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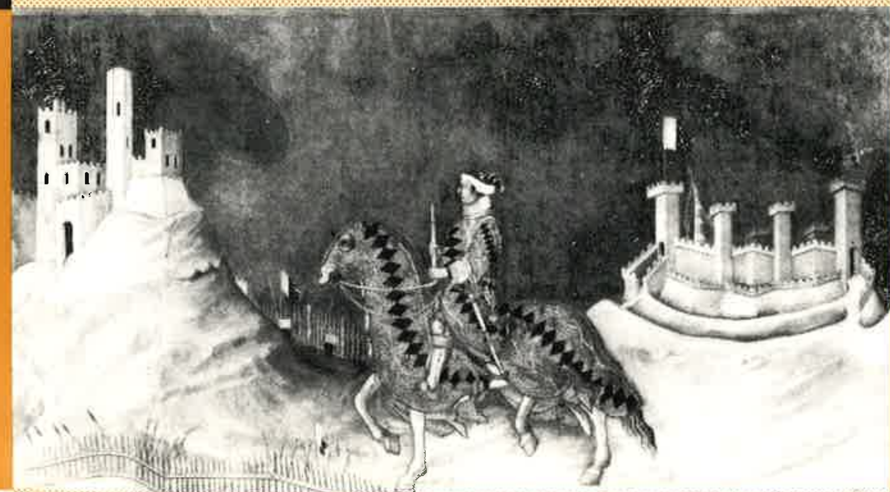
**QUADERNI DEL DIPARTIMENTO DI ECONOMIA POLITICA**

Lionello F. Punzo

# HILBERT'S MATHEMATICAL FORMALISM AND THE NEOCLASSICAL REVOLUTION



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Lionello F. Punzo

# HILBERT'S MATHEMATICAL FORMALISM AND THE NEOCLASSICAL REVOLUTION

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INTRODUCTION.

The term "revolution" normally stands to indicate a sharp change in the direction of evolution of a given phenomenon, a "catastrophe" in the jargon of modern dynamic theory. In this sense we often talk of the Newtonian revolution in physics or of a Neoclassical revolution in economics. In this paper, I suggest that a revolution in the history of economic thought was <u>not</u> the one brought about by Jevons, Walras, Marshall, Menger (and some others). Rather, it was the subtle but profound revolution initiated by the group frequenting the Vienna Mathematical Colloquium led by the mathematician Karl Menger, who re-examined the "classical" general equilibrium model of Léon Walras and Gustav Cassel[1]. The setting for the intellectual origins of the "catastrophe" in the evolution of economic thought constituted by the change from "classical" to modern General Equilibrium, was one of the most interesting and lively Mittel-European milieus of the thirties. I shall use the collective name "Viennese" to indicate this circle which included economists (e.g. Oskar Morgenstern, Karl Schlesinger), mathematicians (K. Menger, Abraham Wald, partly John von Neumann) and logicians (Kurt Gödel). They formed a unique blend of different expertise in an equally unique cultural environment.

## I. REVOLUTION OR "CATASTROPHE"

The singularity point at which the discontinuity occurred was the issue of establishing the existence of an economic equilibrium. Three questions were addressed. What is to be understood as an equilibrium state? What does it mean that "it exists"? How should one go about proving such a statement, if it is worth proving at all?

In economics, there are two recognized meanings attached to the notion of equilibrium. There is a substantive usage, that refers to the notion of opposing, balancing forces, objectively conceived, which originates in classical mechanics. But there is also the notion of "the possibility of reconciliation of individual independent choices" (Weintraub 1979). It was the latter that historically prevailed. The former has been translated into the property of independence of juxtaposed blocks of formal relations. The balancing of opposing forces was turned into the possibility of logical (in)consistency of "partial systems" (Weintraub 1985).

Resulting from fairly recent intellectual and historical events, the appearance of the latter or "modern" notion of equilibrium marked a dramatic change in the understanding of the heuristic role of symbolic calculus as an analytical technique in the applied sciences. To render the change acceptable to an otherwise hostile profession, the very basic object of the analysis, the "social economy", had to be redefined to make it coextensive with its formal representation. This, in its turn,



was proposed as an array of ordered mathematical symbols, strung together by the logical necessity implied by the two processes of translation from an informal into a formal language <u>and</u> by stringent deductive reasoning. Finally, it was suggested that the only "scientifically safe" way to conceive the "description of the system" requires stripping it of all factual and intuitive appeal and taking it in its poorest semantic sense. It has to be treated as a <u>closed</u>, <u>self contained formal system</u>, or a kind of <u>logical toy</u>, as it would typically be treated by a certain contemporary school of Mathematics and Logic.

This <u>methodological</u> conception is now basic to the <u>modern</u> version of the General Equilibrium approach (henceforth, simply GE), *Debreu style*. In the present reconstruction of its intellectual origins (admittedly, only one of the many conceivable reconstructions), such a conception is borrowed from an intellectual milieu foreign to economics. It carries the distinct imprint of the domineering aspirations that such milieu entertained at a certain point in its history, at what appeared for a short time to be the peak of its success.

