemporary economic knowledge, is that we conceive philosophy as dealing with what is important for the philosopher/methodologist's contemporaries, and that mathematical economic models assume great importance today for scientists as well as for the population in general. Because they are used for economic policy-making, they are important for any individual belonging to the society to which such policies are applied. Because they are used by many scientists, they are an important input in the production of science today. Finally, mathematical economic models are discussed amongst philosophers, which is additional testimony to their importance, and indeed enough to convince us that they deserve to be looked at. Further, the fact that authors in the Marxian tradition such as Badiou (1969) converge with the tradition of the positivists in acknowledging the widespread use of models in science is a case for believing that the diversity we expounded earlier (Chapter 8, Chapter 9, Chapter 9) is not merely a result of our methodology and of our view on philosophy.

We might be criticized by supporters of Kuhn's view on the grounds that mathematical economic models and the mathematical method are part of *normal science* today.[5] Kuhn's (1970, p.5) view on normal science is exemplified by the following quotation: "When examining normal science in Sections III, IV, and V, we shall want finally to describe the research as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education".

Firstly, the difficulty with the Kuhnian concept of 'normal science' is that it does not seem to be sharp enough to provide a distinction between the 20th century social phenomenon of "professionalisation" in the sciences and the psychological and epistemological phenomenon of the relationship between knowledge and change in individual belief. Both phenomena are studied by methodologists and philosophers with an interest in the widespread use of mathematics in Economics. But their methodology and their results are complementary, though different. For example, Stigler (1965a), Anderson *et al*(1986) and Leontief (1982) point out that many mathematical economists were first trained as physicists, in order to explain their using mathematics. By contrast, Morton (1990, 1993) suggests a similarity between the role of mathematical models in the production of contemporary scientific knowledge and the role of personal beliefs in acquiring a representation of the world coherent with one's experience. Both explanations refer to the use of mathematics as being a (sociologically/gnosiologically) - normal feature of contemporary science. But they provide different understandings of this feature, since Morton (1990, 1993) implies that the use of mathematical models is a non-scientific feature of science, whereas sociological approaches do not necessarily imply this at all. This makes the idea of 'normal science' unclear. Secondly, it can be argued that the normal un-revolutionary science is worth considering. Further, the non-normative epistemological approach to science we embrace involves looking at how normal science is carried out at present[6]. Whether the survey of one feature of contemporary normal science, such as mathematical models, is ultimately epistemologically fruitful, or whether, as Badiou (1969, p.17) suggests, "(...) focusing on models generates an obstacle to the epistemological inquiry" (...) and that: "This proves that models sit at the margins of the production of knowledge. Therein at least one can not contest their role (our translation), this is a question which is left open in this thesis.

The answers we have just given yield two other questions pertaining to the methodology of our inquiry into mathematical economics. One is whether the view about philosophy we share with Aristotle and Eco on the role of philosophy (cf. Supra) is compatible with our methodology (cf. Chapter 2). In other words, is the 'intra-scientific' epistemological approach to mathematical economics consistent with the view that philosophy has a unifying function? The other question is to what extent can one separate the study of mathematical economics from the general and historical phenomenon of the mathematization of the sciences which has been considered elsewhere (cf. chapter 4 Section 2@2, chapter 6 Section 1@1, Chapter 9). and from the philosophical literature on the subject, or whether it is natural to consider the two topics together. Arguably, there is a risk that such an approach implies the vague idea that the sciences "naturally" tend to mathematize. We shall not be able to provide answer to thesequestions since they point to problems fundamental to philosophical inquiry in general that are not specific to our own particular study. Hence they go beyond the scope of this thesis.

NOTES TO CHAPTER 9

[1] This note contains bibliographical references on this matter and in connection to computer science and linguistics in particular from 1953 onwards.

[2] In a methodological thesis such as ours, this would indeed require an extensive introduction to epistemological subjects, which is beyond the scope of this work. However, because we hold the view that a methodological study may develop into an epistemological study (cf. Chapter 1), we give detailed bibliographical references on this subject. We are grateful to Professor Skorupski for drawing our attention to the need to make this remark.

