Department of Economics and Statistics "Cognetti de Martiis"
Campus Luigi Einaudi, Lungo Dora Siena 100/A, 10153 Torino (Italy)
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ECONOMIC THEORIES IN COMPETITION. A NEW NARRATIVE OF THE DEBATE ON GENERAL ECONOMIC EQUILIBRIUM THEORY IN THE 1930S

ROBERTO MARCHIONATTI AND FIORENZO MORNATI

CENTRO DI STUDI SULLA STORIA E I METODI DELL'ECONOMIA POLITICA "CLAUDIO NAPOLEONI"

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Economic Theories in Competition. A New Narrative of the Debate on General Economic Equilibrium Theory in the 1930s

Roberto Marchionatti and Fiorenzo Mornati[1]

Abstract

The paper deals with the debate on the General Economic Equilibrium in the 1930s in Vienna and at the London School of Economics and offers an interpretation of it different from that of the traditional narratives. It interprets the debate as a renewed confrontation between the two different classical methodological paths of research in GEE, the Paretian and the Walrasian ones. What emerges from this examination is a picture of different approaches and theories in competition, in particular on the issue of the relationship between theory and the real world. This was the fundamental issue at stake. Herein lies also the interest in those distant controversies for the current debate in economics.

Keywords: Hicks, Wald, Von Neumann, General Economic Equilibrium, Mathematical formalism

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[1] Dipartimento di Economia e Statistica "Cognetti de Martiis", Università di Torino (Campus Luigi Einaudi, Lungo Dora Siena 100/A, 10153 Torino), roberto.marchionatti@unito.it.

[2] Dipartimento di Economia e Statistica "Cognetti de Martiis", Università di Torino (Campus Luigi Einaudi, Lungo Dora Siena 100/A, 10153 Torino), fiorenzo.mornati@unito.it.

1. Introduction

General Economic Equilibrium theory (hereafter GEE) is considered the fundamental framework for theoretical discourse in mainstream economics. In spite of its well-known limitations – the serious problems connected with unicity and stability of equilibrium – GEE has maintained its theoretical leadership in microeconomics; moreover, in recent decades it has widened its influence by colonizing also macroeconomics. In this context, when speaking of GEE, the reference is to its neo-Walrasian version.

Founded in Karl Menger's Viennese *Mathematische Kolloquium* in the 1930s by Abraham Wald and John Von Neumann, then developed in the 1950s and 1960s by many authors, in particular Gerard Debreu and Kenneth Arrow, neo-Walrasian GEE is usually considered the rigorous development of Walras's and Pareto's classical theories of general equilibrium. The narrative histories of GEE substantially support this view[1]: above all, they emphasise the advance achieved from the analytical point of view, in regard to the unsolved formal problems of the predecessors. These narratives certainly improved the knowledge in a field in which previous "narrative histories … rarely proceed beyond Walras and Pareto" (Weintraub 1983, p. 2); at the same time, they lack in the analysis of the extension and significance of the theoretical debate in that crucial decade of the 1930s. Concentration on the contributions of Menger's *Kolloquium* seems to represent, from the historical point of view, a narrow perspective: in fact, on the one hand, it underestimates Hicks's contribution at the London School of Economics (hereafter LSE), considering it old-fashioned –, in part including in this judgement also Samuelson's contribution at Harvard. Moreover, it ignores the fundamental epistemological differences between the *Kries* and the *Mathematische Kolloquium* in Vienna. At that time these controversies were important, even if in the subsequent decades they weakened and their deep meaning vanished, in the perceptions of the scholars, in a fragile synthesis hiding differences, at the theoretical and epistemological level, and emphasizing the emergence of a "formalist paradigm" (Blaug 2003).

This paper assumes a perspective far from the traditional narratives by comparing the epistemologically different research programmes pursued in Vienna and at the LSE. Focusing on the three centers where the theoretical issues of the GEE were discussed – LSE, the *Wiener Kries* and the *Mathematische Kolloquium* – it interprets these discussions as a renewed confrontation between the two different classical methodological paths of research in GEE, the Paretian and the Walrasian ones. It considers the dissent that arose in Vienna and emphasises Wald's and von Neumann's foundation of the formalist perspective in economics[2] as the result, not of a cumulative process of knowledge and refinement of mathematical tools, but of an 'epistemological break' regarding the nature and method of economics. What emerges from this examination is a picture of different conceptions of what a scientific theory must be, and of theories in competition: another example of that typical phenomenon of "the years of the high theory" (Shackle 1967).

2. Mathematical Economics at the Beginning of the Twentieth Century and Pareto's Legacy

In 1900 the fourth edition, the so-called definitive edition, of Walras's *Elements* was published. However, in the first fifteen years of the twentieth century, it was Vilfredo Pareto who made the major theoretical contributions to the developing field of mathematical economics, and Walrasian GEE in particular. His work, from the *Considerazioni sui principi fondamentali dell'economia pura* (2007 [1892-3]) to the *Manuale di Economia Politica* (2006 [1906][1909, French translation], carried the pure economic theory that emerged in the marginalist revolution of the 1870s to its highest stage of development for that time (see Marchionatti-Gambino 1997, Marchionatti-Mornati 2003). At that point, the mathematical system of pure static economics seemed to the contemporary mathematical economists to require only some work for its completion.