## II. GENERAL EQUILIBRIUM AS THE FUNDAMENTAL THEORY

What is commonly called General Equilibrium is neither very clear nor agreed upon: is it an "analysis", a "theory", a "sequence of models", a "metatheory"? Nevertheless, everybody seems to agree that: "GE is the fundamental economic theory". This can be taken to reflect the trivial principle, according to which in economics (like perhaps in all sciences) one expects that everything depends on everything else. GE supplies a working application of this principle of universal interdependence.

However, I share the view expressed by Weintraub (Weintraub 1979, 1985) that GE can be "fundamental" in a non trivial sense only if it is "the theory for all economic theories", in other words the metatheory appropriate to economics[2].

A metatheory is best understood as an empty container, i.e. a set of instructions as to (i) how to select undefined or primitive terms; (ii) how to combine them in well formed formulas; (iii) how to obtain true propositions in the form of theorems via deductive reasoning. In other words, a metatheory is a handbook on how to handle "scientific machinery" correctly. On the other hand, there can be a unique metatheory for a whole set of theories, which belong to the different and lower (applied) levels of theorizing. Models (which are identified with "theories", in this jargon) are derived either via reinterpretation of primitive terms or via alternative, sometimes weaker, sets of (specific) assumptions.

As a metatheory, GE provides a broad framework within which each model is a special structure to be individually understood at some stage of its development. The functional role of GE within the architecture of economic theorizing can be expressed, by resorting to modern computer language, as one of an all-purpose, programming language in relation to application programs. Specific models (e.g. the "model of a private ownership economy", the "model of a perfectly competitive economy", etc.)



are, therefore, classified according to the special assumptions, mechanisms etc. that identify them with respect to the general approach. In a sense, they are derived from the latter via "degeneration", or as special cases.

The notion of a metatheoretical level of analysis and the related methodological outlook surely represented a novelty of major momentum in the history of GE and more generally in the history of economic thought. It is fair to say that there was no metatheory either for Walras or for Pareto, for instance, nor had any need for one ever been felt before. But GE did not evolve into a metatheory as the result of historical "accidents", as Debreu suggests (Debreu 1984). The evolution was rather the outcome of a sequence of logically connected "events" which were parts of a still semi-articulated or embryonically conceived plan. This plan aimed at redesigning GE from its very foundations.

Briefly, it starts with the assertion that GE should be concerned with classes of formal representations (each class having a representative in a "generic model"). Then, a generic model is constructed by putting together (i.e. by literally assembling) blocks of logically distinct mathematical relations ("partial systems"), each deriving from independent hypotheses or assumed laws on the system and/or individuals' behaviors and, possibly, from additional requirements of (systemic) consistency. It is a consequence of the above conception of economic interdependence that any model must incorporate a mechanism of simultaneous determination of all its endogenous variables treated as unknowns. This hides a deception, however. In fact, given the peculiar nature of its structural relations, such a mechanism can at most mimic a proper computational algorithm: as a computing device, the model in which it is embedded is incomplete if not misspecified. Thus, the "determination of the values of the unknowns" is taken in the abstract sense of logical implication and deductive cogency; it has no longer that "causal sense" that was, rightly or wrongly, traditionally attributed to the results obtained by differential calculus and systems of simultaneous equations. It is in the latter sense that it was used, for instance, by Walras and even more explicitly by Knut Wicksell (with a number of ambiguities); but also by Cassel.

In the Mathematics of the beginning of the century, the study of the properties of solution sets of mathematical problems and of solution procedures had largely yielded to the study of generic properties of whole classes of mathematical entities and of the relations that could be meaningfully established among them. Likewise, in the program of redesigning economics initiated in Vienna, the use of mathematics as a tool to attain, at least in principle, exact measurability (and quantitative predictability) of the values of economic variables yielded to the logical calculus: a model was reduced to a manipulation of essentially symbolic strings. As such, it could only be subjected to a logical (i.e. non immediately empirical) significance test. Thence, an unprecedented emphasis was placed upon qualitative existential statements.

We recognize (in all this) many of the claims of modern GE. Where do they come from if not from Hilbert' Mathematical Philosophy?



## III. MATHEMATICS AND MATHEMATICAL ECONOMICS

The weight attributed in my interpretation to the influence exerted on economics by a German mathematician, David Hilbert, may seem excessive[3]. In fact, I maintain that what I called "a catastrophe" in the history of economic analysis is essentially the result of the influence of the mathematical philosophy called Mathematical Formalism that was heralded by Hilbert from his stronghold, Göttingen. This philosophy provided the two pillars: the axiomatic method, and the principle of hierarchical interdependence between a plurality of theories and the unifying uniqueness of the metatheory behind them.