[3] Hausman's references are: P. Suppes, Introduction to Logic, 1957; J. Sneed, The Logical Structure of Mathematical Physics, 1971; W. Stegmueller, The Structure and Dynamics of Theories, 1976 and The Structuralist View of Theories, 1979; and R. Giere, 1979 and 1982, Understanding Scientific Reasoning. The latter book contains accounts of the former literature.

[4] According to Hausman (1992) Sneed's work is reported in E. Händler, 1980, "The Logical Structure of Modern Neoclassical Static Micro-economic Equilibrium Theory", Erkenntnis, 15; W. Stegmueller, W. Balzer, W. Spohn, 1982, Philosophy of Economics: Proceedings, Munich, July 1981; D. Hands, 1979, "The Structuralist View of Economic Theories: The Case of General Equilibrium in Particular", Economics and Philosophy, 1; W. Balzer, B. Hamminga, 1989, Philosophy of Economics. Suppes' view probably influenced Debreus's view on economic theory (cf. chapter 7), because Suppes co-edited a book on the mathematisation of the social sciences with Arrow, who has also conducted research with Debreu (cf. chapter 7, note 3).

[5] Earlier on, Clément Colson used the phrase "forme normale de la science" to designate common scientific practice (Cours d'Economie politique, 1924-1933, Livre 1, p.143, quoted and referenced in Zylberberg, 1990). The term tends to be pejorative or at least negative in Kuhn's case, since he considers that normal science is a routine that hinders the development of research. This is not so, it seems, for Colson. According to Desrosieres (1994, p.11) in 1929, Clément Colson was Président du Conseil de la Statistique Générale de France; in addition, he was a Professor at the Ecole Polytechnique.

SUMMARY OF PART THREE

Summary of Part Three

The third part approaches mathematical economics from a standpoint that differs from that of the second. Its aim is to contribute to our understanding of the concept of a 'mathematical economic model'. The concept of a model is important, not only for the methodology of economics, but also for contemporary epistemology. This is because it refers to a defining feature of contemporary science, at the same time it being difficult to obtain a satisfactory methodological or epistemological definition of it.

Chapter 7 illustrates what is today called a mathematical model in economics for the purpose of clarity; also to show that the concept of a model in economics is ambiguous from a methodological and epistemological standpoint. It is shown that models are neither specific to a school of economic thought, nor to the level of abstraction or to the level of generalisation of a piece of knowledge. It also shows that nearly every sub-field of mathematics is used in these models.

Chapter 8 is concerned with the history of the use of the term 'model' in economic literature. It increases the information contained in Zerner(1993) on this topic.

The selected survey of issues of the scientific and critical periodical The Monist from 1890 to 1896 does not reveal any occurrence of the term in economic literature. This may be because these particular issues do not contain many economic articles. However, an idea related to that of a 'model' does occur in an article by Paul Carus and Ernst Mach published in 1890 on the philosophy of mind. It is the idea of a "parallelism" between mental representations of reality and reality itself.

The first occurrences of the term 'model' in economics recorded by Zerner(1993) are dated 1936, and they concern econometrics. Edelberg(1936) uses the English term in *An Econometric model for Production and Distribution*. He neither defines it, nor uses it widely. The term refers to an input-output *equation* which represents reality. Tinbergen also uses the term in 1936, in a speech delivered probably in Dutch concerned with the Dutch economy from a macro-economic perspective. Most probably, the meaning of the term is the same as that defined later by Tinbergen(1937), in an article in English, in which 'model' refers to the schematization of economic life required for the mathematization of an economy into a system of equations.

We recorded the next occurrence of the term in the English-reviewed version of a Russian article by Slutsky(1937), published in 1937 and concerned with business cycles. Slutsky's definition of a model is not fully clear, but it definitely refers to the mathematical entity of a series. According to Slutsky, it means a mathematical representation (among others) of the structure of an empirical process. Different processes with the same structure may have the same mathematical representation. Two occurrences of the English term are recorded in 1944. In a foundational and supplementary article on the methodology of econometrics, Haavelmo(1944) defines "*theoretical* models" (our emphasis) as *a priori* schematizations of the economy. Models in this sense may not be mathematical. They have an economic meaning only if they are considered together with methodological and semantic instructions that connect them to quantified economic data. The second occurrence is in the foundational book on game theory by Morgenstern and Von Neumann(1944). The authors consider that an economic model is the same as a model in physics. They define it as a theoretical construct similar to reality so far as the features relevant to the investigation are concerned.

