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**The economic theory of *Wiener Kreis* and *Mathematische Kolloquium*. The complex role of Karl Menger**

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**Summary** This paper is rooted on the idea that between the 1920s and the mid 1930s, studies on economic theory in Vienna were fundamental to understand the following developments of economics as a science. ‘Economics in Vienna’ during that period implies not only the contributions of the Austrian economic school, but also the roots of the two lines of economic mainstream developed after World War II: econometrics and mathematical economics, whose origins were respectively inside the *Wiener Kreis* (where Neurath introduced economic theory) and in the *Matematische Kolloquium*. In Vienna an interesting form of heterodox economy was developing, too: the exigency of a critique to the economic mainstream - in an analytical and also political point of view - was very strong and was shared by many Austrian social thinkers (beyond Neurath), like Karl Polanyi, Otto Bauer, Felix Schaffer, Felix Weil, J. Marschak. As Oskar Morgenstern recalled, - in April 1975 during a seminar at the New York University held with Fritz Machlup on “Vienna in the 1930’s” - , the locations in which economics was studied were: the University, the Institute of Business Cycle – leded by Ludwig von Mises-, the group around the *Zeitschrift für Nationalökonomie*, and some circles such as the Schilck’s one (the famous *Wiener Kreis*) and the *Mathematische Kolloquium*.

This paper is focused on the economic theory of *Wiener Kreis* and the *Mathematische Kolloquium* and in particular it deals with the relation between economics and mathematics. Its aims are to show the role of economics as a science in Austrian neopositivism (in particular in Otto Neurath’s contributions); to underline how economic theory became a part of *Unified Science* (the thesis of the *Wiener Kreis Manifesto*, 1929); to describe the methodological and epistemological cleavage that arose in Karl Menger’s *Mathematische Kolloquium*; to stress the fact that the reformulation of the model of general economic

equilibrium (GEE) in *Kolloquium* distanced economic theory greatly from the *Kreis*'s neopositivist point of view; and to point out some opinions on the relation between the *Kreis* and the *Kolloquium* that the past literature has not clarified completely. The role of Karl Menger is fundamental to understand of these historical developments, which brought about very close adherence between Austrian economic theory and mathematical economics. But I shall try to show that this interpretation is very problematic.

**JEL classification: B25; B31; B40**

### ***Introduction***

As Weintraub (2002) said, when we talk about the interconnections between economics and mathematics, we must first clarify to which kind of mathematics we are referring, without confusing the several kinds of mathematical language and their varying degrees of abstraction and formalization. When dealing with the controversial relation between economics and mathematics, we are accustomed to studying the subject from the point of view of economists, seeking to understand what mathematical instruments they employ, and to determine whether they make proper use of them. It is also possible to approach the matter by taking the opposite point of view: that is, by investigating how mathematicians (and scientists in general) consider economics and in particular mathematical economics. In this paper I shall seek to show how the Viennese physicists and mathematicians of the *Wiener Kreis* and the *Mathematische Kolloquium* considered economic theory, and the kind of mathematical economics that they developed.

I shall try to answer to the following questions. What kind of interest did economics arouse, first in the *Kreis* and then in the *Kolloquium*? What members of these groups were engaged in economics and what kinds of economic problems did they deal with? Did they regard economics as a science and, if they did, what epistemological and methodological model did they apply to it. Was this model the same in the *Kreis* and in the *Kolloquium*? What was the role of Karl Menger, the mathematician son of Carl Menger, who first joined the *Kreis* and then left it to found the *Kolloquium*? Finally, can we agree with Menger's judgement on the relation between mathematical economics and the Austrian school?

On answering these questions, we shall see the methodological and epistemological break between Karl Menger's *Kolloquium* and the universalistic and encyclopaedic project pursued by the *Wiener Kreis* during the 1930s. On the contrary, the past literature – Menger (1973; 1980; 1994), Gilles (1981), Weintraub (1983), Punzo (1991) – has argued for some sort of continuity between the *Kreis*, the *Kolloquium*, and even the economic theory of Austrian

school. As Gilles (1981) wrote: “What then is Karl Menger’s unified philosophical outlook? Not surprisingly for a member of the Vienna Circle, it is *logical positivism*, and this label actually fits Menger better than it does some other members of the Circle”. Gilles continues: “many important ideas of the Vienna Circle originates with Menger – though they are often attributed to others”. As Punzo (1991) puts it: “[The] interest [in mathematical foundational issues] was enhanced by strong, often personal, bonds with the members of the *Wiener Kreis*, and economists often participated in both clubs”.

Menger stressed the close relation between the *Kreis*’s members and the mathematicians of the *Kolloquium*, recalling that: “on several occasions, the mathematicians in the Circle helped the philosophers by providing them with technical information – often concerning their own results. I remember, for instance, a discussion about inductive processes in physics” (Menger 1980, p. 13). And furthermore: “in these meetings, topics and results in their and my fields of interest were reported and discussed. We followed the unconstrained style of the Schlick Circle” (Menger 1994, p. 201).

Most recently, Stadler (2001) and Becchio – Marchionatti (2005) have recognized the controversial role of Karl Menger in the passage from the *Kreis* to the *Kolloquium*: “Menger further distanced himself from the Vienna Circle in the course of the latter’s protocol sentence debate between two opposing positions: Schlick’s “foundation of knowledge” (in the form of affirmations”) and Carnap’s and Neurath’s fallible conception of the empirical basis of science in the form of the physicalist protocol sentences. Advocating a moderate position, Menger finally expressed his criticism of unified science in a remarkable overall assessment” (Stadler 2001, p. 400).

### ***1. History of Kreis and Kolloquium (1922 – 1939)***

The *Wiener Kreis* was founded in 1922 as a philosophical circle by the physicist Moritz Schlick, who organized meetings every Thursday evening to discuss philosophy and the natural and social sciences, to which the members endeavoured to apply the neoempiricist paradigm. The epistemological model of this new empiricism was set out in Schlick’s *Theory of Knowledge* (1918), in which he distinguished between *kennen* (mere intuitions in the stream of sensations) and *erkennen* (concepts linked to symbols in order to produce scientific knowledge). These concepts must always be verified: not, however, by correlating them with mere perceptive data, but by finding a tool able to remove heterogeneity between empirical observation and logical elaboration. Scientists working in numerous disciplines joined the *Kreis*. Hans Hahn and Kurt Reidmeister were mathematicians: the former arrived in Vienna

from Prague in 1923, in order to take up the chair of philosophy of science (previously occupied by Mach and Boltzmann). Otto Neurath was interested in mathematics, economics and sociology. Victor Kraft was a historian and Felix Kaufmann a lawyer. In 1926 the philosopher Rudolph Carnap joined the group and introduced it to Wittgenstein's *Tractatus*. Wittgenstein's statement that "everything that can be said, can be said clearly" (T 4.116) became the *Kreis*'s intellectual directive: it expressed not only an a-metaphysical but also an anti-metaphysical point of view.