From Hilbert's own special version of the axiomatic approach, modern GE derives: i) the notion of economics as the analysis of "formal systems" rather than synthetic representations of actual economies; and ii) the necessity of a metatheoretical level of inquiry (i.e. of GE), where the "rules of the game" are explicitly stated and justified and, moreover, existential statements are established for their twofold role: model validation and the theoretical explanation of endogenous variables (Debreu 1959). Paraphrasing Imre Lakatos[4], the subject matter of GE has therefore become an abstraction of economics. From Nicholas Bourbaki (who, for the limited purpose of the present argument, can be treated as a follower of Hilbert[5]), is derived the conception of the set of the theories as a space "linearly" constructed by adding models together, and unified by certain specific model-building principles. Essentially, these metatheoretical principles state that "theories" are "models" and "models" are logical "structures" to be internally understood in their own logic, rather than in their relation to an intuitively depicted reality. Applicative models are unified under the general and abstract notion of a canonical cognitive procedure regulated by "general laws". Finally, as in Logical Positivism, to which Mathematical Formalism is the "bulwark" (Lakatos 1976), any theory within the GE framework is characterized by at least one but possibly many, non exhaustive partial interpretations. This set of "empirical meanings" is both temporally and spatially expandable, a view implicit in the work of Hilbert but explicitly expressed for the first time by von Neumann in a different context, i.e. in his view of Quantum Mechanics as a partially interpreted theory[6].

I maintain that it is wrong to speak of the axiomatic approach as opposed to the descriptive or the historical approaches. It is furthermore misleading to identify one a specific version of the axiomatic approach, taken as if it were the only one available, with the unique axiomatic approach.

In the recent historical evolution of economics a peculiar blend of ideas belonging to Mathematical Formalism has prevailed, a blend which looks in many ways like a compromise to cope with Gödel's criticism of the Formalistic Program[7]. If this is true, the history of modern economic analysis, and thus also the history of the various notions of equilibrium, could be usefully reconstructed with an eye to the history of mathematics, rather than to the history of physics or other applied sciences. This viewpoint has recently been considered by some authors (e.g. Bruna Ingrao and Giorgio Israel 1990; Philip Mirowski 1986; Weintraub (1985)). In my view, however, the nature of the relationship of economics to mathematics and how much it has changed in this century, have not yet been fully appreciated. It used to be essentially mediated by physics; from the thirties it became one of immediate and hierarchical dependence on mathematics. A second wave of imports into economics from "abroad" was witnessed at that time, but this time the inflow of ideas came directly from mathematical philosophies and the debate on the foundations of mathematical truth. Moreover, it arrived "in real time", without any time lags. This was due mainly (though not uniquely) to the interest shown by mathematicians of the Formalist School (Hilbert himself, to begin with) in finding applications for their approach in the various fields of the applied sciences[8]. Obviously, such borrowing of ideas and analytical techniques was not at all new in the history of modern science. But, in the end, the real novelty was in the peculiarities of the mathematical approach proposed as an "all-purpose heuristic machine".



This interpretative hypothesis sheds light on otherwise incomprehensible features of the process of revision of the general equilibrium model that started in the thirties and eventually led to the modern canonical version: the "Arrow-

-Debreu-McKenzie model". Moreover, it accounts for the rapid establishment of an approach to the mathematization of economics that emphasized logical foundations rather than the more numerically oriented techniques, so important in traditional GE theories. Even seemingly neutral analytical issues were historically exploited as vehicles for bringing about a major change in the general outlook and scientific criteria. This is the essential point in my reconstruction of the story of how Wald's and von Neumann's two models came into existence.

## IV. CASSEL, WALD, VON NEUMANN AND HILBERT

For the purpose of this paper, I am going to review only the essentials of the models of Cassel, as analyzed by Wald, and of von Neumann. This will highlight the difference between the modern conception of an equilibrium state associated with a realization of an abstract structure and its earlier notion as an "intuitively observed" state of a given economy. This will also reveal the contrast existing between the corresponding notions of the "equilibrium model" and of "the model of an equilibrium" for a given economy. Borrowing the terminology from contemporary mathematical philosophies, I will associate the latter usage with an intuitionistic view that can be found in Walras and Cassel, for instance; the former will be called formalistic and attributed to the modern version of GE[9]. Thus, the exercise in logical reconstruction that I am going to offer, will serve the purpose of illustrating three features of the debate on the



foundations of GE that carry an unmistakable Formalistic earmark. These are: the distinction of the axiomatic model from (the set of) its interpretations; the identification of scientific progress with the discovery of new interpretations of a given set of primitive terms; finally, the association of the system states with realizations of virtual structures, whose "existence" can be ascertained with the method of indirect or nonconstructive proofs . In all these aspects, von Neumann was on the same track as the other Viennese but, as an enthusiastic supporter of Hilbert, he was also well ahead of them.

As is well known, in Cassel we really find two models. In both of them, a finite number of "factors" deliver services to be employed in goods production, where "goods" and "production processes" are also in finite numbers $m$ , $n$ , moreover equal to each other. In model A (the one discussed by Schlesinger, Heinrich von Stackelberg, F. L.B. Zeuthen, and eventually "solved" by Wald), the primal problem describes a process of factors allocation among production processes with specialized outputs. There is a parallel, technically dual, process of price formation for factors services and for goods currently produced. Hence, three sets of <u>arithmetic</u> relations are obtained (Cassel 1932, Chap.4): two distinct sets for the market equilibrium between demand for and supply of factor services and for goods; there is also a set of price-cost relations, one for each producible good and available process. <u>In Cassel</u>, each set of mathematical relations is <u>assumed</u> to be satisfied with strict equalities, in other words market equilibrium is assumed to prevail everywhere. This strong hypothesis is precisely the starting point of the work of critical revision that eventually condensed successfully in the Viennese circle[11].