Chapter 8 is also concerned with the "institutional sources" of the use of the term 'model' in economics (chapter 8, section 2). These sources were individuals and institutions who spread the use of the term in the community of economists because they were influential. Three early users of the term are also institutional sources: Tinbergen, Von Neumann and Morgenstern. In addition, there is the Cowles Commission.

Tinbergen enriched and elaborated the meaning of the term. In his writings in the early 1930's, a 'model' (also referred to as a "theoretical scheme") is a simplification of the economy which is a prerequisite to the mathematization of economic theory. From the

1930's, a 'model' (also referred to as a "theoretical scheme") is a simplification of the economy which is a prerequisite to the mathematization of economic theory. From the 1930's to the 1960's, the meaning of the term evolves and it refers to functional relationships of *a formal-mathematica*l nature exclusively, between symbolic variables. These relationships represent structural relationships between empirical variables. *An economi*c model refers to one possible "content" (or interpretation) of the formal structure. Tinbergen's "economic model" may be considered as a 'linguistic scheme', that is, a set of statements whose syntactic system is mathematics and whose semantic system is both economic statistical data and the economic register of vernacular language. In his books from 1945 onwards, Tinbergen elaborated the meaning of 'model' in two ways. First, 'model' refers not only to *a theoretica*l instrument but also to an instrument for political decision-making. Secondly, 'model' refers to a*n ide*al economic setting, which is the aim of an economic policy.

Von Neumann and Morgenstern's interpretation of the term from 1944 onwards is the same as Tinbergen's in the sense that it is viewed as a theoretical and simplified representation of reality. However, their use differs from his in that they apply it in connection to game theory and in a general meta-scientific epistemological perspective that takes physics as the example to follow. In contrast, Tinbergen uses it in connection to econometrics, from an economic expert's point of view, and in order to describe the tools of economic science. In addition, in the game theorists' view, the economic content of the model refers to 'communal sense', whereas in Tinbergen's case, the content refers both to 'communal sense' and to 'common sense', and hence the content includes physical, countable entities (*cf* chapter 4 , note 2).

In the terminology of the Cowles Commission, which Friedman(1953) reports, the definition of a model is part and parcel of the definition of a validation protocol for hypotheses, which compares them to evidence. A model is a set of hypotheses compatible with (statistical) evidence. It is "identified" or not, depending on whether or not there is only one such hypothesis. This use is approximate to that of Haavelmo. It is also similar to the use of the term in logic (*cf infra*), in the sense that it equally refers to a *true* hypothesis.

In chapter 10, it is discussed whether models in general are a defining feature of contemporary science and philosophical views on this question are mentioned. First, it is shown that the term 'model' has become an acceptable term in many sciences such as social sciences, as well as in physics, from the 1960's onwards (cf. also chapter 7, section 1@1). In mathematical and linguistic logic, it is recorded in German as early as in the mid-1920's, although it only developing after the 1940's. In particular, Von Neumann and the members of the Vienna Circle are amongst the early users of the term. However, the use of the term 'model' in one science is not always consistent with its use in another science. One example of this inconsistency concerns logic and economics, where 'model' tends to refer to a general structure and also to theoretical, hypothetical and formal entities. In logic, it refers to a particular mathematical entity with a given structure, that makes some formal statements come true, ie it "realises" the formal structure.

Secondly, we refer to the philosophers Daniel Hausman, Alain Badiou and Adam Morton because they share an interest in both models and economics. They all agree that models are a defining feature of contemporary science and that the philosophical terminology referring to them is inappropriate. Hausman (1992) is concerned with adapting this terminology in order to *describe* the use of models by economists. He defines a model as a manageable definition of a predicate applicable to actual economic systems. Badiou (1969) is concerned with *assessing* the *scientific* feature of a model. His conclusion is that models are scientific in logic only. He considers that in the other sciences, so-called models are not scientific, they are the epiphenomenon of the misleading epistemological view that science consists in the production of representations true to reality. Morton (1990, 1993) insists on the similarities between models in science and mental frameworks in folk psychology. Consequently, he considers that the models used in science may not have any scientific features. This is the case if the explanations they provide lack "depth" or if it is not sufficiently "contrastive".