As Schumpeter (1954, p.861) wrote, Pareto's theory was based on Walras's work, and at the same time it deserved to be considered "a new creation". But Pareto's theory was more than a development of Walras's theory on the analytical level. Pareto traced the methodological outlines of an economic science profoundly different from that of Walras. Walras maintained that pure economics is "a physical-mathematical science like mechanics" that uses the "rational method" and not the "experimental method" (Walras 1954 [1900], p. 71). Therefore theory is not confirmed by experience but by the structure of theorems and proofs. By contrast, Pareto considered pure economics to be a natural science founded on facts, a science that uses the experimental method peculiar to the natural sciences. Pareto thus initiated a research program in which the definition of assumptions and the empirical verification of theories were fundamental. Pareto's dissent with Walras was clear from his first essays, where he affirmed that the tendency in the mathematical school to subordinate experience to theories was its greatest danger. He criticized Walras' tendency to lead science on a metaphysical path, where reasoning dominates experience, as he wrote in his *Considerazioni*. Experience and observation are the correct methods of reasoning, Pareto maintained. In this context the mathematical method allows for a higher degree of rigour in demonstration, and it enables the treatment of problems far more complicated than those generally solved by ordinary logic. On the other hand, he stressed that economists must use mathematics with caution: the greater rigour of the proof may be only apparent because of the uncertainty of the premises, so that the central theoretical question was the validity of the premises upon which the theorems that yield the conclusions are built.[3]

Differently from Walras, whose insistence on the rational over the experimental method alienated him from the scientific community, Pareto's work received explicitly positive comments from several scientists and mathematicians in Europe and the United States (see Marchionatti 2004). However, the great Walrasian expectations of the 1870s – the dream of a social astronomy – had greatly diminished in the first fifteen years of the twentieth century. It was generally recognized that the field of mathematical economics is restricted to static equilibrium. In this limited field, the argument ran, the tools of mathematical expression provide a general way to expound the relationship of economic interdependence in a stationary state and are able to specify the conditions and limits of theorems and to prove them rigorously. The discoveries of mathematical economics must not, however, blind economists to the fact that their theories are but static equilibrium theories. Dynamic changes are not taken into account by these theories.[4] Moreover, and this is generally considered to be the main defect of the general equilibrium theory, they are of extreme abstractness. This abstractness makes it difficult to apply the theory's conclusions to the explanation of actual facts. The problem of defining the properties of mathematical functions was considered particularly serious because mathematically 'well behaved' functions hardly ever characterize real cases and their indeterminate nature was considered to be the main obstacle to the application of general models to particular cases. It was emphasised (see for example the then well-known book by Zawadski) the great difference between the application of mathematics to economics and its application to mechanics.[5] It was believed that, whilst in mechanics it is always possible to pass from general formulae to actual phenomena, gradually specifying the characteristics of the functions in these formulae, this is not possible in economics. That Pareto was very aware in the last part of his life of these difficulties, and of economic theory's limited ability to explain real economic phenomena, is evidenced by his address, the *Discorso del Giubileo* (1917) and his increasing interest in sociology.

To summarize, the main issues under discussion in the early years of the new century, in the mathematical economists' community, were the theory's excessive abstraction, the unreality of its assumptions and models, and the difficulty of explaining real phenomena, rather than its formal aspects. These economists seemed generally not share the preoccupation with the formal establishment of equilibrium that was to dominate mathematical economics later. They were chiefly interested in the problems connected with the relationship between mathematical expression and experimental reality. In their view, the equilibrium model must be a representation of a real state of the economy, and the main concern of the researcher is not to examine the conditions that must be satisfied in order to be certain that a particular equilibrium exists. This explains their emphasis on the necessity of 'not too unrealistic' assumptions.

In the 1920s the theoretical framework of GEE was not modified. The works by the Paretians and Walrasians consisted essentially in presentations of a Walras-Pareto system in textbooks, rather than important creative contributions[6]. In this sense the 1920s mark a decline of the Walras-Pareto perspective. The work of the 'grand Paretians' of the 1930s – Hicks first of all – was a sign of the resurrection of the neo-classical general equilibrium theory, together with the formal contributions of Wald and von Neumann in Vienna. However, these contributions, in particular those of the Vienneses, were profoundly different from those of the classical period in regard to method and formal analysis.

3. The Neo-Paretian Perspective in London: Hicks's Contribution

3.1. Pareto at the LSE

Paretian economics had a dominant influence on the development of neoclassical microeconomics in the English-speaking world in the 1930s (Marchionatti 2006). A new generation of economists – among them John Hicks, R.G.D. Allen, Paul Samuelson, Abba Lerner, and Oskar Lange – concentrated on some of the main themes outlined by Vilfredo Pareto's Manuale, i.e. the analysis of individual behaviour, market efficiency and welfare economics, using classical differentiable calculus. The story begins at the London School of Economics. In 1929 John Hicks, on Lionel Robbins's request, began to lecture on Walras's and Pareto's general economic equilibrium theories (see Hicks 1973, Hamouda 1993). A few years later, he and Allen wrote their famous article "A Reconsideration of the Theory of Value" (1934), based on the use of the concept of Pareto's indifference curves, where they introduced the concepts of income and substitution effects, as well as the principle of marginal substitution.[7] One year before, in 1933, some LSE graduate students had founded the *Review of Economic Studies*, a journal which contributed greatly to acquaintance with Pareto in the UK and published many articles on Paretian themes.[8] Pareto's work on pure theory was considered 'a beginning' to be developed along the new lines laid down by Slutsky, Allen and Hicks, implicitly reintroducing the successive approximation approach which the late Pareto *de facto* abandoned. Indeed, the achievements in microeconomic theory at LSE between 1934 and 1939 (the year of Hicks's *Value and Capital*) were noteworthy. Hicks's book was the most ambitious outcome of this neo-Paretian approach.

3.2. Hicks's Value and Capital

Value and Capital is a work on theoretical economics based on the idea that the problem of economic theory at that time was the construction of "a technique for studying the interrelations of markets" (p. 2) assuming as a starting point Walras's and Pareto's work (and Wicksell's for his consideration of the dynamic problem of capital) : "our work", Hicks writes in the introduction, "is bound to be in their [Walras's and Pareto's] tradition, and to be a continuation of theirs" (ibid.).

The book begins by presenting the new theory of subjective value based on the concept of ordinal utility and then applies the result to rework the GEE static analysis of Walras and Pareto. In the first two sections Hicks grounds the static theory of general economic equilibrium of perfect competition on a theory of demand which is a development of Pareto's theory of choice and a theory of production which is an adaptation of demand theory to the study of firm behaviour. Hicks focuses on the static theory of exchange (essentially presented in chapters I to IV and in the *Mathematical Appendix* of the book), because, according to himself, in both the static equilibrium and the dynamic equilibrium of production "almost exactly the same questions" of the exchange theory come up: "this is why the theory of exchange is an essential part of the study of the economic system in general" (p. 77).