In 1927 Karl Menger returned to Vienna (where he had been born and where he had studied physics and mathematics) on invitation by Hans Hahn, his old friend and mentor, (Menger 1994). From 1924 to 1927 he had worked in Amsterdam as Brouwer's assistant. Disappointed by Brouwer's intuitionism, he came back to Vienna, where he was appointed professor of geometry and took over the chair previously occupied by Kurt Reidmeister (who had moved to Prague). He joined the *Kreis* on invitation by Schlick and Carnap, who were very interested in his studies of curve theory. In 1928 Menger organized his *Mathematische Kolloquium*, in collaboration with the Warsaw School: these were informal meetings held in the evenings with some of his students and colleagues, among them Kurt Gödel and Abraham Wald. Many foreign visitors attended Menger's seminars as well, for instance John von Neumann. During a seminar on "Vienna in the 1930s" held on 9 April 1975 with F. Machlup, at NYU, Oskar Morgenstern recalled some of them: Knight, Ohlin, Kaldor, Robbins, Marget, Perroux, Viner, Balogh, Schneider, Lange, Zeuthen, Robertson, Anderson, Nurske, Robinson, Alan and Paul Sweezy, Graham.<sup>1</sup> In 1931 Menger became the editor of *Ergebniss eines Mathematische Kolloquium* (I series, Vienna, 1931-37),<sup>2</sup> in which papers given at the *Kolloquium*'s meetings were published.

In the meantime, *Wissenschaftliche Weltauffassung: Der Wiener Kreis* (1929) was printed and signed by Hans Hahn, Otto Neurath and Rudolph Carnap. This was regarded as the *Kreis*'s *manifesto*. According to Philip Frank, the expression "wissenschaftlichen Weltauffassung" (i.e. a particular scientific conception of the world) was used instead of "Weltanschauung", a term with excessively metaphysical connotations. The *Manifesto* was dedicated to Moritz Schlick, and it was published on the occasion of the Prague conference on "the Gnoseology of Exact Sciences". The *Manifesto* stated the intention of the Viennese scientists to extend the *Kreis*'s project to the entire scientific community, and then to society as a whole, the purpose being to free scientific and common knowledge from metaphysics,

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<sup>1</sup> See Oskar Morgenstern's Papers, Perkins Library, Duke University, box 63.

<sup>2</sup> Menger held his *Kolloquium* also at Notre Dame where he was also the editor of *Reports of a Mathematical Colloquium* (II, 1937-46). Between 1946 and 1971 he taught at the Illinois Institute of Technology, Chicago.

and to replace old materialism with modern empiricism. This desire for openness became evident in the so-called ‘internationalization’ of the *Kreis* during the 1930s, when its activities focused on the organization of international congresses (Prague 1929; Königsberg 1930; Prague 1934; Paris 1935; Copenhagen 1936; Paris 1937; Cambridge, Mass 1938; Harvard 1939) in order to promote its project for an *International Encyclopaedia of Unified Science*, which was finally presented at the “First Congress for the Unity of Science” held in Paris (1935). The first volume of the *Encyclopaedia* was published in 1938, and it was, according to Neurath, the natural development of the *Manifesto*’s program. This universalistic approach able to unify all the empirical sciences from a methodological point of view by the use of rigorous language (so-called *physicalism*), was pursued by the *Kreis* during the period of its international *acme* (Stadler 2001).

In the meantime, many member of *Kreis* had left Austria after Dolfuss’s assassination; as Menger wrote: “in 1934 Schlick, Waismann (at that time both completely under Wittgenstein’s spell) and Kraft were the only members of the original Circle left in Vienna” (Menger 1974, p. 114). Nazi domination definitively destroyed freedom in Austria and all the Viennese intellectual movements. Menger described the period in Vienna from 1927 up to 1938 in his *Reminiscences*, published posthumously in 1994. Their drafts are conserved in the archive of Perkins Library, Duke University, Durham (North Carolina, U.S.). On reading these drafts one finds that many parts of the original text are still unpublished. The reason for the book’s incompleteness is Menger’s precarious health while he was working on it<sup>3</sup>. These unpublished chapters are of great interest because they describe the evolution of the rupture between the *Kreis* and the *Kolloquium*, i.e. between physicians and social scientists (Schlick, Neurath, Carnap, Waismann), on the one hand, and mathematicians (Menger, Wald, von Neumann, Gödel) on the other. This discontinuity between these two philosophical movements has often been overlooked, or even denied.

## **2. Economics in the Wiener Kreis: from Manifesto (1929) to Encyclopaedia of Unified Sciences (1938)**

Otto Neurath introduced economics into the *Wiener Kreis* (Neurath 1973; 1983; 2004 and Uebel 1991). He had studied philosophy and economics at Vienna (as an undergraduate) and Berlin (as a Ph.D. student). Karl Menger described him as “a man of an immense energy and curiosity” (Menger 1994, p. 60). He began his career with some short essays on Boolean

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<sup>3</sup> Karl Menger’s archive contains numerous letters in which he asks old Viennese friends to help him reconstruct the history of the Viennese period. See in particular the letter from Karl Popper (21 March 1971) and Herbert Feigl (3 May 1972), Karl Menger Papers, Perkins Library, Duke University, box 22 (Vienna Circle Materials).

algebra and then became closely interested in economics and sociology. According to the bibliography of the *Manifesto*, Neurath was the only member of the *Kreis* concerned with the foundations of sociology and economics.

Neurath described himself as a ‘social engineer’ and sought to find a technical approach to economics that would transform economic systems; he studied economic theory in order to change the social order according to a rational model for the improvement of the lower classes. In Vienna he had attended Böhm-Bawerk’s seminars, studying the Mengerian version of marginalism, and criticizing it. In Neurath’s view, the metaphysical concept of marginal utility, which was perfectly suited to the “bourgeois theory of Menger father” (Leonard, 1998), was useless not only in understanding but above all in modifying the material conditions of the working classes.

Neurath’s first essays in economics were written at the beginning of the century and consisted of methodological inquiries into ancient and modern economic history. He declared himself closely influenced by Pareto’s *Manuel*,<sup>4</sup> with which he shared the opinion that, after introduction of *homo oeconomicus*, economic theory had become too abstract: “homo oeconomicus was created, as Pareto remarked (1909) endowed also with an economic motive that until then had been alien to psychology and will probably remain so in the future” (Neurath 2004, p. 274). Abstractness was the first defect of economics; the other was its “fallacy”, which derived from the identification of economics as a whole with just one historical model of it (the monetary economy): “economic theory also followed too closely the example of monetary economics” (p. 278). This mistake caused the transformation of political economy to monetary economy. Neurath wrote: “the thought was lost sight of that money itself is but a means of the technique of organization that might be radically changed or even removed” (ibid.). According to Neurath, these two errors were rooted in “the opposition between abstract economic theory and concrete economics from the history of economic thought” that provoked the famous debate on *Methodenstreit*.

Neurath was convinced that the aim of economic theory is to explain how the material conditions of life are formed by “transfers of goods” and to identify the conditions under which one state can be derived from the other. He claimed: “*true science consists in systematically examining all possible cases*. Exact political economy has not achieved this until now. It does not even encompass all actual cases. This is one of the reason why exact theory finds itself in opposition to the historical school and why it does not have an awful lot

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<sup>4</sup> V. Pareto *Manuel d’économie politique*, Paris, 1909; see also V. Pareto “Anwendungen der Mathematik auf Nationalökonomie”, in *Enzyklopädie der Mathematische Wissenschaften*, I, 2, Teubner, Leipzig, 1910?.

to say to those economists who occupy themselves with issues of practical interest, theories of crises, cartels and trusts” (ibid.).