As production requires (only) factor services, if factors were given in fixed amounts, the only sustainable growth rate would be equal to zero. Were factors supplies elastic over a sufficient amount of time, model A would represent a short run state or a temporary equilibrium, as later discussed by Lindhal and Hicks. Such an assumption on the supply acts as a necessary condition in the definition of Cassel's Evenly Progressive Economy, described in his model B. In the long run the growth potential of an economy, when left to itself and not disturbed from its path, will be regulated by the growth rate of the factor growing at the slowest pace. To realize a continuous dynamic equilibrium with equality between demand and supply for all factors, the system must start in equilibrium (as in the Walrasian "equilibrium ab ovo") and all "factors" must have a common growth rate. Factors whose supplies cannot be altered ("natural factors") must be eliminated (otherwise the overall rate would be zero); only augmentable factors (i.e. "produced goods") can be allowed for endless growth to take place. Hence: goods will be produced by means of goods alone.

I have emphasized those of Cassel's assumptions defining model B, which are also in von Neumann's growth model. The only remaining, though crucial, difference between the Cassel's Evenly Progressive Economy and von Neumann's growth model is the property of being optimal of the latter's dynamic equilibrium[12].



Nevertheless, what clearly emerges from the previous presentation is that von Neumann's growth model conceptually belongs to the class of general equilibrium models that were critically reexamined in the Vienna Mathematical Colloquium. The central analytical issue common to all those models is the proof of their logical consistency.

How did von Neumann come to the issue of the existence of the equilibrium state as an optimal path? It is fairly well known that he discovered how to treat it by reinterpreting the game theoretical framework developed a few years before: he adapted the interpretation of its symbols and produced an extension of the mathematical theorem deployed there to prove the central result.

In two-person, zero-sum game discussed in 1927 at Hilbert's mathematical seminar in Göttingen and soon published in 1928 in Mathematische Annalen, vectors of variables stand for probability distributions by the two players and the payoff matrix of the pure game is a general real-valued matrix whose entries have no sign restriction. This is the mathematical reason why the analysis of the properties of the associated game could not be performed with reference to theorems developed earlier (by the two mathematicians, Frobenius and Perron) as these refer to the class of nonnegative matrices. Moreover, economic reasoning suggested the use of inequalities instead of the more traditional equations, so that the problem could not be treated by the method of transformation to principal coordinates developed by Hilbert. Von Neumann resorted to the inequalities setting and successfully proved what came to be known as the minimax theorem, establishing the existence of an equilibrium for that class of games. The novel formulation of the mathematical problem required the abandonment of the algebraic (even simply arithmetic) treatment and a plunge into combinatorial and topological methods, of which he was then a master. In the growth model of 1937, the matrix of payoff coefficients could be reinterpreted as describing the net output matrix of a set of available processes of production. It only required the introduction of Walras's "coefficients de fabrication" with the assumption of a constant returns to scale technology, made popular by Cassel. The new model of production and exchange had certain general features that only more recently attracted great theoretical attention. However, the presence of joint products and of fixed capital are natural results of the fact, just mentioned, that matrix entries are unrestricted real numbers.

The "historical accident" of von Neumann's discovery of two distinct interpretations for the same set of mathematical entities and symbols illustrates very well how a new, equally acceptable theory can be generated according to Hilbert (and later Debreu).

## V. MODELS AND THEIR REALIZATIONS

In Cassel model A (the one taken up and discussed by Wald), the relationship between the primal and dual problems is in a



sense mathematically "imperfect": i) all currently produced goods are assumed to fetch positive prices, which should really be proved; on the other hand, ii) all producible goods are actually produced since equality between cost of production and sale price is ensured in each process. By artificially placing these two issues beyond the scope of formal proofs, only half leap necessary to go from the "classical" GE theory of the founders to its modern version was made by Wald.

The full step was made by von Neumann with the introduction of the rule of competitive pricing <u>and</u> the rule of choice of efficient techniques. Both the "set of commodities" <u>and</u> the "technique to be used" are determined <u>endogenously</u>. They lie in a subspace of the cross product space of "producible goods" and "known processes". Being the restriction to the subspace of commodities and used techniques, they are one of the <u>realizations</u> of the model. To solve von Neumann's mathematical system, a procedure is required for the simultaneous determination of the subspace of the "equilibrium supporting structures" <u>within</u> the set of original or pre-economic data. If there are many candidates for this role, the procedure must also incorporate a selection criterion. In classical mathematics, the criterion traditionally employed is based on optimization: the optimality property typical of von Neumann equilibrium solutions is absent from Cassel because he considers a unique realization (which, therefore, is trivially optimal). In von Neumann, as each realization may describe an economy, the one-to-one correspondence between an economy and its model is lost. Thus, with the notion of several alternative model realizations first introduced by von Neumann, the canonical GE model was born complete with all its essential formal rules and equilibrium properties.