Hicks starts from the equilibrium of the consumer in conditions of perfect competition. The rule for equilibrium is that the marginal rate of substitution between any two goods must be equal to their price ratio. The condition for stability of the equilibrium is that $du = 0$ and $d^2u < 0$, i.e. in order that u (the utility function) is "a true maximum" (p. 305-6). Hicks emphasizes that these conditions do not depend on the existence of a particular utility function: they can be deduced from any arbitrary utility function $\varphi(u)$ provided that $\varphi'(u) > 0$. After examining the effect on demand of an increase in income and the effect of a change in price with constant income, Hicks defines the well known equation, originally formulated by Slutsky, which he regarded as "the Fundamental Equation of Value Theory", which divides the effect of any price change on demand into two effects : the income effect and the substitution effect.

Hicks then considers the existence of the equilibrium of exchange in a world where N individuals exist, "bringing to the market various quantities of n goods, and exchanging them under conditions of perfect competition" (p. 314). After showing that there are $n - 1$ equations to determine the n-1 prices, Hicks maintains that the equilibrium exists. According to Hicks, having shown, *à la* Walras, "the mechanism of the interrelation of markets.... was a great achievement", but of a certain sterility, because it is not clear how this mechanism works, i.e. the laws of change are not discussed: the mechanism does not explain, Hicks writes, "what would happen if tastes and resources changed" (p. 61). But, he believes, "with the technique now at our disposal [that is, the new theory of demand], we can make a similar investigation for the general case" (ibid.) and counter most of the accusation of sterility brought against the General Equilibrium. The laws of change of the price system – writes Hicks – "like the laws of individual demand, have to be derived from stability conditions" (p. 62). First, one must examine what conditions are necessary in order that a given equilibrium position will be stable ; then one must make "an assumption of regularity, that position in the neighbourhood of the equilibrium position will be stable also" (ibid.). According to Hicks, the general equilibrium is stable if a slight movement away from the equilibrium position "should set up forces tending to restore equilibrium". He calls the difference between the demand and supply at any price "the excess demand" for a good. He then reformulates the equilibrium condition of a market as the condition that the excess demand should be zero, and the condition of stability of equilibrium as the condition that the excess demand should increase when the price falls and decreases when the price increases. Taking the supply of any goods as constant, in order for equilibrium to be stable the market demand of a good must vary in the opposite direction to its price, and the other prices must vary so as to maintain the other markets in equilibrium. The stability conditions enable us to study the variation of prices following a change in consumer preferences towards a certain good. Firstly, the stability conditions show that necessary to induce the owner of a certain good to sell it, in a sufficient quantity, to the potential buyer is an increase of the price of the good such to induce a fall in his excess demand, i.e. an increase of his supply. As regards the other goods, assuming the existence of two goods, we obtain, for the equilibrium of the market of the other good, that if the good is normal, the ratio between the two prices increases if the two goods are substitutes and decreases if they are complementary.

This reworking is followed by the "foundations of dynamic economics", where Hicks discusses the problem of intertemporal equilibrium and presents his model of temporary equilibrium following the path laid down by Hayek (1933). Finally, it deals with the working of a dynamic system in order to create a theory of the economic process over time. The dynamic parts of the book – certainly a truly innovative contribution[9]– are, however, not organically connected with the previous parts, due primarily to Hicks's subjective requirement of describing the dynamics of real economies as well as the statics. In fact, in general, Hicks' aim was to respond to the criticism of static and highly abstract representation of the economy, and the accusation of sterility brought against the Lausanne theory by Marshallians in England. Therefore, the aim of his theoretical project was to find a way out of the *impasse* into which the GEE theory had become trapped in its classical era, while at the same time maintaining continuity with that work. In this project, the reworking of that theory, consistently with the LSE perspective, was fundamental because it was considered to represent the foundation of the economic laws operating in reality.

3.3. Value and Capital*: "A revolutionary book" or "an old-fashioned book"?*

At the time of its publication *Value and Capital* was widely considered a work reconciling the theory of equilibrium and the theory of real dynamics, but in any case a revolutionary book in the Paretian perspective (for a synthesis of the reviews see Young 1991 and 2008). It certainly inspired the work of English-speaking economists, in particular Paul Samuelson, who in the *Preface* to the *Foundations of Economic Analysis* recognised the similarity of points of view with *Value and Capital* and, fifty year after, confirmed that Hicks was one of the few economists of the 1930s who received his “attention” (Samuelson 1998, p. 1381).[10]

On the other hand, a highly critical examination of the book was conducted in 1941 by Oskar Morgenstern, at that time professor at Princeton. Morgenstern’s criticism focused on the formal problems of the book[11]: he accused Hicks of lacking rigour and of being outdated. The main point raised by Morgenstern (1941) was "a fundamental issue … which has loomed large in the writings of mathematical economists ever since Walras": "it is the question as to whether the determinateness of any given economic system … is assured when the number of unknowns involved equals the number of equations that can be set up" (p. 368). Establishing such a system, Morgenstern maintained, would be a major achievement, and the main issue was whether or not the system of equations embodied all the assumptions that the economist has to include. But, he observed, relatively scant attention was paid to this question in the mathematical treatment of economic theory: the mathematical economists of the classic era (from Walras to Fisher, Cassel and Pareto) had failed, Morgenstern maintained, "even to see the task that was before them", and, he declared, "Professor Hicks has to be added to this list" (p. 369). Only very recently, Morgenstern noted, had "an important step forwards … been made, due exclusively to mathematicians and not to economists" (p. 369). He was referring to the work of John von Neumann and Abraham Wald in Vienna, which Hicks had ignored. Needless to say, his criticism reflected the influence of von Neumann.[12]