In 1917 he wrote *The Conceptual Structure of Economic Theory and its Foundations* (Neurath 2004). This marked Neurath’s passage from an exclusive methodological interest to investigations into concrete alternatives to liberal society. He maintained that it was necessary “to create a structure for an economic theory that is able in principle to provide equal theoretical treatment to all possible forms of economic activity.” (ibid p. 312). Neurath now formulated his theory of the so-called “economy-in-kind,” as an alternative to both the capitalist and socialist economies. An economy-in-kind derived, not from the concept of economic efficiency but from that of “wealth”, i.e. “what one produces and consumes in the widest sense”. This concept “is linked to all those scholars who simultaneously treat different forms of economy and to all those who as utopians treat of possible institutions” Neurath remarked that only a small number of economists were able to do this, most notably Carl Menger and Vilfredo Pareto.

During the First World War, Neurath joined the debate on planned economy (Uebel 1991; Caldwell 1997, Neurath 2004). In 1919 he argued that during times of peace, profit-oriented economic production provoked cyclical periods of overproduction and unemployment. In wartime, on the contrary, production which was not directed to profit was always fully utilized, and suppression of the price system (replaced by planned prices) gave greater stability. Neurath recommended that some sort of planned economy should continue during peacetime, because it assured a better allocation of means. He insisted that there was no need for economic computation, because production can be objectively determined by general needs, rather than by profit-seeking, and he maintained that any computation of *inputs* and *outputs* could be conducted in physical terms.<sup>5</sup>

Neurath soon became one of the most influential member of the *Kreis*: in 1929, together with Carnap and Hahn, he signed the *Manifesto*, which placed economics among the five sciences that were to become objects of the new positivist philosophy. The *Manifesto* regarded Carl Menger (like Quesnay, Smith, Ricardo, Comte, Marx, Walras, Müller-Lyer) as a forerunner of those scientists able to use empirical and anti-metaphysical methodology in social sciences. The task of such a method was to clarify the paradigms of disciplines still excessively metaphysical. For example, it was necessary to purge history and economics of concepts like “spirit of the people” (*Volkgeist*), as had been done in physics, when the concept of “causality” had been replaced by “relation”.

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<sup>5</sup> Neurath’s paper on economic computation provoked L. von Mises’s reaction (von Mises 1920; von Hayek 1935).

During the 1930s Neurath worked on the ideal of a unified science, *Einheitswissenschaft*, and on “physicalism”, its new scientific language. In *Physicalism* (1931), he explained how the ideal of an unified science could be achieved by transferring the language of physics to the other sciences according to neoempiricist approach. He wrote: “At first the Vienna Circle analysed ‘physics’ in a narrower sense almost exclusively; now psychology, biology, sociology. The task of this movement is unified science and nothing less” (Neurath 1983, p. 52). The hierarchy of known objects now comprised concepts of the mind’s experiences and qualities; physical objects; objects of the social sciences. All of these were included in the so-called unified science because they had constant and quantitative relations to which physicalism was applicable. Neurath also introduced sociology into this ideal of unified science, considering it to be the merging of history and political economy.

The project to unify sciences was actualized in the *Encyclopaedia of Unified Sciences*, which was presented at international congresses for the unity of science organized between 1934 and 1939 by members of the *Kreis* (Neurath and Carnap in particular) jointly with the Berlin group, the Warsaw school and American pragmatists, principally Charles Morris (Stadler, 2001). Only one of the papers given at these congresses dealt with economics. It was Robert Gibrat’s *La Science economique. Methodes et philosophie* (Gibrat, 1936), which treated economics as a complex science that must be expressed in mathematical terms. According to Gibrat the proper mathematical tools for economics were econometric ones able to analyze statistical data.<sup>6</sup> The sixth volume of the *International Encyclopaedia of Unified Science*, devoted to economics, was written by Gerhard Tintner and came out in 1968. It was the natural development of Gibrat’s point of view: economic theory must use mathematics, and econometric models in particular. Tintner wrote: “As Carnap (1938) has pointed out, the procedure of social and hence economics are fundamentally the same as in the natural science (see also Morris 1938, 1946). Prices and quantities sold, interest rates, and the like are all quantitative concepts. ... For investigations of a whole economy, it is frequently necessary to construct index numbers – for example, a cost-of-living index, various price indexes which represent prices in sectors of the total economy ...” (Tintner 1968, p. 11).

### **3. From Kreis to Kolloquium: Karl Menger on language and mathematics**

Karl Menger joined the *Kreis* in 1927 on his return to Vienna from Amsterdam, but at the end of 1928 he “became aware of, and ever more disturbed by, [its] imprecision in the

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<sup>6</sup> In January 1933 the first number of «Econometrica» was printed. It was the official review of the Econometric Society, founded in 1930, by Irving Fisher and Ragnar Frisch. According to its statute, the aim of this society was to promote the use of quantitative approach in analyzing economic problems. (See also Mirowski 1989).

epistemological ideas and formulations” (Menger 1982, p. 90). The publication of the *Manifesto*, which he called “rather superficial” (Menger 1974, p. 114), alienated him from the *Kreis*: “the pamphlet estranged me from the Circle to the point where I asked Neurath to list me henceforth merely as *close* to, rather than as a *member* of, the Circle” (Menger 1982, p. 92). The distance between Menger and the *Kreis* became evident in regard to the role of mathematics and language in the new *Weltauffassung*, (Menger 1974; 1982; 1994). These different points of view are fundamental for understanding the rupture between the *Kreis* and the *Kolloquium* in economic theory as well.

To comprehend the *Kreis*'s point of view on mathematics we must consider Hahn's thoughts on the matter (Hahn 1980). In 1930 he wrote: “observation and the tautological transformations of thought – these are the only means of knowledge we recognize” (Hahn, 1980, p. 21) He claimed that the mathematics of the *Kreis* constituted an alternative to both Hilbert's formalism and Brouwer's intuitionism. Hahn considered Russell's logicism to be the only valid epistemological model: “our adoption of Russell's position may cause some surprise in Germany where all ears are turned to the controversy between intuitionism and formalism – between Brouwer and Hilbert... [Brouwer's] point of departure seems too much akin to Kant's pure intuitionism and Kant's *a priori*. ... As regards Hilbert's *formalism*, what must be pointed out from our point of view is above all the unexplained role of metamathematical considerations” (p. 26). This meta-mathematical knowledge derived neither from experience nor from logical transformations, and hence its nature was not explained. It was for this reason that Hahn adopted a “skeptical position towards Hilbert's point of departure” (p. 26).

In a subsequent paper of 1931, (Hahn 1980) Hahn again took up this position against the intuitionism of A. Heyting and the formalism of J. von Neumann: “I regard the investigations of both Brouwer and Hilbert as highly significant within mathematics, but I do not regard them as theories of the foundations of mathematics” (p. 32). Hahn again recalled Russell's approach but rejected his absolute realism: “I now assume, like Russell, that for describing the world (or better a selection of world) we have at our disposal a system of predicative functions, etc. – though, unlike Russell, I do not believe that the predicative functions are something absolutely given, something we can point out in the world” (p. 35).

