Mathematics as the role model for neoclassical economics (Blanqui Lecture)

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BLANQUI LECTURE
In the sign of the axiomatic method: mathematics as the role model for neoclassical economics

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§1. Beware the underdog

<<Mathematical analysis may deal with economic issues, like with any other scientific issue, in two different ways. The first is the method that I would like to call *formal*, by using which the analyst does not care at all of the intrinsic truthfulness or falseness of the theorem that he is willing to demonstrate, nor does he bother himself with investigating whether the problem's data are based upon real or hypothetical foundations, as his only goal is that of replacing the involved and inexact forms of ordinary language with the simple and exact statements of mathematical language. On the contrary, the other method, which we can call *objective*, never satisfies itself with dressing up with a rigorous formula any concept whose reality and nature have not been ascertained beforehand, but rather it makes analysis subservient to the search of truth and its demonstration. The difference between these two systems is the same as that between nominalism and realism, between a vane science of words and a true and positive philosophy of things.>>

<<Deductive reasoning about social phenomena invited the use of mathematics from the first. Among the social sciences, economics was in a privileged position to respond to that invitation, for its two central concepts, commodity and prices, are quantified in a unique manner, as soon as units of measurement are chosen. ... As a formal model of an economy acquires a mathematical life of its own, it becomes the object of an inexorable process in which rigor, generality and simplicity are relentlessly pursued. ... An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object. ... Next, assumptions on the objects representing the primitive concepts are specified, and consequences are mathematically derived from them. The economic interpretation of the theorems so obtained is the last step of the analysis. According to this schema, an axiomatized theory has a mathematical form that is completely separated from its economic content. If one removes the economic interpretation of the

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primitive concepts, of the assumptions and of the conclusions of the model, its bare mathematical structure must still stand.>>

The first citation is taken from the entry "Mathematics applied to Political Economy" in the *Dizionario Universale di Economia Politica e di Commercio* by the 19th-century Italian economist Gerolamo Boccardo (1877, 218, my translation). The second is taken from the entry "Mathematical economics" by a probably more famous economist, Gerard Debreu (1987, 399 and 401), in the *New Palgrave Dictionary*.

Boccardo was a committed positivist, who claimed that the scientific method could achieve the greatest perfection only through the application of the mathematical method: while, in fact, empirical observations were the indispensable starting point of any knowledge endeavor – including political economy – only mathematics could grant such observations the exact and systematic form that was required to turn any discipline – including again political economy – into a "true" science, the paradigmatic case being of course that of physics (Boccardo 1877, 217). But if the primary role of mathematics was to grant exactness and order to observations, it followed that no discipline could achieve a truly scientific status by employing the deductive method only. Hence, Boccardo thought that those economists who, like William Whewell, championed the formal approach were actually reducing political economy to a mere mathematical game, devoid of any empirical import and similar in spirit to the *theory* of chess (ibid., p.219).[1]

It is a big leap from Boccardo to Debreu, under every respect. Indeed, one of the main points in the latter's entry in the *New Palgrave* – as well as in other papers (see e.g. Debreu 1984; 1991) – is that physics cannot, and thus should not, represent the role model for economics precisely because it is a discipline which has never completely surrendered to mathematics and has always retained its experimental and observational foundations. Given that economics is not amenable to experimentation, it is forced to find its role model in the only scientific discipline which is non-experimental, namely, mathematics (Debreu 1991, 2). While I leave to further research to check the validity of the first part of the latter sentence in the light of the recent boom of experimental economics, my focus here is on the implications that Debreu draws from the second part. Adopting mathematics as a paradigm entails, in fact, that mathematical economics is not anymore a specialist branch of the whole discipline, but the only

[1] The theory, note well, and not the real play, because only in the former case the typical problem is how to win starting from a pre-assigned position of the pieces on the board, regardless of the unrealism of the position itself.

possible form of any scientifically robust theorization over economic phenomena. Moreover, it also entails that economic analysis should not look for its premises in the outside world because a mathematical model's life is totally independent of empirical reality, while its scientific validity can only be tested by the logical consistency of its propositions (Debreu 1987, 400-1).