4. Neo-Paretian and Neo-Walrasian Perspectives in Vienna

4.1. Premise: The Viennese Debate

Between the two world wars economic discussion was lively in Vienna, not only in the Austrian School's seminars run by Hans Mayer and Ludwig von Mises, but also in the two philosophical and scientific circles: the *Wiener Kreis*, created at the beginning of the 1920s by the physicist and philosopher Moritz Schlick; and the *Mathematische Kolloquium* founded in 1928 and conducted by the mathematician Karl Menger, and attended mainly by logicians and mathematicians. Although economic discussion was only a small part of the debate in these two circles, it was a stage of crucial importance in the relationship among economic theory, philosophy and mathematics in the twentieth century. Traditionally, the literature on the history of economic ideas does not distinguish between the approaches to economics of the two circles, associating them as expressions of the new path of scientific discourse. More importantly, there has been general consensus that there was some sort of continuity and intellectual interchange between the *Kreis* and the *Kolloquium* (Gilles 1981, Weintraub 1983, 1985, 2002, Ingrao and Israel 1987, Punzo 1989 and 1991, Golland 1993, Mutoh 2003). In fact, there is evidence that the two Viennese circles came to express different conceptions of the scientific discourse (Becchio and Marchionatti 2007). The *Kreis*'s members adopted Bertrand Russell's logicism and the experimental approach to reality taken by physics. By contrast, the *Kolloquium*'s members adhered to David Hilbert's mathematical formalism and adopted a deductive and highly formalised method. The mathematicians of the *Kolloquium* rejected the *Kreis*'s 'physicalism' and tended to downplay the importance of the verificationist paradigm. These different philosophical conceptions were reflected in different conceptions of economic theory: a conception of economics as empirical science supported by the *Wiener Kreis*; and a conception of economics as a mathematical science supported by the *Mathematische Kolloquium*.

4.2. Economics as an Empirical Science: The Neo-Paretian Perspective in the Wiener Kreis

Logical empiricism was founded by the *Wiener Kreis*. This new empiricism shared with its predecessor the assumption that knowledge starts from the observation of empirical data. However, the new empiricists added that the statements made by empirical sciences are connected and ordered by a new logical analysis (Neurath 1973): the discovery of new statements (laws or theorems) is the aim of all scientific research. But they rejected all logically or empirically unverifiable statements as meaningless. This new view of science was the philosophical foundation of the *Kreis*'s scientific vision as described in the *Manifesto* written by Otto Neurath together with Hans Hahn and Rudoph Carnap (Carnap, Hahn and Neurath 1929). This vision was developed into the subsequent concept of unified science (*Einheitswissenshaft*), whose language was so-called physicalism – i.e. the application of the language of physics to the other sciences.

In the *Manifesto*, economics was placed among the five branches of science that "must conduct an epistemological examination of its foundations, a logical analysis of its concepts" (Neurath, 1973, p. 315) in order to purge it of metaphysical residuals. To be noted is that this was also a crucial point in Pareto's research program. In fact, Pareto's thought had an influential role in the *Kries*: it was known particularly through Neurath's writings; but what should be emphasized is not only that there was explicit reading of Pareto's work in the *Kreis*, but also that it anticipated the *Kreis*'s theoretical conception of the scientific nature of economics. Both the members of the *Kreis* and Pareto pursued the goal of creating a unified science. Both stressed the importance of the verificationist paradigm. The *Kreis*'s relationship with Pareto is not surprising if we consider Pareto's attention to the development of science and his increasing agreement with the ideas put forward by Poincaré – one of the authors at the center of the initial reflections of the *Kreis* – at the beginning of the twentieth century (Marchionatti and Gambino 1997).

We can maintain that the disagreement between the *Wiener Kreis* and the *Mathematische Kolloquium* over the economic discourse is analogous to the old classical disagreement between Walras and Pareto over the nature and method of political economy referred to in Section 2 above. This is what makes Pareto, from the epistemological point of view, a precursor of the Viennese neo-positivism in economics.

Another important connection between Pareto and the *Kries* must be emphasized. In a paper presented at a conference held in Paris in 1935, and whose purpose was to submit the program of the *Encyclopaedia* to the scientific community, the French economist, engineer and statistician Robert Gibrat (Gibrat 1936) explicitly associated the *Kries*'s program with the recently-born econometric movement. Econometrics emerged as one of the 'modern models' that conceived economic theory as the field of application of exact logic. It adopted the methods of natural science that would assure the clarity and rigor necessary for theory and empirical research in economics, and the epistemological paradigm of verificationism which originated in the *Kreis* circle. These modern models were explicitly conceived as applied developments of Walras's and Pareto's mathematical economics, as Tinbergen (1949) pointed out. The early econometricians continued to give fundamental importance to Pareto's main methodological problems – i.e. the issues of the excessive abstraction of pure economics, the realism of the key assumptions and models, and the relationship between the mathematical formulation of the models and experimental reality. By contrast, a totally different epistemological approach was adopted by 'the economists' of the *Mathematische Kolloquium*.

4.3. Economics as a Mathematical (Formal) Science: The Neo-Walrasian Perspective in the Mathematische Kolloquium

4.3.1. Premise

Karl Menger had been a regular participant in the *Kries*, but he soon had reservations about developments in the *Kries*'s original empiricism (Menger 1994; Kass 1996). The publication of the *Manifesto* left him skeptical, to the point that he called it "rather superficial" (Menger 1974, p. 114). He consequently founded the *Matematische Kolloquium* in 1928. In the *Kolloquium*, studies on the contemporary development of geometry and logics as well as "studies concerning the new applications of exact sciences to problems of sociological character" were carried out (Menger 1935, p. 327). In a note of this article he added: "for example on the existence and uniqueness of solutions for the production equations in mathematical economics".

"The real link between Walras … and the nascent developments" (Weintraub 1983, p. 6) was the banker and economist Karl Schlesinger. He addressed the issue of the existence of economically meaningful (positive) solutions in the Walrasian model in a short paper published in 1935 in the *Ergbenisse eines Mathematischen Kolloquium* and previously presented, at Menger's invitation, to the *Kolloquium*. The model considered by Schlesinger was the so-called Walras-Cassel system based on Gustav Cassel's simplified reformulation of the Walrasian general economic equilibrium (Cassel 1899 and 1918).[13] Schlesinger (1935), like Stackelberg (1933), Neisser (1932) and Zeuthen (1932) before him, emphasized that the equality between the number of equations and the number of unknowns does not necessarily mean that the system possesses positive solutions in prices. He reformulated Cassel's system in terms of inequalities, but without going on to its mathematical solution. Schlesinger's paper opened the way for Abraham Wald's work[14]. In a series of trail-blazing papers (1935 and 1936) Wald demonstrated the existence of an equilibrium for the Walras-Cassel system.