Hahn's position on mathematics is crucial for understanding the discussion on mathematics by the *Kreis*, but it was not the “one and only” position that it took up on this matter. For example, Rudolph Carnap did not fully share it. In 1931 he wrote to von Neumann: “My own conception is not that of Russell but rather rests on a combination of Russell's and Hilbert's

ideas: therefore it will also be affected quite strongly by Gödel's results" (Mancosu, 1998, p. 41)<sup>7</sup>.

The debate on the foundation of mathematics was complex, in fact, above all after the Königsberger congress (September 1930), during which Gödel announced his revolutionary results. We can deduce from some letters between Carnap and von Neumann that Carnap was extremely interested in Gödel's theorems: he "surely had difficulties with the technical details of the proof [but] the importance of Gödel's result was immediately clear to him" (Mancosu, 1998, p. 39). Von Neumann was also deeply influenced by Gödel, whose announcement of the incompleteness proof was "one of the major turning points of [his] intellectual life ... [and] it stole away at least half of von Neumann's mathematical *raison d'être*" (Mirowski, 1992, p.122). Menger was struck by Gödel's theorem as well. Herbert Simon (Simon, 1991) recalled in his autobiography that Menger had once told him that he began his career deeply interested in logics and in the fundamentals of mathematics, but after the publication of Gödel's theorem he had believed it impossible to give a rigorous foundation to mathematics. He himself wondered what was meant by mathematical certainty and never found the answer.

As said, the distance between Menger and the *Kreis* concerned mathematics and language. Menger did not share the *Kreis* members' view of mathematics as a set of tautologies that can be reduced to a logical-formal pattern entirely independent from actual reality. He rejected the idea of a single logic with rules on which mathematics is formed and suggested a variety of logics. He wrote: "So I finally arrived at the conclusion that the member of the Circle must be thinking of the reduction of mathematics to logic claimed in the *Principia Mathematica* in 1910 ... [but] I failed to see what the ill-defined use of the term 'tautology' in the provocative reference to mathematics as a system of tautologies substantially added to Russell's claim that mathematics can be reduced to logic". (Menger 1994, p. 141). Hence Menger in a certain sense endorsed Hilbert's endeavour to construct a sort of meta-mathematics. He wrote: "a mathematical theory, I emphasized, consists of nothing but transformations of precisely stated propositions into other propositions according to precisely stated rules, with freedom in the choice of the rules as well as of the propositions" (Menger 1974, p.111).

As regards language, Menger did not share the *Kreis*'s interest in Wittgenstein's *Tractatus*. In drafts of Menger 1994, there is a chapter entitled "Atomic proposition, tautologies and

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<sup>7</sup> The correspondence between Carnap and von Neumann on this matter shows that also the Hungarian mathematicians were deeply impressed by Gödel's results. In June 1931 von Neumann wrote to Carnap: "Thus I am today of the opinion that Gödel has shown the unrealizability of Hilbert's program; there is no more reason to reject intuitionism (if one disregards the aesthetic issue, which in practice will also for me be the decisive factor)" (Mancosu, 1998, p. 40).

language”, in which he wrote that “the *Tractatus* as such was no longer on the agenda; but obviously the book was very much on the minds of Schlick and Waismann, somewhat less on Hahn’s and Carnap’s, and only Neurath was in opposition”. But, he added, “all members, however, had participated in a sentence-by-sentence scrutiny of the *Tractatus* a year or so earlier” and “a jargon had developed with which I was not familiar”<sup>8</sup> (see also Menger 1982, p. 86). Menger did not share the holistic idea of the unity of language, and he voiced his opposition to it at some meetings of the *Kreis*, although only Gödel agreed with his position. He wrote: “I objected to the recurring references in the Circle to *the* language and repeatedly asked Carnap, Schlick and other members what justified the implied belief in the uniqueness of language. But on this point, too, I failed to receive a satisfactory answer. Schlick did not seem to take the question seriously” (Menger 1994 p. 141). Menger remembered how frequently Gödel and himself had attended Schlick’s meetings. At the end of one of them, during which the formal structure of language had been discussed, Menger said to Gödel: “today we have seen how little wittgensteinians are these Wittgenstenians and we kept silent”; Gödel’s reply was: “talking about language I am persuaded, I am amazed, of the fact that people never understand each other”<sup>9</sup>.

The distance between Menger and *Kreis* became manifest when the movement began to propagate the idea of a “Unified Science”. He saw this as “an inroad of a kind of metaphysics” that “originated with Waismann” (Menger 1974, p. 110). This ideal of unifying the sciences conflicted with “those schools of social science, then flourishing especially in Germany, which extolled intuition, cognition of essences, and consciousness of absolute values” (Menger 1974, p 111). Like every member of *Kreis*, Menger rejected “all verbiage about essences and the absolute”, but he “was in less than full sympathy with the unity movement” (ibid.), which he considered embedded in a monistic scheme entirely useless from a methodological point of view. He wrote: “Apart from a general aversion to monistic schemes, I feared that for the sake of a methodological monism some at least potentially useful procedures might be neglected or even discarded”, and he added: “Nor have I ever understood what particularly useful cognitive purpose would be served by such definitory unifications and separations even if they were to be achieved” (ibid p. 111- 112).

Menger’s distance from the *Kreis* seems extreme; and this was probably the main reason that led him to found his *Mathematische Kolloquium* in 1928. He wrote: “I tried to model the Colloquium a little after Schlick’s Philosophical Circle, though in some ways this ideal was of course unattainable” (Menger 1982, p 90).

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<sup>8</sup> Karl Menger Papers, Perkins Library, Duke University, box 22 (Vienna Circle materials).

<sup>9</sup> Cf. Menger (1994) pag. 210.

#### **4. Economics in the *Kolloquium*: the contribution by Karl Menger**

The *Mathematische Kolloquium* was the natural place for Menger's thought to develop. Mathematical inquiries were not its only concern: in 1935 Menger wrote a paper in Italian in which he explained the *Kolloquium*'s principal interests. It discussed geometry, mathematics and logics, but the social sciences, especially economics and ethics, soon became themes of great interest to it. Menger wrote: "in this *Kolloquium*, beyond studying the recent developments of geometry and logics, we are interested in the new applications of exact sciences to *sociological* problems" and in a note he added: "for example on the existence and unity of solutions for production equations in mathematical economics, see Schlesinger-Wald, *Ergebnisse* 6, p. 10", (Menger 1935, p. 327 [my translation]).<sup>10</sup> As Menger said (1979, p.2), the aim of his researches, all long his life, was to find a "*theory of the application of mathematics to science*" and among these sciences he included economics.