In von Neumann and Wald, inequalities and complementarity slackness conditions[13] replace Cassel's (and Walras's) equations because the <u>description</u> of an equilibrium is replaced by the description of its "logical generation". For this new set of mathematical relations, "counting", as the method to ascertain the equality of the number of independent relations and of endogenous variables treated as unknowns, is neither sufficient nor necessary. Von Neumann's success in generalizing Brouwer's fixed point theorem to prove the existence of an economically meaningful solution may be something of a fortunate historical accident. However, the choice of a topological treatment is not a purely technical artifice. It accompanies the introduction of a new sharp distinction of the abstract (mathematical/logical) system made up of pre-economic data from the (possibly empty) set of its economically relevant subsystems. Due to this distinction, it becomes necessary to prove that the only relevant set is indeed nonempty, what we know as an existence proof. The fact that the model is a formal system conceived as a "closed logical universe" is mirrored by the reliance upon formal but indirect (i.e. nonconstructive) proofs which are now typical of modern GE: how the desired result (i.e. equilibrium values of the unknowns) can be actually obtained, is not shown, only that it is not unreasonable to conceive of their existence as a mathematical



object[14]. As Mathematical Intuitionists maintain in opposition to Formalists, such proofs only make sense, if at all, in a crystallized artificial or nonhuman universe, to their eyes a kind of reliable but purely logical toy[15].

## VI. FOUNDATIONS OF ECONOMICS, VIENNESE STYLE

This appraisal of GE supports the view that the theme of general economic interdependence was used as a testing ground for a largely predetermined thesis. The debate on Cassel is an essential phase in the process of "hardening of the core" of a new Research Programme; I think however that calling this Programme "neoWalrasian" (Weintraub 1979, 1985) may cause some misunderstanding unless properly qualified.

In my view, the crucial point is as follows. The case brought against Cassel was practically <u>engineered</u> to demonstrate the capabilities of a new approach based on strict and interpretation-free axiomatization. Cassel (and Walras) were scapegoats, singled out for their reliance on empirical intuition. Why did all this take place in and around Vienna? Now we have a clue to answer this question.

In Vienna more than in Germany, the origins of the debate lie in the encounter of a mature Austrian economic and social thought and the philosophy of Mathematical Formalism prevailing in German-speaking countries. This encounter gave rise to an intellectual milieu with a strong interdisciplinary orientation and vast intellectual curiosity. Its component of academic and professional economists had traditionally been suspicious of the use of calculus (and hence of Walrasian mathematical economics). With various naive technical arguments it had voiced the view that mathematics was largely irrelevant in economics due to the general non-differentiability of the mathematical functions that would describe economic behavior[16]. In a confused way perhaps, they stressed the qualitative nature of economic reasoning, thus sharpening their departure from the marginalistic identification of exact economic analysis with the possibility of quantification. Their mathematical training had traditionally been poor (as the narrative in K. Menger (1973) confirms). However, in the thirties their interest in mathematics was aroused by their involvement in the debate that led to Logical Positivism. They came to "discover" the necessity of rigorous, formal arguments from the study of Aristotelian and contemporary logics, as a result of their interest in the philosophy of science.

Beginning with Carl Menger, the Austrians had therefore advocated the marriage of economics and logic, rather than economics and calculus. Eventually, the youngest of the Austrian generations was on the frontier. The early developments of twentieth century mathematics, revolving around the issue of the Foundations, privileged the analysis of the general inferential laws that applied to universally defined structures built on sets of primitive entities and elementary relations[17]. These developments provided assurance that there was not only room for, but also dignity and necessity for, the use of fairly new



analytical techniques (set theory, combinatorics, topology and the like) that belonged to the intersection of mathematics and logics. To deal with the complexity implied by the atomistic structure of human societies, these discrete techniques looked more promising than those based on the use of well behaved functions appropriate to the "simplistic" Walrasian world[18]. The use of these new formal tools had no less precise significance than any traditional variational technique because there was a broader meaning to "calculus" than was understood by marginalism, a meaning in which the numerical aspect was but one of the implications (and not necessarily the most important).

This naturally led to an inquiry into the fundamental questions of "truth" and how to assess it. So, the Viennese milieu felt inclined to import ideas and to derive intellectual stimulus and inspiration from the burgeoning fields of contemporary mathematics and got more and more involved in similar foundational issues. This interest was enhanced by strong (often personal) bonds with the members of the Wiener Kreis, and economists often participated in both clubs[19].

The scientists involved in the debate on GE were taking a big step away from the intuitionistic understanding of the sources of theoretical insight typical of classical marginalism <u>and</u> of the British classical school. The importance placed upon logical deduction and apriorism as sources of economic knowledge by authors like Carl Menger and Eugen Böhm-Bawerk, made their 1930s descendants willing to accept a deductivistic approach to theoretical modeling that was combined with a strong relativistic stand. These came from the current state of the mathematical philosophy.

In fact, the painful recognition of the "crisis in intuition" as the foundation of scientific and mathematical truth, following the "discovery" of non Euclidean geometry, was an essential driving force in the mathematical developments of the century. Hilbert's reaction to this crisis (and other mathematical events at the turn of this century) indicated one way out: it was better to give up all "Euclidean" hope of assigning intuitive and empirical contents to models as scientific devices. Models and deductive structures had become coextensive. Then, for lack of anything better the "external" foundations, now missing, had to be replaced by the requirement of their forming a <u>complete</u> set of <u>independent</u> and <u>consistent</u> axioms, <u>and</u> by metatheoretical principles to produce correct answers to theoretical questions generated from within the theory.