From a methodological point of view, Wald's work took as its premise Menger's conception of meta-economics – a meta-theory corresponds to the logical relations between the statements of a theory (Menger 1936; see also Becchio 2009). Menger (1994) claimed that "from the point of view of methodology", his 1936 paper was "the first instance in economics of a *clear separation* between the question of logical interrelations among various propositions and the question of empirical validity" (p. 300). According to him, it was the key point needed to transform economics into a science. This "clear separation" between the question of logic and the question of empirical validity, which Schumpeter (1954) described as "a shining example of the general tendency towards increased rigor that is an important characteristic of the economics of our own period" (p. 1037), is at the basis of Wald's work and of the programme for the new mathematization of Walrasian general economic equilibrium theory. According to the historical reconstructions (Weintraub 1983, Arrow 1989), Menger showed Wald's paper to von Neumann and invited him to publish in the *Ergebnisse* his 1932 paper on general economic equilibrium dynamics read to the *Princeton Mathematical Society*. Von Neumann's paper (1937) was a more advanced mathematical formalization, from the technical point of view, of the problem of the existence of an equilibrium, and it freed the model from any bond with the real world, which still existed in Wald's methodological premise.

4.3.2. Abraham Wald's contribution: between tradition and innovation

In his 1936 expository article Wald (1936b) started by maintaining that "mathematical economics" is "a new method" (p. 368), and "an indispensable tool for many subtle investigations of various areas of economic phenomena" (ibid.). Unfortunately, he added, "sins have been committed in mathematical economics": unawareness of the assumptions and their implications, and of their conditions of validity. These sins are not imputable to mathematical method itself, Wald claimed, for "they have their origin in inappropriate, even erroneous, applications of mathematics" (ibid.). He thought that for a "fruitful application of mathematics in economics" it was essential that all the assumptions "be enumerated completely and precisely" (ibid.). These recommendations had already been strongly emphasized by Menger in his 1936 paper. According to Wald, "if these directions are strictly adhered to", then "the only objection which can be raised against a theory is that it includes assumptions which are foreign to the real world and that, as a result, the theory lacks applicability" (p. 369) – the key issue according to Pareto and the Paretian mathematical economists. Wald recognized that "in many areas of mathematical economics very substantial abstractions are being used, so that one can hardly speak of a good approximation to reality" (ibid.), but he defended the adoption of "far-reaching abstractions" using some (weak) arguments that had already been used by some mathematical economists of past generations – i.e. that "mathematical economics is a very young science" (ibid.) and "economic phenomena are of such a complicated, involved nature that far reaching abstractions must be used at the start merely to be able to survey the problem" (ibid.). These problems required adopting the method of successive approximations – "more realistic assumptions must be carried out step by step". Wald optimistically concluded that if these "directions are strictly adhered to", then "it will always be known precisely just where the assumptions are still so simplified and unrealistic that they must be replaced with better ones, so that ultimately theories will be derived that are well applicable to the real world" (p. 369).

Wald was the first to deal with the mathematical questions of existence and uniqueness in a systematic way. He started by criticizing the assumptions made by the old mathematical economics on the equality of the number of equations and unknowns, recalling that: "the equality of the number of equations and unknowns does not prove that a solution exists, much less the uniqueness of a solution" (369-370). The assumption that this equality may represent a sufficient condition for the solution of the system of equations is inadequate in the economic field because solutions have economic meaning only if are non-negative in the prices of goods and services. Therefore Wald investigated the conditions of non-negativity. He adopted the simplified version of Walras's equations proposed by Cassel with the modifications introduced by Schlesinger (1935). The Cassel system is written as follows:

$r_i = \Sigma a_{ij} s_i \quad (i = 1, \ldots, m; j = 1, \ldots, n)$

$\sigma_j = \Sigma\ a_{ji} \rho_j \quad (i = 1, \ldots, m; j = 1, \ldots, n)$

$\sigma_j = f_j (s_1, s_2, \ldots, s_n) \qquad (j = 1, \ldots, n)$[15]

where:

$r_1, r_2, \ldots, r_m$ are the available quantities of the m productive services $R_1, R_2, \ldots, R_m$

$s_1, s_2, \ldots s_m$ are the quantities produced of the n goods $S_1, S_2, \ldots, S_n$

$\sigma_1, \sigma_2, \ldots, \sigma_n$ are the prices of n goods

$\rho_1, \rho_2, \ldots, \rho_m$ are the prices of the m productive services

a_{ij} are the technical coefficients considered constant ($i = 1, \ldots, m; j = 1, \ldots, n$)

Wald considers as production factors all the factors, both scarce (those considered by Walras and Cassel) and not scarce, or free. This implies the transformation of the equations into inequalities. Hence we have:

$$r_i \geq \sum a_{ij} s_j \qquad (i = 1, ..., m;\ j = 1, ..., n)$$

or

$$r_i = \sum a_{ij} s_j + u_i \quad (i = 1, ..., m;\ j = 1, ..., n)$$

where $\forall\, i$, $u_i \geq 0$. If $u_i > 0$ then r_i is a free good and $\rho_i = 0$. This means adding *m* equations:

$$u_i \rho_i = 0 \quad (i = 1, ..., m)$$

The problem to be solved is to demonstrate the existence of a economically meaningful solution of the system of *2m + 2n* equations in *2m + 2n* unknowns, i.e. u_i, ρ_i, s_j, σ_j, (i = 1, ..., m; j = 1, ..., n). The new system of equations is:

$$r_i = \sum a_{ij} s_i + u_i \qquad (i = 1, ..., m;\ j = 1, ...,n)$$

$$u_i \rho_i = 0 \qquad (i = 1, ..., m)$$

$$\sigma_j = \sum a_{ji} \rho_j \qquad (i = 1, ..., m;\ j = 1, ...,n)$$

$$\sigma_j = f_j(s_1, s_2, \ldots, s_n) \quad (j = 1, ..., n)$$

where r_i and a_{ji} are given and f_j are known functions. Wald showed the existence of economically meaningful solutions under a set of (limitedly realistic, according to himself) hypotheses.