While still a student, Menger had been deeply interested in economic issues. In 1923 he edited the second edition of his father's *Grundsätze*. According to Hayek (1932), this second edition "remains in the form of voluminous but fragmentary and disordered manuscripts, which only the prolonged and patient efforts of a very skilful editor could make accessible. For the present, at any rate, the results of the work of Menger's later years must be regarded as lost" (Hayek 1932, p. 416). Frank Knight, editor of the first English translation of *Grundsätze* in 1950, agreed with Hayek.<sup>11</sup> Still today there is no English translation of *Grundsätze* 1923. Karl Menger did not share Hayek's and Knight's opinions. In his introduction to the second edition of the *Grundsätze*, he wrote that, in 1889, his father had judged his major work incomplete and said that it should be reformulated in order to combat the triumph of historicism in German post-Kantian philosophy.

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<sup>10</sup> Economic issues were treated as very important in *Kolloquium* from the beginning. I cannot agree with Golland (1996), who has maintained that past literature, Punzo's contributions in particular, "exaggerates the importance of economics in Menger's colloquium (p. 9).

<sup>11</sup> Contrary to Hayek, Karl Polanyi considered the second edition of *Grundsätze* to be fundamental for Menger's thought and the history of economic theory. According to Polanyi, the definition of economics as the science of the allocation of scarce means to given ends should be limited to only the kind of economics based on exchange (*Verkehrswirtschaft*). Menger was the first economist to realize that "economic" had another meaning: in the fourth chapter of *Grundsätze* 1923, he distinguished two kind of economics. The former, called *maximizing* or *economical* (*okonomisierende*), is founded on the postulate of scarcity. The latter, called *techno-economical* (*sparend*, or *wirtschaftend*), is founded on the physical character of production, free from any link with scarcity of means. After the marginalistic revolution, economic theory used the principle of scarcity as its only paradigm, achieving such outstanding results that the substantivist meaning of economics was abandoned. Knight translated the German word *wirtschaften*, (the substantivist meaning) with the English word *economizing*, which Menger restricted to the formal meaning of "economic". Polanyi (1971) emphasised that Menger had written the second edition of *Grundsätze* in order to restrict application of the formal meaning of economics only to Western economic systems founded on exchange.

In 1923 Karl Menger wrote his first economic paper, “Unsicherheitsmoment in der Wertlehre. Betrachtungen in Anschluss an das sogenannte Petersburger Spiel”, on the role of uncertainty in economics (Menger 1967). Hans Mayer, then the editor of *Zeitschrift für Nationalökonomie*, refused to publish it because of the excessive use of mathematical formulas. The paper was discussed in 1927 at a meeting of the Viennese Economic Society and provoked differing reactions. It was only published in 1934, when Oskar Morgenstern - according to Menger “one of the very few Austrian economists who were free from prejudices against mathematical methods in economics” (Menger 1967, p. 259) - was appointed editor of the *Zeitschrift*.

Menger addressed the paper to economists and psychologists, inviting them to find a general function able to describe the constant behaviour of agents and deviations from it. He started with the St. Petersburg paradox. This had been formulated at the end of the eighteenth century by Daniel Bernoulli, who was the first to investigate the meaning of the so-called “expected utility” for a gambler who persists in playing the same game. Menger was concerned with subjective probability and stressed that deviations of behaviour are very complex: even in a game with a finite number of solutions, individual choices may disregard mathematical expectations. Menger wrote: “what I regard as the essential point of that early paper is the introduction of probabilistic idea into this branch of economics, whereas Bernoulli had introduced an economic idea into the theory of probability” (Menger 1979, p. 6)

In the drafts of his *Reminiscences*, Menger recalls that only after the mid-1930s were mathematical economics considered in Vienna. Carnap was so deeply impressed by Menger’s analysis of the St. Petersburg paradox, that, twenty years later, he quoted it in his *Foundations of Probability*. Menger’s article indubitably influenced the subsequent reflections of von Neumann and Morgenstern on the axiomatization of utility in games theory (Kuhn and Tucker 1958), but, according to Menger, still persisting in both the Neumann-Morgenstern’s *theory of games* and Frank P. Ramsey’s article on probability is the idea that the choice with a higher mathematical probability is preferred. By contrast, Menger underlined the preponderant weight of uncertainty in economic behaviour.

In a letter of 18 September 1935, Ragnar Frisch wrote to Menger: “I am very happy to hear that you expect in the future to devote part of your energy to the problems of economics and the social sciences. There is a vast need for people, with a genuine mathematical knowledge and ability, taking up problems in this field. I am sure that we may expect highly significant

contributions from you”<sup>12</sup>. As we shall see, Menger’s efforts and researches in mathematical economics were focused on the axiomatization of economic theory, or better, those aspect of economics to which mathematics could be applied according to an meta-mathematical model *à la Hilbert*. His *Kolloquium* became the place in which the formalization of the GEE model came about.

In 1936 Menger published “Bemerkungen zu den Ertragsgesetzen”, which was translated in English in 1954 as “Remarks on the Law of Diminishing Returns. A study in Meta-Economics”, and according to Schumpeter (1954), “a shining example of the general tendency towards increased rigor that is an important characteristic of the economics of our own period” (p. 1037). He wrote the paper in answer to Ludwig von Mises’s claim “that certain propositions of economics can be *proved* [and] as an example he mentioned the law of diminishing returns and referred me to the literature for the proofs” (Menger 1979, p. 279); after having read the paper, Mises wrote Menger “that he learned a great” from it. The meaning of the title is explained by Menger himself: “following a suggestion of Hilbert, modern logicians refer to the study of the logical relations between the statements of a theory as the corresponding *meta-theory*. In this terminology, the contents of the present paper can be described as a chapter in *meta-economics*” (ibid. p. 280). The paper was indented “to familiarize economists (especially of non-mathematical schools) who claimed to *prove* certain assertions about returns, with what logicians demand of proofs” and “to present a method that economists (and indeed all social scientists) interested in proofs of quantitative laws may find useful in checking conjectures: the method of demonstrating that a proposition  $\beta$  is *not* a consequence of a proposition  $\alpha$  by exhibiting a function that satisfies  $\alpha$  but not  $\beta$ ” (p.6).

According to Menger, the proper methodology of economics complies with the following three rules:

- “1. By saying that a proposition does not follow from certain assertions, we do not mean that it is not valid ...
2. By saying that a proposition does not follow from certain others, we do not mean that it is valid ... Only experience can show whether or not the derived proposition is valid.
3. The crucial issue for economics to be whether or not is not these laws are empirically confirmable” (ibid)

Menger then considered the law of diminishing returns to land first formulated by Eugen Böhm-Bawerk: “additional applications of capital and labor on a piece of land increase the

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<sup>12</sup> Ragnar Frisch to Karl Menger, 18 September 1935, in Karl Menger Papers, Perkins Library, Duke University, Durham, N.C, box 2 (Correspondence).

total product, but after a certain point this output increases relatively less than further costs. In other words: like increases of cost produce a decreasing increase of the product” (p. 281) [A].

From this law we deduce that:

- a.* “an increase of cost outlay yields a smaller increase of the product when added to a larger outlay than when added to a smaller outlay (it depends on the amount of land used)”;
- b.* “as the cost outlay rises, the average product of every piece of land falls after a certain point has been reached

According to Menger *a.* and *b.* are two formulations of the same law (both derived from the first one), *but* – as Menger underlined – “hence far from being equivalent, neither of the two laws implies the other” and it can be shown “by elementary algebra”; hence we wonder “which of these two propositions do economists mean when they speak of the law of diminishing returns”).