A simple explanation of the different attitude towards mathematics of Boccardo and Debreu would follow from embracing an incrementalist view of the history of economics, so that one could proclaim a few platitudes such as that the poor Boccardo did not know what we, the intellectual children of the great Debreu, now know, that this is what progress in economics is all about, that mathematics is just a useful tool-box but no economist is ever really driven only by formal quibbles, and so on and so forth. The problem with this kind of answer is that it neglects the historical fact that the very same contrast between the alternative ways – the objective and the formal – of using mathematics in economics has occurred time and again throughout the 20th century. A prominent case is the controversy in the 1954 *Review of Economics and Statistics* between another, Boccardo-style, underdog, a Mr. Nobody statistical economist with a specialization in industrial cost analysis named David Novick, on the one side, and a legion of future Hall-of-Famers in the dismal science (Paul Samuelson, Lawrence Klein, James Duesenberry, John Chipman, Jan Tinbergen, David Champernowne, Robert Solow, Robert Dorfman and Tjalling Koopmans) on the other. The topic of the controversy – which has been brought again under the spotlight by Phil Mirowski in his *Machine Dreams* (Mirowski 2002, 396-406) – was no less than the most proper way to apply mathematics in economics.

The gist of Novick's bold two-page tirade against the new, postwar neoclassical orthodoxy was his plea to never lose sight of the difference between mathematics as a language form and mathematics as a quantitative method. The latter was how mathematics had always been used in applied natural and physical sciences and thus the standard to which economics should also conform (Novick 1954, 358). On the contrary, he lamented that economists and the other social scientists had taken the bad habit of using mathematics

<<…as it has been used in theoretic physics or chemistry and not as the mathematical results of theory proved by statistics in physics or chemistry are applied in everyday engineering or mechanics. The current use of mathematical language in social science is largely a form of intellectual shorthand and in no way demonstrates that the methods heretofore so successful in the physical sciences have suddenly become adaptable to the social sciences.>> (ibid., p.357).

As Deirdre McCloskey recently put it, that is the kind of qualitative mathematics – the mathematics of "why" and "whether" – that typically originates from Mathematical Departments, not the mathematics of "how much" that is used in applied physics or engineering (McCloskey 2005, 3). Thus, Novick concluded that modern economic theory might even be

<<...a most interesting one, susceptible to "toy" proofs, but [it was] not at all adaptable to the facts of the real world.>> (Novick 1954, 357),

that is to say, it was <<...of no use for science.>> (McCloskey 2005, 3).

It is not hard to imagine the reaction to Novick's attack by the defenders of the new mainstream. Indeed, by reading their replies, one may get an illuminating perspective of the scientific background, research propensities and personal attitudes of the nine Hall-of-Famers listed above: from Dorfman's patient catechism on what mathematics stands for in the social sciences to Klein and Tinbergen's passionate defense of econometric techniques, from the elegant and partially receptive (though still overall critic) words of Champernowne and Chipman to the rude and dismissive tone of – guess whom? – Samuelson and Solow. Yet, as Mirowski (2002, 404) correctly notes, the single most important reply was Koopmans's – as he was the only one to fully realize that Novick's complaints were directed not against the use of mathematics itself but, first and foremost, against the new kind of mathematics that had just entered economics, namely, the mathematics of convexity, matrix algebra, set theory and the axiomatic method (Koopmans 1954, 377). Hence, in the three pages of Koopmans's answer we may find one of the earliest defenses of the so-called formalist approach to mathematical economics, pre-dating even his classic 1957 *Three Essays on the State of Economic Science*.

While I leave the details of Koopmans's argument, as well as the very down-to-earth motivations behind the whole controversy, to the readers of Mirowski's book, the simple lesson that I wish to draw from the episode is that the issue of the most proper kind of mathematics for the social sciences was very much open still in the fatal 1954, that is, the year of Arrow and Debreu's existence proof.[2] It follows that the standard reconstruction of the history of 20th-century neoclassical economics in terms of a steady increase in mathematization and of a smooth passage from the stage of informal – that is, *non-rigorous* – investigation to the stage of formal – that is, *rigorous* – analysis is largely unsatisfactory. This for at least two reasons: first, because the transition was neither smooth nor

[2] This of course is hardly a novelty: see e.g. the amusing reconstruction in Weintraub 2002, Ch.6, of how Arrow and Debreu's seminal paper came to be accepted for publication by *Econometrica*.

steady, and, second, because it is by no means obvious that formal be synonymous with rigorous. Indeed, *both* approaches to mathematics – the objective and the formal, to reiterate Boccardo's terms – are, and have always been, rigorous, each of course in its own way. Or, how else could any David Novick perform his professional, and highly sensitive, task of costing weapon systems on behalf of the US Department of Defense if not by rigorously applying the most advanced mathematical techniques of engineering and industrial management? Hence, I believe it is crucial for any narrative of how modern neoclassical economics came to be to realize that the change in its mathematical techniques that took place in the interwar and, especially, postwar period has to be explained in terms of a change in the notion of mathematical rigor.