4.3.3. Beyond tradition: John von Neumann’s contribution

4.3.3.1. *On the intellectual origin of the model.* Arrow (1989) thought that there was a Walrasian influence in von Neumann’s model and this has been the prevailing interpretation in the literature. However, subsequent historical research has shown that the intellectual origins of the model are not so simple. They seem to derive not only from the Viennese discussion on the Walras-Cassel model but also from the Berlin debate in Ladislaus von Bortkievicz’s circle.[16] According to some scholars (Wittman 1967, Kurz and Salvadori 1993, Leonard 1995), von Neumann may have been influenced by the price model in a planned economy formulated by Robert Remak (1929). Remak was a young mathematician, student of the mathematicians Georg Frobenius and H. A. Schwarz, who had an intellectual relationship with Bortkievicz (Wittman 1967). He was *privat-dozent* at the University of Berlin from

1929 to 1933, more or less the same period in which Von Neumann was there (Ulam 1958). The simultaneous presence in Berlin of Von Neumann and Remak gave rise to the conjecture, advanced by Wittman (1967) and revived by Kurz and Salvadori (1993) and also accepted by Leonard (1995), that in preparing his model, the young von Neumann had in mind the model that his older colleague had presented at a Berlin Mathematical Society seminar.

In 1929, following Bortkievicz's suggestions (Remak 1933), Remak carried out a study on the determination of rational prices for a centrally planned closed economy. His model represented the economy as a classical circular process of production. He considered a closed economy without wages and profits in which the quantities of the various commodities produced and consumed are known. The problem for such a system is the determination of a set of prices which would "provide the basis for a financially viable economy" (Remak 1929, 271). Under the assumptions that the production process is circular, the total quantities of each product and the productive technology are given, the period of production is the year, and the system is in a stationary state, the problem to solve is the following: given the technical coefficients of production, $a_{ij} \geq 0$ (i, j = 1, ... , n), i.e. the quantity of each commodity that the industry i furnishes to the industry j to produce a unity of the commodity j (which could be positive or zero in the case in which the industry i does not supply anything to the industry j), to determine the prices of the commodities y_i (i = 1, ... , n) so that each industry's income from the supplied commodities is equal to its expenditure on the received goods. This system is called a 'superimposed price system' or 'rational price system'. Remak demonstrates the existence of an economically relevant solution, i.e. where prices are ≥ 0, unique up to a factor of proportionality. This 'classical' representation is the one adopted by von Neumann in his 1937 paper.[17]

4.3.3.2. *Von Neumann's model*. The model assumes a linear technology of a set of processes of production and goods. It is a "closed" circular model because there is no distinction between resources and final uses – "goods are produced not only from natural factors of production, but in the first place from each other" (p. 1) -. Calling

a_{ij} and b_{ij} the units of G_j (j = 1...n) respectively consumed and produced in P_i (i = 1...m) with $a_{ij} \geq 0$ and $b_{ij} \geq 0$, the process may be described as follows:

$$P_i : \sum_{j=1}^{n} a_{ij} G_j \rightarrow \sum_{j=1}^{n} b_{ij} G_{j_{j_i}}$$

Each process is considered to be "of unit time duration" ("processes of longer duration to be broken down into single processes of unit duration") (p. 2). Processes are used with a certain intensity. Von Neumann defines equilibrium as the state "where the whole economy expands without change of structure" (p. 2), i.e. where the ratios of the intensities are "unchanged" (ibid.). They must be multiplied by a common factor α per unity of time, α being the ratio between production in a period of time and production in the preceding period – i.e. "the coefficient of expansion of the whole economy" (ibid). Given the quantities a_{ij} and b_{ij}, it is necessary to determine: (i) the intensities of the processes, (ii) the coefficient of expansion of the whole economy, (iii) the prices of goods, (iv) the interest factor. This gives rise to a system of inequalities. In order to solve this system, von Neumann made use of mathematical methods much newer than Wald's, and able to improve greatly on his proofs. In fact, von Neumann's method of analysis did not use differential techniques but instead employed topological techniques for the first time in economics. The demonstrative technique transformed the problem of determining an equilibrium into a *minimax* problem: that is, the conditions of existence of an equilibrium are equivalent to the condition necessary for a *minimax* solution (a saddle point). The solution of the system of equations is possible, von Neumann wrote, "only by means of a generalization of Brouwer's Fixed-Point Theorem – i.e. by the use of very fundamental topological facts" (p.1). This connects the solution of systems of linear inequalities to the *minimax* solution of a two-person zero-sum game of a previous 1928 article where von Neumann laid the mathematical bases of game theory and proved the first *minimax* theorem. In a note to the 1937 paper, von Neumann emphasized the connection: "the question whether our problem has a solution is oddly connected with that of a problem occurring in the Theory of Games dealt with elsewhere (Math. Annalen, 1928)" (von Neumann 1937, p. 5). Whereas von

Neumann proved the existence of a saddle point for a certain function in the 1928 paper, in the 1937 paper he proved a 'fixed point lemma' that generalizes Brouwer's theorem, from which the existence of a saddle point for the equilibrium function follows (see Kjeldsen 2001). The use of a fixed-point theorem in the proof of existence of equilibrium became a standard tool in general equilibrium analysis, one of the technical cornerstones of the modern approach.

4.3.3.3. Axiomatic approach in a totally coherent way. As many scholars have emphasized (for example Ingrao and Israel 1987, Punzo 1991), the axiomatic approach in economics[18] is applied in von Neumann's paper in a totally coherent way, in the sense that the concern for the economic interpretation of the model – still existing in Wald - disappears:

> "In order to be able to discuss [the properties of the economic system] quite freely we shall idealize other elements of the situation … Most of these idealisations are irrelevant" (p. 1)

This theoretical attitude derived, firstly, from the fact that, as Champernowne noted in his "commentary note" to the English translation of von Neumann's paper (1945), von Neumann dealt with the economic question "as a mathematician". In this way he obtained a mathematical solution of a "highly generalised problem in theoretical economics" characterized by the elegance of its solution, logical completeness, concision and rigor, but he adopted "extremely artificial assumptions" (p. 10) or "idealisations" as von Neumann termed them. Secondly, von Neumann's attitude derived from the fact that he dealt with theoretical economic problems like a *formalist mathematician* – i.e. he conceived the model as a formal structure whose legitimacy and cogency depend on its internal consistency.[19]

Conclusions

Narrative histories of the GEE in the 1930s have some major shortcomings. They focus essentially on the *Mathematische Kolloquium*'s contributions in Vienna, considering them to constitute a revolutionary new step in economic theory and the rigorous development of Walras's and Pareto's classical theories of general equilibrium. They omit the *Wiener Kries*'s reflections on the nature and method of economics. They underestimate, considering it old-fashioned compared with the Viennese works, Hicks's contribution at LSE. By focusing on the theoretical production and the methodological discussion in Vienna and at the LSE, this paper outlines a new interpretation in terms of 'theories in competition' on the issue of the relationship between theory and the real world: this, in fact, was the fundamental issue at stake. Herein also lies the interest in those distant controversies for the current debate in economics.