Hilbert took advantage of the crisis in the Foundations of mathematics and pushed it to its extreme implications. The Viennese (and particularly, von Neumann) caused, in the Austrian logics of economics, the abandonment of the Euclidean axiomatic program which HIlbert had carried out in mathematics[20]. Set against the cultural and sociological background of Vienna and Mittle-Europe of the 30s, the debate on Cassel, together with certain papers by K. Menger and others and the largely independent but intellectually related work of von Neumann in game and growth theories, can be appraised as an essential phase



in the debate on the foundations of economics.

Very little of Walras was left in the neo-Walrasian program, not even the dynamic flavor of his (and Cassel's) "proofs" of the existence of an equilibrium[21].

## VII. CONJECTURES AND "PROOFS"

In the version analysed by the Viennese, the reduced form of Cassel's model A is made up of two logically independent blocks of equations, one deriving from the "laws" of production and the other from the "laws" of consumer behavior. Duality, on the other hand, appears in structural and irreducible form in von Neumann's model. For models so constructed, one cannot escape the task of explicitly demonstrating consistency: that blocks can "stick together" must be shown, it cannot be assumed.

As suggested elsewhere (Punzo 1989), the Viennese found "local counterexamples" to Cassel's model A. Existence became <u>the issue</u> of the possibility of inconsistency in the way we know it, because they exhibited instances in which two partial systems of mathematical relations were inconsistent did not yield acceptable economic values for the variables. In treating the existence issue, the mathematical philosophy they entertained, and the cultural and scientific background of some of them, allowed them to skip entirely the dynamic considerations that are so crucial in Walras. The import of the Hilbertian idea of a purely logical, hence "static", procedure for the statement of a dynamic property is the real "Austrian revenge" over Walrasian general equilibrium theory.

Cassel's argument for the existence of an equilibrium is heuristic but outlines an algorithm that mirrors a market equilibrating mechanism. Moreover, it provides one of those examples, not new in the history of scientific thought, of a conjecture or "informal proof" which can be far more telling than the statement it is unable to prove. It unveils a conception together with its implications.

In Cassel, the subspace of coordinates denominated after commodities, scarce factors and used processes is known at the outset by assumption. Consequently, Cassel's two models can only be understood as *descriptions* of certain states interpreted as specific kinds of equilibria. This is an assumption which is grounded in the "belief" that the set of solutions is nonempty for a well specified system of equations. It does not, however, exclude the possibility that some of the unknowns take on unacceptable values. Negative values (for, say, factors rentals) would signal, for instance, that some of the factors were not scarce, a piece of relevant information about the properties of the assumed set of data. In such a case, it would be evident that the set is misspecified: in modern terminology, the process of econometric identification must have made some error.

Thus Cassel's presumption that allowed him to proceed directly to the "computation" of equilibrium is consistent, first of all, with the assumption, which is "reasonable" within his theory, that his formalization captures only the qualitative elements actively participating in the determination of the



system state. If the model effectively describes an "empirically given" economy, correctly identified, Cassel's conjecture that the mathematical description of an equilibrium is a meaningful set of equations, must be true by definition. No existence issue may arise as such; only how to compute it[22].

VIII. STATE-SPACE AND OTHER MODELS

The denomination of "state-space models", borrowed from the theory of dynamical systems, seems appropriate to all models sharing the characteristics of not incorporating a clear borderline between the pre-theoretical world of data and the theoretical world of observable states. These models do not depend on formal manipulations either to derive their "theoretical meaningfulness" or to demonstrate their empirical explanatory power. They are "intuitive" if they successfully "capture" economic reality. All equilibrium models constructed before the Formalistic Revolution in the 30s fall into this class. From this viewpoint, I do not see any appreciable difference between the classical and marginalistic schools. To the contrary, the history of economic thought shows remarkable continuity up to the thirties. The GE analysis of Walras, Pareto and Cassel was not the social accounting of the British but it was still closer to the British view than to modern GE.

In the Formalistic approach, a general equilibrium model can be obtained only by making use of the distinction between primitive and economically relevant entities. A theory is "general" only when the latter are all determined endogenously.The fixed economic universe of Cassel and of the classical tradition obviously cannot account for the generation of an equilibrium because it refers to an empirically unique and observed economy. Primitive terms need not be interpreted because they appear with their own historically given interpretation.

In the end, however, all classical GE hid a methodological inconsistency which sometimes comes to the surface unexpectedly. This is due to the presence of two potentially rival principles in a state of unstable equilibrium: the reductionism implicit in the (claimed) adherence to the microeconomic point of view, where an economy is made up of selfish individuals, and (a version of) biologistic wholism. Thus, the description of an entirely endogenous process of price formation, as the social manifestation of independent decision units, is often superseded by a systemic notion that depicts a resource allocation process globally regulated by the "scarcity principle". A noncompromisingly functionalistic approach could only be the product of an intellectual milieu profoundly influenced by Ernest Mach, as the Viennese in fact were.