Finally, basic methodological issues about the question itself discussed in this chapter are approached.

GENERAL CONCLUSION

We shall now take stock of the enquiries carried out in this thesis. First, we shall expound the results concerning the first topic, that is the mathematization of economic theory achieved by Jevons in his <u>Theory of political Economy</u> and also by Debreu in his <u>Theory of Value</u>. Then the results concerning the history of model building in economics will be presented to the reader. Finally, we will present the results of our experimental use of semiotic analysis. In addition, possible applications of these results are proposed. For this purpose, we analyse some methodological criticisms that have been or may be addressed to us before we suggest further research directions.

The first question addressed in reference to the first topic was whether or not the use of mathematics by Jevons and Debreu in these books can be counted as contributions to mathematics itself. At first glance, and with the methodological and epistemological *proviso* expressed in chapter 5, chapter 6 and below, it appears that they cannot. They are not contributions to mathematics in the sense that the formalism, the mathematical entities and the theorems these authors used had been defined, used and proved before. Also the mathematical symbolism they used is common according both to contemporary and historical standards. However, generally speaking, the time lag between the development of mathematical technique and their application in Economics is shorter in the 20th c. than in the 19th c. (cf. chapter **4**, chapter **5**, chapter **6**, section 3@1, chapter **7**, note 7).

The second question was whether or not authors considered mathematics as a 'conceptual tool' or as an 'empirical tool' for constructing economic theory. It has been shown that Jevons considered that mathematics played both roles. He viewed mathematics as the science of numbers. He considered that positive numbers (e.g. statistical data) indicated reality, and that statistics were a means for the validation of economic hypotheses. He also held that they were concerned with basic laws of thought, and hence of knowledge. Consequently, he considered mathematics at the same time as a component of what is empirical (economic reality), and as a tool to put theories to the test of what is empirical. He also considered them as a 'conceptual tool' to explore theoretical hypotheses. In the Theory of Value, Debreu only considered that mathematics is a 'conceptual tool'. They guarantee that the theory is rigorous and non-contradictory. It may be wrongly considered that Debreu viewed mathematics as an 'empirical tool' in the sense of a tool that gives the essential features of what is empirical. This is because he introduces mathematical formalism in a way that may suggest that his theory rephrases 'communal sense' (cf. **Cheix 1996**, section 4) and empirical knowledge in the sense of knowledge contained in vernacular language. It has been shown that some economists (e.g. Tinbergen) have considered mathematics in this way. However, Debreu's discourse in vernacular language is intentionally an heuristic and pedagogical device to put the theory within the reach of the reader, and it is not an account of economic knowledge.

The third question was whether or not in their mathematical theories the authors contrasted *economic* results with *mathematical* results. It is inappropriate to address the question in reference to Jevons' theory since it does not contain any mathematical proofs. Consequently, it does not contain mathematical results, only statements. In contrast to Jevons', Debreu's theory contains mathematical proofs. In this theory, an economic result may be defined in accordance with Debreu's epistemological views as follows: it is a particular semantic interpretation of a statement expressed with mathematical symbols and obtained in accordance with mathematical laws. Strictly speaking, there are no economic results as such, but economic interpretations of statements in the syntactic system of mathematics, and more precisely of set theory.

The fourth question concerned the arguments the authors put forward to promote the use of mathematics. It was shown that Jevons put forward many arguments. The main arguments are: a) That economic reality is naturally countable; b) That symbolic mathematical statements express the laws of thought; and c) That scientific laws are universal, so that it is highly probable there are economic versions of mathematical laws proved in physics. So far as Debreu is concerned, he considered that the use of mathematical formalism guarantees that the theory is general and that it is non-contradictory.