Hicks's *Value and Capital*, and the methodological reflection on the scientific method in social sciences – in particular in Viennese economics in the context of logical empiricism's foundation –, represented the new form of the theoretical and methodological questions raised by Pareto. As regards Hicks, his theoretical project had the explicit ambition of bridging the gap between statics and dynamics in the GEE model, thereby resolving the sterility of the classical model and the impasse in which it was trapped, but still remaining within the Paretian analytical and methodological framework. Hicks's attempt was only in part successful: his reformulation of the static model was certainly an important achievement, as the majority of reviews and comments acknowledged, but his construction of a dynamic system attempted in Part IV of his book had disappointing results. His program was in part recalled by Samuelson, thereby accomplished an extremely successful modernisation of economics. As regards the *Wiener Kries*'s reflection on the relation between theoretical model and the real world, it represents, from the epistemological and methodological point of view, a modernisation of Pareto's thought. What is also important to emphasize is that it related directly with the early econometric programme: this can be considered another attempt to resolve the Paretian impasse from a Paretian perspective.

As regards the *Mathematische Kolloquium*'s contribution by Menger, Wald and von Neumann – according to the economics *vulgata* the great achievement of the period – we have seen that they took the abstractness of the Walrasian model to its extremes in their economic thinking. They abandoned the idea of mathematics as logic and rigour, *together with* a strong emphasis on facts and applications, that predominated among mathematical economists of the Paretian school of the preceding period and among contemporary mathematical economists and econometricians, like E.B. Wilson (Samuelson's mentor) at Harvard, Charles Roos and Harold Hotelling at Columbia, Henry Schultz at Chicago, as well as Arthur Bowley and R.G.D. Allen at LSE. Menger's reflections and Von Neumann's axiomatic program set out in the 1937 paper provide theoretical justification for the weak link between theory and the real world in Walras – a well-known problematic issue emphasised by Poincarè in his correspondence with Walras himself. Von Neumann's program freed the Walrasian one from the need for the realism of hypotheses and their verification. In this sense, we have emphasized, the debate on the Walras-Cassel model in the *Mathematische Kolloquium* was the beginning of the neo-Walrasian theory of the 1950s.[20] It laid the bases for the radical extension of formalism in economics definitively affirmed with Debreu (1959)[21]: "allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations", states Debreu, adding that such a dichotomy between the theory in the strict sense and its interpretation "reveals all the assumptions and the logical structure of the analysis" and "makes possible immediate extensions of that analysis without modification of the theory by simple reinterpretations of the concepts" (p. x). This implies that its actual aim is not realism but understanding the implications of axioms and assumptions for the results. In this perspective economic theory turns out to be simply the construction of a model of rational resource allocations which may be used to evaluate economic performance. We are thus brought back to the normative attitude expressed by Walras and criticized by Pareto as 'metaphysics'. But if the assumptions have, as Pareto wrote, from the scientific point of view, a grounding role, the crucial issue is their relevance and acceptability, which requires "a very close examination" of the premises from an experimental point of view. In any case, as well known, also in the narrow domain of pure theory the problems raised seem highly problematic if not insurmountable.

In the 'confrontation' among theories of the 1930s, two different conceptions of rigour – the term that all scientific schools use to emphasize their scientificity with respect to the other schools – emerge: the rigour of (experimental) method recommended by Pareto, and the mathematical (formal) rigour recommended by Wald and von Neumann. Of course, the former does not exclude the latter concept of rigour, but it limits its freedom. Pareto's (and Marshall's and Edgeworth's too) lesson was that the language of rigor in economics does not necessarily imply the adoption of a language reduced to a manipulation of symbolic strings, but that, then and now, the question is how to look at the real world in a scientific manner and not escape from it with an excessive formalism.

Therefore, that confrontation was not between innovators and conservative, old-fashioned positions, but between different conceptions of economics and methodological approaches. It was one stage in a recurrent confrontation in the history of economics that we have represented by referring to the two epistemologically different lines of inquiry epitomized by Pareto and Walras.

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Notes

[1] The literature on General Economic Equilibrium of the period between the two World Wars is considerable, and this paper widely refers to it. The by now classic works on that period are Weintraub (1983) and Ingrao and Israel (1990), the latter being the most complete existing history of GEE from Walras's forerunners to the contemporary formulations.

[2] The interpretation of von Neumann's 1937 article as a contribution to the mathematical formalist programme has been recently well sustained by Gloria-Palermo (2010).

[3] In a letter to Maffeo Pantaleoni, Pareto wrote: "I agree with Mill that syllogism does nothing except say again in the conclusion the premises in a different form. Mathematics is a syllogistic machine. From this emerges the need for *very close examination* of the premises. If accepted, the remaining reasoning (syllogistic and mathematical) is no more than a quasi-mechanical procedure” (letter to Maffeo Pantaleoni, October 3, 1891, in Pareto 1984, Vol. 1, pp.70-71).

[4] In his *Manuale* Pareto introduced the idea of sequences of equilibria, or “successive equilibria” as he called them, but he later became rather skeptical concerning the viability of his project to merge statics and dynamics.

[5] Wladyslaw Zawadski was a Polish statistician and economist. His 1914 book may be considered one of the best textbooks on Paretian mathematical economics published in that period.

[6] It is fair to remember the contributions by Griffith Evans and Charles Ross in US about dynamic equilibrium – they were followed in the 1930s by some Italian Paretian economists like Luigi Amoroso and Eraldo Fossati. However this research approach was almost entirely abandoned after the war (see Pomini 2012).