Again, this point is not a new one, as it comes from the authoritative scholarship of Roy Weintraub who in his most recent works (see e.g. Weintraub 1998; 2002) has stressed time and again that there is a big difference between today's meaning of the word "rigorous", as synonymous with "formal", or even with "axiomatized", and how the same term was interpreted in the late 19th and early 20th century. For the great Italian mathematician, and Pareto's reviewer, Vito Volterra – whom Weintraub takes as exemplar of the old use of the word – to be rigorous in any scientific discipline, from physics to biology, from chemistry to economics, meant to model a phenomenon on the basis of a series of experimental results and/or direct observations. Hence, the opposite of rigorous was not, as today, informal, but rather *unconstrained*, so that a non-rigorous argument was one devoid of a proper foundation upon experimental and/or observational data (see Weintraub 2002, 42-51). Thus, it is hardly surprising that at Volterra's time the paradigmatic case of a rigorous discipline was mechanics, while today the role model for any discipline aiming at achieving full rigor is mathematics itself.

So, the questions arise: how could it happen that neoclassical economics came to embrace a new notion of rigor? Why and when did the economists' role model become the mathematician rather than the mechanical physicist? Remarkably, Volterra himself gave us a possible answer, as he famously expressed his skepticism about the economists' possibility to avail themselves of a stock of empirical material capable in both quantitative and qualitative terms to meet the standard of rigor of mechanics (see Volterra 1906, 298; Ingrao and Israel 1990, Ch.6). According to this interpretation, the triumph of formalism in modern neoclassical economics should be explained in terms of the discipline's increasing awareness of its lack of good experimental and observational data, and thus of its intrinsic inability to fully abide by the paradigm of mechanics. This eventually entailed the abandonment of the early marginalists' dream of getting to the top

place in the scientists' ranking, that is, alongside the "real" scientists like the physicists, and surely very much above all the other social scientists.

Among the keenest supporters of such a dream, one might list the usual suspects, such as W.S. Jevons, whose intellectual debt to the rising thermodynamics has been recently reconstructed by Michael White (White 2004), or Irving Fisher, whose famous hydrostatical device aimed at proving no less than the physical viability of a general economic equilibrium (see Schwalbe 1999; Brainard and Scarf 2000), or Vilfredo Pareto, whose previous training as an engineer left ample traces in his economics. But perhaps even the name of Alfred Marshall might be added, at least as long as one agrees with Weintraub's thesis about the deep influence on his later works of the objective kind of mathematics he had to learn in order to pass the Cambridge Tripos (Weintraub 2002, Ch.1).

The standard story would continue by stressing that, despite such authoritative endorsements, the dream of emulating mechanics was shattered by the intrinsic empirical limits of a discipline dealing with humans rather than atoms. Thus, neoclassical economists would seem to have had no choice but to embrace the alternative, purely formal notion of rigor and to look for their role model in the Mathematical Departments – a forced, almost reluctant choice. As a leading example, one could then mention the Cowles Commission's late 1940s decision to reformulate its research agenda away from the disappointing results of the macroeconometrics project: a crucial move that opened the door to the rise of general equilibrium analysis, game and decision theory, operations research, etc.

I believe that there is more than one grain of truth in such a story, but also that it is not entirely convincing. I can see at least three major gaps in it: first, it does not properly take into account the history of mathematics itself, that is, the history of how and why the meaning of the word "rigor" came to be modified; second, it neglects the possible existence of a specific motivation behind the economists' decision to pursue the mathematicians', rather than the physicists', route; third, it does not provide a plausible explanation for the failure of interwar and postwar economists to run proper economic experiments, given that the last two decades of the 20th century have amply proved that this is far from being a really impossible or fruitless task.