Let us compare these two laws (*a.* and *b.*) with the above methodological scheme (1. 2. 3.). Neither *a.* nor *b.* are logical complements of the other, even though they both derive from [A]; the validity of *a.* does not imply the validity of *b.*, and vice versa. The validity of both depends on empirical testability; in particular it depends on additions to *large* or to *small* outlays: “both laws deal with product increments yielded by additional outlay – but to sufficiently *small* outlays in one case and to sufficiently *large* outlays in the other” (p. 289).

Then, two laws can be reformulated thus:

- a.* “on every piece of land and for every outlay  $y_1$ , a fixed additional outlay,  $h$ , yields a larger product increment when added to  $y_1$  than when added to a sufficiently large outlay”;
- b.* “on every piece of land, the average product of any given outlay is greater than the average product of any sufficiently large outlay” (p. 290).

Menger verified that *a* (the law of diminishing product increments) is not a consequence of *b* (the law of diminishing average product), but also that the contrary is valid. And he concluded: “From the point of view of methodology, the present paper can perhaps lay claim to being 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). This was, Menger believed, the key point in transforming economics into a science, testifying its scientific nature.

##### ***5. Economics in the Kolloquium: the reformulation of the GEE model***

The members of the *Kolloquium* concerned with economics were Abraham Wald and Karl Schlesinger. Oskar Morgenstern and John von Neumann took part, even though the former never considered himself a “formal member of either group [*Kreis* and *Kolloquium*]” (Morgenstern 1976, p. 806) and the latter attended the seminars during his trips from the USA to Europe after his appointment as visiting professor at Princeton in 1931.<sup>13</sup> Wald, Schlesinger and von Neumann were all engaged in the mathematical reformulation of the GEE model. In 1964, Kenneth Arrow wrote to Karl Menger asking him about “the intellectual relationship between Wald’s first paper on existence of equilibrium and von Neumann’s article on an expanding economy”. He wrote: “[As von Neumann] understands the replacement of equations by inequalities along Zeuthen’s lines, whereas clearly the problem he raises, that of a uniformly progressive economy, is derived from another section of Cassel’s book ... did von Neumann arrive at this formulation independently of everyone else? Or did he derive it from Schlesinger or Wald? Or is it too late ever to find out?” The meaning of Arrow’s questions was that according to him: “the really interesting point is that von Neumann’s mathematical methods, introduced in that paper, actually enable one to improve greatly on Wald’s proofs, giving both more general and more elegant results”<sup>14</sup>. Many years later Arrow wrote that Wald and von Neumann “make no reference to each other, despite very close apparent links” (Arrow 1989, p. 16).

The relationship between Wald’s articles and von Neumann’s was undoubtedly close. They both tried to solve analytical problems still unresolved in the Walrasian model, which Wald and Schlesinger reformulated according to the one developed by Cassel (1918). Wald dealt with the issue in four papers written between 1934 and 1936 (Menger 1952; Koopmans 1964; Weintraub 1983). Menger wrote in this regard: “Here [in Wald’s contributions], for the first time, economic equations were not merely formulated. The number of equations was not merely compared with the number of unknowns. The equations were solved” (Menger 1952, p. 18). For the first time, Wald had solved the mathematical problem of the existence and the uniqueness of economically meaningful solutions in an equations system. Wald wrote: “in mathematical economics, certain processes .. are described by systems of equations in which certain economic magnitudes are used as data and others as unknowns. As a rule, economists have contented themselves with equating the number of equations and unknowns and have assumed, without further investigation, that the system of equations had a meaningful solution from an economic viewpoint, and that this solution was unique. But the equality of the number of equations and un-knowns does not prove that a solution exists, much less the

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<sup>13</sup> According to Punzo (1989), we may consider von Neumann “as a member *in pector*” of *Kolloquium*” (p. 47).

<sup>14</sup> Karl Menger Papers, Perkins Library, Duke University, Durham, N.C., box 2 (Correspondence).

uniqueness of a solution” (Wald 1935 p. 369-370). Wald used traditional mathematical instruments (differential equations and inequalities), bearing in mind the importance in economics of investigating the reality of the initial hypothesis.

Von Neumann’s paper on GEE was presented at a Princeton seminar in 1932 and was then discussed by Menger’s *Kolloquium* in 1937. It finally appeared “as the last paper of the last *Ergebnisse*” (Weintraub 1983, p. 13). It was regarded as “a paper that has greatly influenced economic theory up to the present time, and of which all the ramifications have perhaps not yet become fully apparent” (Koopmans, 1964, p. 355) and as “the single most important article in mathematical economics” (Weintraub, 1983, p. 13). In this paper, von Neumann formalized the GEE model with three important differences with respect to Wald’s: he described an economic system without the utility function ; he used different mathematical tools; and he ignored the problem of realism of hypothesis.

There were two “properties” of von Neumann’s model (von Neumann, 1945): goods are produced by each other, in a sort of circular process, and the number of technical production processes may be higher than that of goods (this being the main reason why “counting of equations is no avail”). Four solutions were sought by the model: “to establish which processes will actually be used and which not”; what is the relative velocity at which the total quantity of goods increase; “what prices will obtain”; and finally what the rate of interest will be. In order to obtain these four results, von Neumann used the following “idealizations”: constant returns; the fact that natural production factors (including labour) could be expanded in unlimited quantities; “all income in excess of necessities of life will be reinvested”. These idealizations were abstractions and they distanced von Neumann’s GEE model greatly from the need to bridge the gap between model and reality that Menger and Wald had deemed so urgent.

The quantities used ( $a$ ) and the quantities produced ( $b$ ) of the goods ( $G$ ) on which production processes ( $P$ ) depend are given, and they must obviously always be non negative. The unknowns are four in number and are: the “intensities” ( $x$ ) of the production process ( $P$ )<sup>15</sup>; the coefficient of expansion of the whole economy ( $\alpha$ )<sup>16</sup>; the prices ( $y$ ) of goods; the interest factor ( $\beta$ ). These are connected into four equations (two equations and two inequalities). The coefficient of expansion of the whole system ( $\alpha$ ) depends on the intensity ( $x$ ) of production processes; the rate of interest ( $\beta$ ) depends on prices ( $y$ ). According to von Neumann, “the fact

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<sup>15</sup> BY “intensities” von Neumann meant the factors that multiply the production process  $P$ : if  $x=0$ , this means that  $P$  is not used.