The connection between the mathematization of economic theory achieved by the neo-classics and mathematical model building is an issue upon which the thesis was also expected to shed some light.
In part three, the first appearance of the term 'model' in the community of economists is recorded in the mid-1930s in econometrics, business cycles and macro-economics. The related idea of a 'scheme' and that of a simplified representation of reality may appear in the late 1920s, and definitely does in the early 1930s. By the 1960s, the term 'model' is still used in these domains,also in game theory and in other sciences. In any case, the idea of a 'model' and that of a 'scheme' are connected to the mathematization of economic knowledge; it referred originally to the formulation of theories and later to their validation as well. There might be earlier occurrences of these terms in economic literature because such ideas occurred in scientific literature in the early 1890s. Having said that, the inquiries carried out in Part Two reveal that the term 'model' hardly occurs in the works we surveyed of two neo-classical economists who contributed to the mathematization of economic theory. The term is recorded in an isolated context in Debreu(1959, chapter 5, note 3, p 89), who uses it in connection with the

history of equilibrium theory but who does not define it. By 'model', Debreu means a mathematical and formal version of economic equilibrium theory. Debreu also referred to the "interpretation" of a formal theory, which is similar to the idea of a model in logic and to what Tinbergen meant by the "economic content" of a mathematical model. With the methodological *proviso* formulated in this thesis, this suggests that the idea of a 'mathematical economic model' did not come from the neo-classical school of economic thought.

This conclusion would be used to assist the formulation of the hypothesis that at least two trends contributed equally to the mathematization of economic knowledge in the 20th c: the neo-classical school school and the development of mathematcal model building in science. One could argue that the neo-classical economists used mathematics to achieve the logical cogency of the theory and also to give it a formalized hypothetico-deductive form. By contrast, one could continue, the contributors to the other trend, such as econometricians, used mathematics as they are alledgedly used in the empirical sciences (such as biology and physics) and engineering. In these sciences, they are used to frame theories that can be compared with quantifiable evidence. If it were true, this hypothesis could be used as an argument against our methodology, and especially against our choice of neo-classical economists as the representatives of contemporary mathematical economics. It is true that this hypothesis is consistent with evidence (cf.

Friday, 1949; Mirowski, 1989, 1991; Zerner, 1993; chapter 1, section 2@2; chapter 4 , section 2@1; chapter 5; chapter 8), that the use of mathematics in physics and engineering influenced the mathematization of economics. However, the information contained in this thesis makes it clear that this hypothesis requires clarification. First, physics influences economics not only because it is an empirical science, but also because it is a theoretical science. For example, it is shown (cf. chapter 8) that the idea of a model used in econometric literature in the 1930s is similar to that of a 'parallelism' used by Mach Ernst (the physicist who challenged the mechanistic approach to science, cf. Israel, 1996), in an article concerned with *theoretical* and *epistemological* issues about physics. Jevons was similarly influenced by physical theories. Secondly, Tinbergen did not develop the idea of a mathematical model in the articles he wrote that show a clear influence of mathematical modelling in physics (e.g. Tinbergen, 1933a), but in articles concerned with macro-economic issues. This qualifies the hypothesis in the sense that even though Tinbergen is a member of the second trend (since he contributed to the definition of the concept of a mathematical model in connection with econometrics), the mathematization of economics he achieved was neither presented in direct connection with mathematical physics (as it is the case with Von Neumann and Morgenstern), nor did it amount to a mere transfer of the theories and the formalisms of mathematical physics to the case of economics. Thirdly, Jevons, who is a member of the first trend, did not contribute exclusively to the mathematization of economic *theory* but also to that of economic data (cf. Stigler, 1994, 1982). Finally, it is probable that between 1871 and the 1930s neo-classical economists, who have not been studied in this thesis, contributed to the development of the idea of a mathematical model. If this were proved true, the hypothesis that two *different* trends as defined above contributed to the mathematization of economic knowledge would lose its rationale.