As I said before, I will not deal here with the third issue, so that the rest of my talk will focus on the first two. In particular, I will try to offer a taste of a (partially) alternative story which begins with the so-called formalist revolution in mathematics, then crosses the economists' urge to bring their discipline to the highest possible level of generality and conceptual integrity, and ends with the radical transformation in what I call, after Leo Corry's dichotomy (see below, §6), the image of economics. Such a narrative is developed more fully in Giocoli 2003,

so I offer you my advance apologies for the possible obscurity of some parts of my presentation.

§2. What would Boccardo have thought of him?

<<...I should like to sum up in a few sentences my general conception of the essence of the axiomatic method. I believe: anything at all that can be the object of scientific thought becomes dependent on the axiomatic methods, and thereby indirectly upon mathematics, as soon as it is ripe for the formation of a theory. [...] *In the sign of the axiomatic method, mathematics is summoned to a leading role in science.*>>.

This is taken from one of David Hilbert's most well-known essays, his 1918 "Axiomatisches Denken" (Hilbert 1996a [1918], 1115, emphasis added). The passage effectively summarizes the gist of Hilbert's formalist program, namely, the idea that mathematics should assert itself as the unifying cornerstone for all kinds of scientific endeavors.[3] The hectoring tone of the passage, which deliberately recalls Emperor Constantine's motto "*in hoc signo vinces*", may justify the charge of "imperialism" moved against the program. Yet, what is still seldom recognized is that such "imperialistic" ambitions were in Hilbert's view just the inevitable outcome of the role and the power of the axiomatic method in mathematics.

Consider for instance the following passage, still taken from the 1918 essay:

<<The procedure of the axiomatic method, as it is expressed here, amounts to a *deepening of the foundations* of the individual domains of knowledge - a deepening that is necessary for every edifice that one wishes to expand and to build higher while preserving its stability>> (ibid., 1109, original emphasis).

As convincingly argued by the historian of mathematics Leo Corry (see e.g. Corry 2000, 48-9), Hilbert viewed formal axiomatic systems *instrumentally*, that is, as a powerful tool for mathematical research to be employed whenever a field of knowledge had reached a point of sufficient ripeness. Thus, axiomatics was not an end in itself, but rather a tool to achieve a clearer understanding of any theory capable of being formulated in mathematical terms. This was the sense in which

[3] On the rise and the meaning of mathematical formalism, see Menger 1979 ?1933?; Israel 1977; Kline 1980, Chs.8-11; Eves 1990, Chs.6 and 9; Ewald 1996, Chs.24 and 28; and, above all, Corry 1996, Chs.3 and 7; 1997; 2000. I also owe a lot to the excellent literature on the relationship between formalism and neoclassical economics: see Ingrao & Israel 1990, Ch.7; Leonard 1997; 1998; Punzo 1989; 1991; 1999; and, above all, Weintraub 1985, Ch.6; 1998; 2002, Chs.3-4.

the axiomatic method would warrant the establishment of mathematics as the supreme standard and check for the advancement of all scientific knowledge.

While the literature of the period does contain statements which seem to validate the traditional view of the Hilbertian approach as the sheer identification of mathematics with the study of merely formal systems – think for example of the well-known assertion by John von Neumann, according to whom, with the adoption of the axiomatic method mathematics would become

> <<...an internally closed procedure which operates according to fixed rules [...] and which consists basically in constructing successively certain combinations of primitive symbols which are considered "correct" or "proved" [...] a *combinatorial game* played with primitive symbols.>> (von Neumann 1983 ?1931?, 61-2)

– it is crucial to recognize that Hilbert himself claimed that the axiomatic approach entailed neither the reduction of mathematics to an empty game nor a conceptual break with the classic analytical problems of empirical sciences. Indeed, it is even unsure that he ever really (or, at least, *seriously*), formulated his famous proposal <<...to replace in all geometric statements the words *point*, *line*, *plane*, by *table*, *chair* and *mug*.>> (cf. Ewald 1996, 1089). Rather, what he looked for was an improvement in the mathematician's understanding of empirical sciences, as he believed that the growth of any scientific discipline involved both an expansion in its scope and an ongoing clarification of the logical structure of its existing parts. The axiomatization was just a very important step in such a growth.