[7] Later Hicks and Allen, together with Schultz at Chicago (see Allen 1936), became aware of the fact that their article was essentially Slutsky's theory (Slutsky 1915).

[8] Of remarkable importance, the discussion on the determinateness of the utility function and on Pareto's concept of efficiency, at the basis of the contributions to the socialist calculation debate.

[9] The time dimension was dealt with by considering expectations and plans over future time horizons. Hicks proposed a temporary equilibrium model where the path of the economy follows a sequence of temporary equilibria – the idea proposed by Pareto in his *Manuale*. Hicks introduced the idea of “pure future economy” which inspired the axiomatic model of intertemporal general equilibrium developed by Arrow and Debreu.

[10] Samuelson maintained that the most important progress made by *Value and Capital*, from

the analytical point of view, was "the enunciation of the principle that a group of commodities has the property of a single commodity if their prices all change in the same proportion" (Samuelson 1948, p. 130). It was, according to him, "the cornerstone of his exposition" (Samuelson 1998, p. 1381). On the other hand, Samuelson was also critical of *Value and Capital*. In particular, he showed that the Hicksian stability conditions are not, in general, necessary nor sufficient in order to satisfy the stability of equilibrium in a dynamic system (Samuelson 1941, pp.108-112, Samuelson 1948, pp.269-274). From his point of view, Hicks later wrote that Samuelson, Arrow et al. recognized in *Value and Capital* the starting point of their work on GEE then accomplished "with far more skill in mathematics" than himself. On the other hand Hicks judged those "great" results extraneous to his way of thinking, which did not adhere to making theory for itself nor econometrics (Hicks (1979) pp.201-202) . Roy Allen, who was at that time in constant intellectual dialogue with both Hicks and Samuelson, wrote (Allen 1949) that the two authors "have now come together on essentials" so that the "future development...will flow from an agreed combination of the two expositions" (p.112). This is a judgement which became a common opinion: Arrow (1974, pp.255-260) speaks of a "Hicks-Samuelson Model of General Equilibrium" whose "primary interest... was rather in the laws of working of the general equilibrium system...than in the questions of existence and the like". Allen was able to show also the differences, not trivial, between Hicks and Samuelson: whereas Hicks, he wrote, wanted to present "a full development of one particular line of approach", Samuelson wanted "to unify diverse fields of economic theory by showing up the common, underlying mathematical basis" (ibid.). Actually, this means a different conception of the use of mathematics in economics. More recently Samuels (1993, p.354) stressed that Hicks "was...concerned that theory....be realistic in regard to the real world" and stated that "what we want, in economics, are theories which will be useful, practically useful" whilst Hahn (1990, p.547) pointed out that "Hicks's comparative advantage was in a[n]...informal mixture of technicalities and economics".

[11] On the formal shortcomings of Hicks's GEE, see Collard 1993, pp.334-335

[12] When he wrote his review Morgenstern was "under von Neumann's spell" (Leonard 2010). Morgenstern (1976) recounts von Neumann's irreverent opinion of the productions of mathematical economics at the end of the 1930s, and the following quotation refers particularly to Hicks (see Ingrao-Israel 1990, p. 197 and note 60 p. 410): "You know, Oskar, if those books are unearthed sometime a few hundred years hence, people will not believe they were written in our time. Rather they will think that they are about contemporary with Newton, so primitive is their mathematics. Economics is simply still a million miles away from the state in which an advanced science is, such as physics". Substantially the same judgement was passed on Samuelson : Leonard (2010, p. 244) writes that von Neumann felt a private disdain for the 'primitivity' of the Samuelsonian mathematical economics of *Foundations*.

[13] Cassel's system was adopted as the starting point for the discussion for a series of reasons: Cassel's 1918 book was widely used as textbook in Central Europe countries (Weintraub 1983); the Viennese scholars thought that Cassel's formulation gave the proper solution to the price imputation problem investigated by them (Punzo 1991); Hans Mayer's seminar, where the imputation problem was discussed, was the occasion when Menger and Schlesinger became familiar with the Walras-Cassel system (Mutoh 2003).

[14] On Wald and his formative years, see Morgenstern (1951) and Menger (1952).

[15] Cassel's model assumed that the demand functions depends on the prices of all commodities; by contrast, the Schlesinger-Wald model followed the Austrian school's approach and inverted the relationship, that is, used inverse demand functions in which prices are determined by the quantity of demand (see Punzo 1989).

[16] In the first decades of the twentieth century Berlin was an important centre for mathematical economists due, in part, to the presence there of the eminent statistician and economist Ladislaus von Bortkievicz. Although an effective group of followers was never formed in Germany, Bortkievicz's house, as the Swedish statistician Oskar Anderson remembers (1931), was for decades a place of pilgrimage, where scholars from different countries gathered to discuss problems and seek advice (on Bortkievicz, see Marchionatti and Fiorini, 2000).

[17] The objective affinity between von Neumann's view of the economy and the classical economists' approach was emphasized for the first time by Champernowne (1945-46) in his paper accompanying the English publication of von Neumann's paper and closely discussed by the Italian economist Claudio Napoleoni (1965).

[18] On a discussion on the subject of axiomatisation and the difference between axiomatics and formalism, see Weintraub (1998) and (2002), Mongin (2003) and Agliardi (2004).

[19] The emphasis on the formalist nature of von Neumann's contribution makes the problem of the intellectual origin of the model less relevant, as correctly observed by Gloria-Palermo (2010).

[20] This does not mean that the work of Debreu (1959) and the Arrow-Debreu model of GEE are the terminus of Walras's route, as many authors argue (for example Hildenbrand 1986). As Kirman (2011) has shown, there are different interpretations of what Walras had in mind and he himself switched between the two, even though it can be argued that, due to Walras' 'philosophical' attitude, the dominant interpretation is the neo-Walrasian one (see also De Vroey 1999, Walker 1996, Marchionatti 2007).

[21] As incisively maintained by Weintraub (2002), "From Hilbert to von Neumann, to the *Mengerkries* and Wald, to Bourbaki and thence to Debreu runs the chain of causality, the development of modern economic theory in its unconcern to study real economies" (p. 97).