In breaking with its earlier formulation tainted with teleologism, modern functionalism met a natural ally in Formalistic Philosophy. Formalistic insistence on generality and abstraction and its coherent relativism supplied the basis for a conceptual compromise that sacrificed "substance" and content in favour of well established and obeyed formal rules of axiomatic model theory. From the Formalist viewpoint, there could be no



conflict: the reductionism implicit in the micro viewpoint and the working of certain macro laws could be reconciled as different manifestations of the general law of functional interdependence, each of them at its appropriate level. The resolution of this problem was the reward promised by the acceptance to this programme.

The methodological individualism of the Austrians is the most developed and palatable translation of such functionalism into economists' jargon, fit to mediate the spurious alliance just described. It made it popular in economics. Nonetheless, I believe that methodological individualism is not an essential ingredient of modern GE. It belongs to its protective belt, rather than its core.

The codification of the laws regulating functional relations was the main task historically performed by the Viennese. Models as "formal abstractions" were in need of validation. This was sought in proofs of consistency, because only consistent models would be able to "explain" their endogenous variables, and in a dictionary to translate models and theorems into empirical phenomena. It is an open dictionary, to which pages are added whenever a new interpretation is found, but nothing prevents it from being empty

A model that has successfully passed the consistency test can be transferred to a state space framework. Logically feasible states become observable states. Equilibria are, however, the only kind of states in which the building blocks (whether rational individuals, institutions, or simply sets of formal, objective mathematical relations) find coordination. I call models constructed on these basic ingredients, ex-ante models and the related approach, the ex-ante approach.

In this view of the theoretical developments springing from the Viennese debate, the real and still unresolved contrast within economic theory is between the ex-ante and state-space approaches. It is a piece of history that should be read in this light.

XIX century models claim to be based upon descriptions of "actual" states and economies. The fine borderline between algebra and arithmetic, that appears in their formalizations, is the reason for the impression one often gets, that they are concerned with some kind of "social accounts" rather than with "equilibrium". There is no rigorous formalization of the causal relations that generate system states. When "laws" appear, they pertain to a different level of the theory, the level of explanation, where they resort to "non rigorous", purely verbal statements.

Cassel's daring theoretical position, often thought of as a simplification, had the involuntary merit of revealing the failure of the XIX century attempts to integrate description and explanation. In economics this integration was achieved by the Formalists in their own way. To them these two levels are not worth distinguishing because they are hierarchically related as two levels of the same theoretical construction.

Microeconomic foundations for the explanation did not really exist before the Formalistic Revolution. I am not sure whether



the Viennese debate and the Revolution it triggered in economics, really succeeded in "inventing" them.

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Footnotes:

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[1]The history of the debate is well known. See the mathematical exposition in Robert Dorfman, P.A. Samuelson and Robert Solow (1958); various original papers re-printed in translation (with comments and biographical notes) in W.J. Baumol and S.M. Goldfeld (1968); and in particular the critical reconstruction proposed by E.R. Weintraub (1979), (1985). Note that Karl Menger of the Vienna Mathematical Colloquium was the son of Carl Menger, one of the founders of the Austrian School of Economics.

[2]For the general notion of a metatheory, as understood by the Viennese, see for instance K. Menger (1979), Chap. 1.

[3]David Hilbert and his mathematical seminar in Göttingen played an important role in the development of mathematics in this century, in particular in the debate on the Foundations that opposed the three schools of Formalism, Intuitionism and Logicism. For an evalutation of his mathematical work, see Herman Weyl (1944); for a biography, see Constance Reid (1972).

[4]"The subject matter of metamathematics is an abstraction of mathematics in which mathematical theories are replaced by formal systems, proofs by certain sequences of well-formed formulae, definitions by 'abbreviatory devices' which are 'theoretically dispensable' but 'typographically convenient'.(...) I shall refer to the school of mathematical philosophy which tends to identify mathematics with its formal axiomatic abstraction (and the philosophy of mathematics with metamathematics) as the 'formalistic' school. (...) According to formalists, mathematics is identical with formalized mathematics." (Lakatos (1976), pp.1-3). See also K. Menger (1979), Chap. 1.

[5]For the purposes of my argument, it is relevant that Bourbaki (the collective pen name of a group of French mathematicians) accepts the concept of two levels of theoretical activity and agrees with Hilbert in asserting that a theory must be completed by a metatheory. However, in Hilbert's own program, the metatheory has to develop a proof theory that could provide generally accepted foundations for all the deductive procedures employed in the various mathematical theories. As a result of Gödel's critique, which had exposed the failure of Hilbert's efforts (see footnote no.6), Bourbaki reduces the role of Metatheory to that of explaining the use of abbreviated symbols and the rules for combining them in deductive chains. (See Bourbaki (1950).) For an explicit ackhowledgement of Bourbaki's influence, see Debreu's Nobel Memorial Prize lecture, (Debreu 1984).



[6]See von Neumann (1932); Michael Jammer (1974).