<sup>16</sup> Von Neumann wrote: “We are interested in those states where the whole economy expands without change of structure, i.e. where the ratios of intensities  $x_1 \dots x_m$  of the process  $P_1 \dots P_m$  remain unchanged, although  $x_1 \dots x_m$  themselves may change” (Neumann, 1945, p. 2).

that the number of conditions is equal to the number of unknowns does not constitute a guarantee that the system can be solved” (von Neumann 1945, p. 3). The possible solution furnished by differential equations (as in Wald’s model) was not appropriate in such a system: according to von Neumann, “the mathematical proof is possible only by means of a generalisation of Brouwer’s Fix-Point Theorem, i. e. by the use of very fundamental *topological* facts” (ibid. p. 1). Using convex sets, von Neumann introduced a minimax solution as a mathematical instrument with which to prove the verifiability of the GEE model. On reaching equilibrium there are two results: “The greatest (purely technically possible) factor of expansion  $\alpha'$  of the whole economy is equal to  $\alpha$  and to  $\beta$ , neglecting prices [(y)]” and “the lowest interest factor  $\beta'$  at which a profitless system of prices is possible is equal to  $\alpha$  and to  $\beta$ , neglecting intensities of production [(x)]”. Von Neumann claimed at the end of his paper that this solution for the GEE model is proved “only on the basis of our knowledge that solutions of our original problem exist – without themselves directly referring to this problem”; and he added: “the equality of the maximum in the first form and the minimum in the second can be proved only on the basis of the existence of this solution” (ibid. p. 9). This conclusion underlines – like the “idealisations” at the beginning of the paper – the distance of the model from reality. They both increased abstraction of the GEE theorem of existence to such an extent that the reaction of economists was immediate. According to Champernowne (1945): “[von Neumann’s assumption’s] although necessary if a rigorous proof of the existence of equilibrium was to be possible, evidently render the model unsuitable” for examining real economy; and he added: “the reader may begin to wonder in what way the model has interesting relevance to conditions in the real world”, (Champernowne 1945, p. 12). The realism of hypotheses and the correspondence between reality and the mathematical model (still present in Wald 1935 and Menger 1936) were definitively abandoned in von Neumann’s demonstration of the theorem of the GEE’s existence. As we have no reason for doubting that von Neumann’s model was the final outcome of the *Kolloquium*’s mathematical economics, we may say that the result was a turning point in the *Kolloquium* and in the history of economic theory. According to Dore (1989), von Neumann’s technique (the introduction of convex mathematical sets and of hyperplane “as the theoretical basis for prices – a task essentially completed by Arrow and Debreu” - p. 93), made it possible to reformulate Walras’s GEE model by replacing Cassell’s principles of scarcity with the principle of marginal utility. In this sense, if we accept the idea that von Neumann’s model was the last result in economics at the *Kolloquium*, we can define its economic theory as neo-Walrasian (Weintraub, 1983). Nevertheless, this definition

presumes that the GEE model is an issue that concerns the Walrasian tradition alone. But the problem of finding a GEE model was obviously still present in another tradition, the Ricardo-Bortkiewicz-Remak-Sraffa's tradition (from which von Neumann borrowed the circular nature of the system).

### **6. Karl Menger on the relation between mathematical economics and Austrian school**

It is difficult to talk about economics in Vienna in those years without considering the Austrian school. Menger himself wrote a well-known paper in which he argued for continuity between mathematical economics and the Austrian school (Menger 1983); but this hypothetical continuity is highly problematic.

The misunderstanding arises from Karl Menger's memories of those years set out in his writings. In Menger 1983, he recalls that "two souls were in his breast": he was a mathematician, but he was also the son of one of the most important economists in the history of economic theory. His particular and personal situation induced him to find a possible point of contact between mathematics and economics. According to Karl Menger, Austrian and mathematical economists never understood each other properly. They agreed on many fundamental economic issues, although they used different forms of expression: the Austrians used common language, while the mathematicians used formalization. Menger wrote that Irving Fisher (in *Mathematical investigations in the Theory of Value and Prices*, 1892) was the first economist to maintain this. The American economist claimed that even the laws of physics could be enunciated by two kinds of languages - common and mathematical. In order to show that Fisher's assertion was pertinent to Austrian and mathematical economists as well, Menger examined the principle of marginal utility, for which the Austrian definition was: "for each good, the utility of a larger quantity is greater (or at any rate not less) than that of a smaller quantity, whereas the marginal utility of the larger quantity is less (or at any rate not greater) than that of the smaller" (Menger, 1983 p. 39). The mathematical formulation is instead: if  $q$  denotes the quantity of a good and  $u$  its utility then:

$$u = f(q) \quad du/dq = f'(q) \geq 0, \quad \text{and} \quad d^2u/d^2q = f''(q) \leq 0.$$

According to Menger, some mathematical economists believed that these formulas expressed more than simple words could, and furthermore that they described the situation more precisely. Menger claimed that this was based on a misunderstanding.: far from saying more, these formulas actually say less than the Austrian formulation, because they express the same assertion under an additional, but tacit, hypothesis, viz. the assumption that the function

connecting utility with quantity admits a second derivative, an additional hypothesis that clearly is not anchored in economic facts. The Austrian formulation of decreasing marginal utility is more general because it is valid even if there are places where the function does not admit a second derivative.

Menger pointed to a further error committed by many mathematical economists: from Cournot onwards, they used tacit assumptions, in the theories of return, supply, demand, and so on, by assuming the continuity and differentiability of functions as though these properties were matters of course, whereas they are nothing but prerequisites for the application of classical analysis and are not based on facts. They justified this approach as an advantage rather than an obstacle. This non-verifiability of initial hypothesis was condemned by Menger, as we saw when examining his economic papers. Menger suggested that the following examples should be considered. If “to a higher price of good, there corresponds a lower of it (or at any rate not a higher) demand” and “if  $p$  denotes the price of, and  $q$  the demand for a good” (ibid. p. 41), then:

$$Q = f(p) \text{ and } dq/dp = f'(p) \leq 0$$

This formula is not more precise than sentence above: “the only difference between the [sentence] and [the formula] is this: since [the formula] is limited to functions that are differentiable and whose graphs, therefore, have tangents (which from an economic point of view are not more plausible than curvature), the sentence is *more general*, but it is by no means less precise; it is *of the same mathematical precision*” (ibid.).

$$q = f(p) \text{ and } dq/dp = f'(p) \leq 0.”$$

It is a misapprehension to regard the formula as more precise than the sentence. The only difference between them is this: since the formula is limited to functions that are differentiable, the sentence is more general, “but it is by no means less precise; it is of the same mathematical precision as the formula”.

The Austrian concept of *Grenznutzen* leaves something to be desired in terms of precision, but not the Jevonian concept of final degree of utility. Jevons defined it as “the degree of utility of the last addition or the next possible addition of a very small, or infinitely small, quantity to the existing stock”. “Since by degree of utility Jevons means the ratio of utility to quantity, in modern terms his final degree ... is either  $du/dq = f'(q)$ , i.e. the limit of the difference quotients

$$\frac{f(q+h) - f(q)}{h}$$

as  $h$  gets arbitrarily small, or the quotient itself for the smallest usable increment  $h$ ". This relation can be applied to Wieser's concept of *Grenznutzen* only if  $h=1$ . Austrians, however, "not only steer clear of the concept of calculus ..., [but] in keeping with their ordinal conception of utility, they even eschew arithmetical operations such as dividing or multiplying a utility by a number". "The problem thus arises whether the Principle of Marginal Utility can at all be stated in conformity with those Austrian restrictions. This is indeed possible and even in a form that is valid both for Wieser's marginal utility and for Jevons's final degree" (Menger 1983, p. 42). The utility function must be non-decreasing and convex, i.e. it must satisfy the condition