Finally, it was expected that the experimental use of semiotic analysis (cf. chapter **5**, section 3@2; chapter **6**, section 3@3) would bring new insights into mathematical economics. For this purpose, a text in mathematical economics was defined as a text of the economic corpus that includes both the semiotic system of mathematics and that of vernacular language. In this view, a text in mathematical economics can be understood at different levels, also called 'isotopies'. A text consists of a set of 'expressions' (graphic marks), with 'contents' (what makes the expression meaningful, that is, ideally, all the relationships - grammatical, lexical, phonological- between the expressions of the text). An 'isotopy' is the redundancy of expressions or contents in a text that produces a level of interpretation of a text.
In both texts, a 'notational and graphic isotopy' consists of expressions that are abbreviations,

mathematical symbols or equations. The 'economic isotopy' consists of the expressions that belong to the econmic register in contemporary vernacular English. The 'mathematical isotopy' consists of expressions and contents that are used in contemporary mathematics (in Jevons' case), or that are openly given a mathematical content (in Debreu's case). In addition, there is a 'mechanical isotopy' in Jevons' text, which is defined by the expressions and the contents that Jevons quotes from the corpus of mechanics. In Debreu's text, the 'theoretical isotopy' consists of the expressions in italics that Debreu considers to be the formal theory proper.

The analysis shows that the notational isotopy establishes a correspondence between the economic isotopy and the mechanical isotopy (in Jevons' case) or the mathematical isotopy (in Debreu's case). In Jevons' case, the correspondence is based on mathematical *formalism* (equations), whereas in Debreu's case it is based on the regular use of mathematical *symbols*. So far as economic theoretical inferences are concerned, they are performed at the economic isotopy level with the help of mathematical symbols in Jevons' case, and at the mathematical isotopy level with the help of mathematical formalism as a whole in Debreu's case.

In both cases, the analysis shows that strict correspondence (interpretation) rules being established between isotopies do not eradicate inconsistencies between them. For example, the study of the correspondence between the 'mechanical isotopy' and the 'economic isotopy' reveals a discrepancy between what Jevons *writes* about his use of differentials in economic theory, and his actual *application* of them. One could explain this discrepency by saying that his mathematical knowledge was limited. We shall interpret this contradiction as a sign both that the use he makes of infinitesimals and differentials conflicts with his finitist or materialist metaphysical views (at the economic isotopy level) (cf.chapter **5**, section 1@2) and also that the use of symbolism offers a way to overcome these contradictions so as to pursue the argument and the unification of different parts of economic theory. In addition, this contradiction may be attributed to the other important semiotic function of the text, namely its pedagogical and vulgarisation function (cf. chapter **5**, section 1@2). Inconsistencies between isotopies also occur in the Theory of Value, between the economic semantics of the theory and its formal aspect. We consider that these inconsistencies do not challenge the contributions of Jevons and Debreu to mathematical economics. On the contrary, they prove that they are essentially theoretical (cf.**Cheix 1996** , notes 3 and 11), and they show what the conceptual *tours de force* achieved by these authors consist in.

Having presented the reader with the results of this thesis, we shall formulate the methodological *proviso* that must be considered if these results are to be applied as indicated to conclude this thesis.

The first criticism of our methodology was addressed to us by Professor Skorupski. It is that the attempts at defining 'models' we have referred to, and also our own are of a lexical and taxonomical nature only and not of a philosophical nature. Given that we hold the view on language that cognitive and semantic contents of words are interrelated (cf. chapter 6, note 7), we also hold the view that an epistemological study may involve a terminological study.

A second criticism that could be addressed to us concerns the thesis' inter-disciplinary nature. It implies that this study partly rests on the results of research in other disciplines such as history and linguistics (e.g. on Perrot, 1992, cf. chapter 6, note 11; on Eco, 1968, 1984, and Herreman, 1993b). Interdisciplinarity also implies that we use the ideas of scientific fields other than economics and mathematics in a way less thorough than experts in these fields. This is not a drawback provided that secondary sources are used in a critical way (e.g. chapter 1, section 2) and that conclusions remain of a hypothetical nature and remind the reader that they rely on other studies.

The third criticism that could be addressed to us is that the results of this thesis, and also our external approach to history conflict with our taking an intra-scientific approach to epistemological and methodological issues (cf. Cheix 1996 and chapter 2). We have shown that the methods of a science are often borrowed from another science, and also that some scientific practices (e.g. model building) appear to be a defining feature of a historical era and not of a particular science. This may also be an argument for meta-methodology (cf. Cheix 1996 , note 11). To answer this criticism, we would follow a line of argument sketched in Cheix 1996 (note 11). This is that contemporary scientific methods resemble one another, not because there is an underlying (logical or intellectual) structure specific to *scientific* inquiry, but because today scientific information (including scientific methods) is widely and quickly disseminated. In addition, it has been shown that the function of similar scientific tools (e.g. mathematical models) differs from one discipline to the other (e.g. economics vs. logic).