[7]Among other things, Gödel proved that Hilbert's project of proving the consistency of the whole of mathematics within metamathematics could not be accomplished, thus undermining the very foundations of the original (and strictest) version of Mathematical Formalism. (A most interesting description of Gödel's devastating criticism can be found in Chap.12 of Morris Kline (1980)). K. Menger's position, that was intermediate between Intuitionism and Formalism, is described in Menger (1979). At the time von Neumann was an enthusiastic supporter of Hilbert's program, but later changed his mind under the influence of Gödel and Turing.

[8]As is well known, the application and extension of his axiomatic method were in Hilbert's agenda of unresolved problems presented at the International Mathematical Conference held in Paris in 1900. For this important episode and a full list of the problems, see C. Reid (1972).

[9]For the debate between Formalism and E.J. Brouwer's Intuitionism, see for instance Kline (1980), Chaps 10-12. It is also interesting to look at K. Menger'own version in K. Menger (1979), Chaps. 2 and 5.

[10]See, for instance, Debreu (1959), (1984). For a definition of constructive proof of an existential statement, let me cite Friedrich Waismann: a constructive proof "(..) outlines a procedure, a systematic method, or an *algorithm*, which, if we follow it, will lead us to the root [of an algebraic equation], or shows us at least a path for approximating it with as much accuracy as we please.(...) Existence in this sense means that the object, be it a number, or a function, or an equation, the existence of which is asserted, can be found, or approached, or constructed, in a systematic way, according to a rule of procedure." As for nonconstructive or *indirect proofs*, "We sometimes say that, corresponding to such a number, there exists a thing of a certain specifiable sort, without knowing in the least what we are to do in order to find it or compute it approximately. How, then, do we know that such a number exists? here existence rests on an essentially indirect proof: supposing that such a number does not exist, it can be shown that a contradiction arises." (Waismann (1982).)

[11]This is discussed in Punzo (1989).

[12]It is worth noting that the structure of a neoclassical model of production and exchange lies in a (Euclidean coordinate) space which is the cross product of the spaces to which the structures of the two models A and B belong. It requires demand functions

for goods, and factors as well as produced goods as inputs to good output.

[13]Complementarity slackness conditions state that: i) processes that do not earn the prevaling rate of profit must be discarded; and ii) goods in excess supply will be free goods.

[14]Even numerical unknonws could be treated in this abstract or nonnumerical way. Von Neumann's existence proof is a very illuminating case. Take the rate of expansion $\alpha$ and the rate of interest $\beta$ , which (together with the supporting vectors of activity levels and of competitive prices) enter a von Neumann solution. To make economic sense both variables must take on real values. The traditional, constructive, way to go about proving their "existence" would be to construct a method to compute them. If one could think of them as the unknowns in some appropriately defined polynomial equations, one would have either the elementary method of the discriminant, which is exact but limited to low-order equations, or a variety of approximation methods. Alternatively, one could invoke the Fundamental Theorem of Algebra that states that all algebraic equations have solutions in the complex plane. This is an existential but indirect result. Obviously, with inequalities neither approach is available, but von Neumann followed a "inspiration" closer to the latter. He defined $\alpha$ and $\beta$ as <u>members</u> of two sets of appropriately defined, nonnegative scalars and showed that: these two sets cannot be empty and must have at least one common member. Thus, the three results are obtained: i) there exists an equilibrium pair $(\alpha , \beta)$ such that $\alpha = \beta$ ; ii) the pair is optimal (in the minimax sense); iii) the two variables are both nonnegative. Unknowns are numbers but are not treated as such. The greater generality and elegance of this kind of proof is paid for by the fact that its statement is of a pure qualitative nature.

[15]Formalists do not deny the value nor the desiderability of constructive proofs. In fact, Hilbert assigned to Metatheory the task of finding the fundamental proof of the consistency of the whole of mathematics <u>by generally accepted</u> (finitistic or <u>constructive</u>) <u>methods</u>. This clarifies why there is a revival of interest in numerical methods in GE. For the Intuitionist indirect proofs are no proof at all, and therefore cannot be accepted even in the absence of a constructive proof. They can be accepted by Formalism because of its conception of models as "closed" and of theoretical activity as "discovery of existing truths" rather than "construction" or "human invention". The hot debate on Proof theory shows that not even the apparently technical issue of how to prove a mathematical statement is neutral.

[16]See K. Menger (1973).

[17]The famous anecdote on the "Geometry of tables and chairs.." attributed to Hilbert is a good example of his search for an "abstract viewpoint" even in Eulidean geometry. (Reported in Constance Reid (1972)).

[18]On the Walrasian conception of the economic world and economic behavior, see the critical remarks in the opening paragraphs of von Neumann and Morgenstern (1944).



[19]See the excellent paper by Earlene Craver, Craver (1986).

[20]I accept Lakatos's reading of the evolution from the Euclidean to the Formalistic version of deductivism. In the former, "truth" is always injected into the axiomatic system by choosing assumptions and primitive terms that have intuitive meaning. In the latter, they might receive a meaning at the end, via interpretation. (Lakatos (1978a), (1978b).)

[21]Up to the fifties, Walras's tatonnement was interpreted as a dynamic argument to establish the existence of an equilibrium. See, for instance, R.M. Goodwin (1951).

[22]For a discussion of this important point, see Punzo (1989).

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