If  $a < b < c$ , then

$$f(b) \geq b \cdot \frac{f(c) - f(a)}{c - a} - \frac{af(c) - cf(a)}{c - a}.$$

Menger notes that, "while this definition of convexity is altogether independent of differentiability", the equation requires a quantitative definition of utility and thus "fails to meet one of the Austrian requirements" (ibid., p.43). He then introduces two "ordinal generalizations":

First ordinal generalization: "If two quantities of a good are both increased or both decreased by the same amount, then the utility of the greater changes less (or at any rate not more) than that of the smaller. In symbols,

$$\text{If } x_1 < x_2, \text{ then } |f(x_2 + h) - f(x_2)| \leq |f(x_1 + h) - f(x_1)|."$$

Second ordinal generalization: "For any quantity of good, the increase in utility due to the addition of an increment to the quantity is less (or at any rate not greater) than the loss of utility due to the subtraction of the same amount. In symbols,

$$f(x + h) - f(x) \leq f(x) - f(x - h) \text{ for each } x \text{ and } h \text{ (even for } h < 0\text{).}"$$

Thus "one can formulate in the Austrian fashion ... general principles without using, or referring to, any ideas of calculus" (ibid. p. 44). And so – Menger says in conclusion – even the Austrian marginalistic theory of utility can be translated into mathematical terms (see also Morgenstern 1931, who was of the same opinion).

Nevertheless Menger wondered why Austrians never used mathematics. He found an answer in the correspondence between Leon Walras and Carl Menger: while for the Lausanne economist, mathematics is *the unique* means of research, for the Viennese it is only a method of description. According to Karl Menger, the Austrians tried to find the *causes* of economic phenomena; while mathematical economists described their functional relations as in Mach's model. According to Menger, the reason for the difference was that the Austrians' aim was to

know the *essence* of economic phenomena, while mathematic economists “pay[ed] less attention than the Austrians do – perhaps sometimes not enough attention! – to the definition of concepts represented by symbols” (ibid. p. 52-53). Menger seems very self-contradictory in respect of what he had said previously: he maintained the adherence between Austrian economic model and mathematical economics, complaining about only a simple difference in language. Nevertheless his conclusions went beyond a simple, even if important, point: Menger (wrongly) considered Austrian economic theory to be embedded in a realistic epistemological model (based on *causes* and *essences*) while mathematicians adopted a functionalistic one. This seems to be somewhat naïve: The Austrian economists surely shared the second epistemological and methodological model (from Carl Menger’s struggle against Schmoller’s historical school until Schumpeter’s theory and Hayek’s studies).

### **Conclusions**

The relation between the *Kreis* and the *Kolloquium* was very complex. Otto Neurath introduced economics into the *Kreis* during the 1920s. At the end of the 1920s, the *Manifesto* gave economic theory a specific role in the neoempiricist program: it was a social science that could be treated by a new epistemological model, and Carl Menger was regarded as a forerunner of this new approach. The *International Encyclopaedia* incorporated economic theory into the project for a unified science. In the early 1930s in Vienna, the rupture between the *Kreis* and the *Kolloquium* (on mathematics, language and encyclopaedic ideal of science) had profound consequences also for economic theory.

According to Menger, the *Kolloquium* was the result of three different intellectual influences: the *Kreis*, Hilbert’s formalism, and Austrian economic school. This is true, but only in part. The *Kreis*’s empiricism was undoubtedly fundamental (if we consider it as a reaction to German neo-Kantianism, against which both the *Kreis* and the *Kolloquium* were hostile). It is also true that the *Kolloquium* mathematicians accepted Hilbert’s paradigm (as did Menger and von Neumann - although they were aware of the implications of Gödel’s results on it - while Hilbert’s formalism was rejected by the *Kreis*, and Gödel’s theorem was simply ignored in the development of the movement). Finally, the Austrian tradition (in particular Carl Menger’s works) was the most important reference in the *Kolloquium* and was well regarded by the *Kreis* as well.

Nevertheless, the *Kolloquium* broke with the *Kreis*’s physicalism. During the 1930s a major cleavage opened up between the *Kolloquium* and the *Kreis*: the former used an axiomatic-deductive method with a refined formalization also to deal with economics; the latter took an empirical experimentalism from physics. The *Kreis* scientists wanted to apply experimental

models in economics and their interests were directed to econometric methods; the mathematicians of the *Kolloquium*, headed by Menger, formalized the GEE model.<sup>17</sup> This model was not a development, but rather as a renunciation of the Austrian economic model and of the *Kreis's* project, which centred on the encyclopaedic ideal of a unified science. Punzo (1991) has rightly claimed that the relation between mathematics and economics during 1930s did not mediate by physics, and that Wald's and von Neumann's economic model arose from this new experimentalism-free approach. As regards the Austrian school of economics, its members seemed to be closer to *Kreis's* point of view than the GEE model of Schlesinger and Wald, who, not by chance, reformulated Walras' theory and not Carl Menger's.

The distance between mathematical economists (*Kolloquium*) and experimentalist economists (*Kreis* and econometricians) is highlighted by events in Menger's life that can be reconstructed from his correspondence. For example, in 1935, he declined to contribute to *Econometrica*, whose editor, Ragnar Frisch, wrote to him (in the same letter as quoted in § 4): "If you come into this field [mathematical economics], may I take this opportunity of asking you to give me as editor of *Econometrica* a chance of seeing your MSS., if you complete anything about applications of mathematics to economics"<sup>18</sup>. Menger never published in *Econometrica*. He also refused to write "Mathematics" for Neurath's *Encyclopedia* and attend the congress for the Unity of Science (held in Paris, 1937), as we read in a letter of 14 March 1937 where Neurath criticised Menger for refusing to be a member of the Advisor Commission for the *Encyclopedia*.<sup>19</sup> Even many years later, Menger declined to join the Institute for the Unity of Science, writing a paper on mathematics for it.<sup>20</sup> After the mid-1930s the rupture between the Viennese physicians (and social scientists) and mathematicians was complete.

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<sup>17</sup> Nevertheless, considering the formalization of the Walrasian model as the final approach to economics mean that we accept the maxim according to which "once understood the rule, then we bow to paradigm". The quotation is from S. Kierkegaard *The Diary of Seducer*, [Milano, Rizzoli, 1994, p. 93]. The rule is the art of aesthetic loving and the paradigm is Don John, the seducer; here the rule is the formalization of Viennese mathematicians and the paradigm is GEE, of course.

<sup>18</sup> Ragnar Frisch to Karl Menger, 18 September 1935, in Karl Menger Papers, Perkins Library, Duke University, box 2 (Correspondence).

<sup>19</sup> Otto Neurath to Karl Menger February, 19, 1937 and March, 14, 1937, in Karl Menger Papers, Perkins Library, Duke University, box 2 (Correspondence).

<sup>20</sup> In 1952 Philip Frank send him a letter in which he asked him: "perhaps you would be interested in writing a small book on the philosophical interpretations of mathematics. The books should be on such a level that they are understandable for all people with an average college education and a certain interest in science and philosophy. It should be of a length of about 250 pages". Philipp Frank to Karl Menger, June, 4 1952, in Karl Menger Papers, Perkins Library, Duke University, box 2 (Correspondence).

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