A fourth criticism could challenge our choice to limit our comments to strictly methodological issues and to exclude both praxeological and epistemological issues (e.g. Cheix 1996 , note 9 and note 15; chapter **4** , section 2@2; chapter **6**, note 5, note 7 and note 23; chapter **7**, section 2; chapter **8**, section 1@2; chapter **9** , section 1@2).

For example, it could be argued with some justification that the assessment of the methods used in economics varies significantly, depending on whether economic knowledge is considered as a practice (e.g. the art of producing goods and allocating wealth) or as a theoretical science. In the first case, it can be considered that theoretical and practical protocols are valid because custom and habits have proved their success. In the second case (e.g. if economics is considered as an explanatory science), the success of a method is not enough to make it valid. There must be an explanation connecting the cause (the method) to the effect (success).

Similarly, it could be pointed out that epistemological and methodological issues cannot be separated in the manner we have applied. For example, it could be argued that our conclusion that Jevons and Debreu did not contribute to mathematics (cf. *supra*) depends on the view, on mathematical ontology, that it is not necessary for mathematical entities to have a concrete and physical interpretation in order to exist. A *contrario*, if one holds the view that mathematical entities exist only if they have a concrete interpretation, one can support the view that Jevons and Debreu contributed to mathematics. Jevons' interpretation of infinitesimals as degrees of utility made their existence more probable. Similarly, it can be argued that Debreu increased the validity of the fixed point theorem (which apparently had only a formal and non-constructive proof), by showing that it meets theoretical expectations that arose concerning the study of an economy: the theorem provides equilibrium analysis with the result this theory had anticipated.

Another example that could be mentioned to show that it is impossible to separate methodological from epistemological issues, is the debate concerning the emptiness of mathematical formalism (cf. chapter 6, section 2). It could be argued that it amounts to the following question: what do mathematical statements *refer* to?, rather than to a question about mathematical economics in particular. The emptiness of mathematical statements is indeed an important issue in 20th c literature in Philosophy of Mathematics. Consequently, the argument that opposes economists praising mathematics because its terms have a fixed meaning (e.g. Jevons, cf. chapter **5**) and also the deductive structure of axiomatized economic theories and their rigor (e. g. Koopmans, 1957; Debreu, 1959, 1986) against those who hold the view that these theories may still not be meaningful (e. g. Perroux 1991 and Van Parijs, 1990, p. 13) echoes a similar general argument about mathematics. For example, Ajdukiewicz (1960) and more recently Thom (1993) held the view that rigorous mathematics could be synonymous with insignificant mathematics. To conclude, this fourth criticism is perfectly relevant. However, there must be precise limits to research studies, which justifies our standpoint.

Some aspects of our application of semiotics can be criticized. We are grateful to Professor Petitot for drawing our attention to the hypothesis (cf. **Cheix 1996** , section 4, first philosophical landmark and chapter 2, section **2**, especially note **9**), that the scientific nature of knowledge is related to its being written. It could be argued that the defining feature of scientific thought occurs first and foremost in oral communication between scientists. If we had held this view, semiotic analysis could still have been used, but the semiotic corpus we would have referred to would have been the literature specialized in the study of speech.

The issue of the oral vs. written nature of scientific knowledge is relatively important, as it is implicit in many views which contrast mathematical economics with literary economics. For example, Jevons(1871) held the view that literary economics was already mathematical (cf. chapter 5, section 1@1) and that his contribution to mathematical economics was to express it with mathematical symbols. Similarly Koopmans(1957, p177) viewed economic theory as a succession of mathematical models, connected to each other by the rules of deductive logic, just as statements expressed in literary economics.:

> "The postulational structure of the mathematical tool parallels that of the substantive theory to be constructed, and the two are studied and apprehended simultaneously. The welcome result is that 'mathematical' and 'literary' economics are moving closer to each other. They meet on the ground of a common requirement for good hard thought from explicit basic postulates, rather than for manipulative skills in calculus, differential equations, or determinants".

These views neglect an important point which Gradies(1993) emphasizes (cf. summary and conclusion of Part 2). It is that the logical structure of a reasoning (or at least the cognitive and psychological content of such a structure) depends on the syntactic structure of the semiotic system in which it is expressed. In particular, if it were proved that the scientific feature of mathematics was relevant to oral communication, then the 'translation' from literary to mathematical economics could not be considered as a mere rephrasing. It should be considered instead that this 'translation' involves a fundamental change of the syntax of the semiotic system in which economic theory is expressed, and that this might imply an important change in the logical structure of the theory such that, contrary to the view held by Jevons (cf. chapter 5, section 1@2), symbolic and formal logic cannot always be consistant with common sense as expressed in vernacular written languages. This syntactic change may be the cause of the difficulties pointed out by Mahieu(1989) about the axiomatization of economic theory (cf. chapter 6, section 2@2, note 8).

As suggested, the research undertaken for this thesis may be carried further with a stronger emphasis on the epistemological or alternatively on the sociological dimension of the issues approached (cf. chapter 2, section 1@1; chapter 1, section 2@1; chapter 3, note 2; chapter 6, section 2@2; chapter 9, section 2@2). However, we shall mention further research directions only in close connection with the results obtained and the methodology of economics.

First, the enquiry into the historical origin of mathematical economics can be continued. It has been shown that there is a gap in the history of the term 'model' between 1937 and when it occurs in game theory in 1944. A likely cause of this is the interruption of war in Europe in 1939-1945. It was also suggested that the term might occur in 1927 in

Russian economic literature (cf. chapter 7, note 4; chapter 8, section 1@2). Further inquiry on this topic should explore: (a) the Second World War period and especially operational research, because it is one origin of game theory; (b) Soviet mathematical economics in general, with a priority on the 1920's; (c) Mach Ernst's idea of a "parallelism" between mental and physical structures and also its influence on the scientific community (in particular in connection to attacks against mechanistic views on sciences) dating from 1890 at least; (d) the works of other neo-classical economists; (e) the literature studied by Zylberberg (1990)(cf. chapter I, note 4), including the tradition of ingeneer-economists, their use of electrical models (cf. Allen, 1963, p. 281) and of ideas from computing science.

Secondly, the use of semiotic analysis could be continued in a more systematic manner since it was experimental and partial (e.g. chapter 2, section 2@2, note 14). Basic methodological issues that this would imply have been mentioned. Also it was kindly suggested to us by Professor Desclés that this analysis could be computerised. A consequence would be the increase of both the corpus of mathematical economics to be analysed and the validity of the results. This would imply focusing more on the study of expressions with a syntactic or grammatical function than was the case in this thesis. In our study, we emphasized the role of notations in connecting the different levels of interpretation of a text, and neglected the role of syntactic and grammatical items.

We suggest that the following issues could be explored using this methodology: (1) Do syntactic and grammatical units play the same role in mathematical economics and in mathematics? For example, Herreman(1993b) describes the role played by these units in attributing topological contents to expressions with no mathematical content. (2) Is the concept of a 'borrowing' from one science to another identical with the borrowing of a syntactic structure or of a semantic field, or else of a semiotic function(cf. chapter 2, section 1)? The use of mathematics in economics is often considered as just such a 'borrowing' and it is understood that this is illegitimate (cf. chapter 4, section 2@1, chapter 5, chapter 9). But borrowings are common in science, even if it is difficult to justify them and to explain why some are successful and others are not. Many methodologists, such as Durkheim (1898), have attempted to clarify this issue. (3) Are contradictions between different levels of interpretation of theoretical texts in mathematical economics (cf. chapter 6, section 3) an impediment to computerized analysis of economic surveys, such as the analysis provided by Planès *et al*(1993)? This question arises first because this computerized analysis is based on there being different semantic levels in a survey. Secondly, the concept of demand (and that of offer) is central in this analysis and we have shown that to some degree, it was ambiguous in Debreu's version of neo-classical theory.

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