For a clear perspective of the strong – if seldom acknowledged – empirical underpinnings of Hilbert's conception of axiomatics, and of the central role he still warranted to intuition and experience, one may for instance look at his 1905 lectures on the axiomatic method (see Corry 1997, 123 ff.), where he tackled one of the main unsolved puzzles in his famous 1900 list, namely, the problem of whether it was possible to axiomatize the physical sciences in the same manner as he had already done for geometry. To demonstrate that this was indeed the case, the lectures offered an axiomatic treatment of several applied fields, mostly taken from physics, but also including social science topics, such as psychophysics and insurances. Crucially, Hilbert claimed throughout the text that the axioms could contribute not only to the conceptual clarification of these fields' existing theories, but also to make them more flexible with respect to new empirical data.

Thus, even the term "formalist", when applied to Hilbert, may be somehow misleading. Such a term does in fact betray the beliefs of a mathematician who

wrote that: <<…if scientific knowledge is to be possible, certain intuitive conceptions and insights are indispensable; logic alone does not suffice>> (quoted by Ewald 1996, p.1107) and who firmly opposed the view

<<…that only the concepts of analysis, or even those of arithmetic alone, are susceptible of a fully rigorous treatment. This opinion […] I consider entirely erroneous. Such a one-sided interpretation of the requirement of rigour would soon lead to the ignoring of all concepts arising from geometry, mechanics and physics, […] But what an important nerve, vital to mathematical science, would be cut by the extirpation of geometry and mathematical physics! On the contrary I think that wherever, from the side of the theory of knowledge or in geometry, or from the theories of natural or physical science, mathematical ideas come up, the problem arises for mathematical science to investigate the principles underlying these ideas and so establish them upon a simple and complete system of axioms, that the exactness of the new ideas and their applicability to deduction shall be in no respect inferior to those of the old arithmetical concepts.>> (Hilbert 1996 [1900], 1100).

With the benefit of hindsight, can we be so sure that our old pal Boccardo would have placed Hilbert in his "black list" of the supporters of a purely formal view of mathematics in the social sciences?

§3. Axiomatic bigamy

Why is it so important for historians of economics to clarify the real import of Hilbert's axiomatic approach? The answer is that this effort may cast new light upon two meaningful issues for the history of 20th-century neoclassical thought, namely, the rise of general equilibrium theory and the foundation of game and decision theory.

As I said before, there is in fact an alternative, much more traditional, presentation of the main tenets of the formalist school. The typical claims that are attributed to Hilbert and his fellows are:

- ***first*, that in the realm of mathematics the form is the essence, so that mathematics is a discipline concerned with formal symbolic systems, devoid of concrete content and whose ultimate basis lies in the primitives – pure signs, with no empirical meaning – and the properties attributed to them – i.e., the axioms;**
- ***second*, that the real counterpart of a mathematical model is simply irrelevant with respect to its logical structure, so that, while mathematics constitutes the universal language for all scientific endeavors, the connection**

with reality is postponed to a second stage of the analysis which is not necessarily up to the mathematician to perform;

- ***third*, that the single most important issue in the formalist program is the demonstration of the consistency of arithmetics as the indispensable step to achieve the grand goal of proving the consistency of the whole of mathematics;**
- ***fourth*, that this goal requires the development of so-called proof theory, or meta-mathematics, which is a sort of "handbook" of how to theorize correctly, i.e., a set of instructions for selecting the primitives and the axioms, combining them and deducing the propositions;**
- ***fifth*, and finally, that meta-mathematics is the core of the whole formalist program, since only by attaining the meta-theoretical level of any given mathematical theory – say, geometry or arithmetics – it can be demonstrated that the theory itself contains no contradictions.**

The standard presentation then goes on by recalling how in 1930 a young Viennese mathematician, called Kurt Gödel, showed that the self-consistency of (the whole of) logic or mathematics could *not* be proved by (a part of) logic or mathematics (Gödel 1986 ?1931?), and ends by stressing that this result effectively terminated the formalist program.

Roy Weintraub has been the first in our discipline to note that such a traditional reading of the Hilbertian school has somehow biased the assessment of the historical relationship between formalism and neoclassical economics (Weintraub 2002, Ch.3). For example, some historians – including Weintraub himself (see Weintraub 1985) – have emphasized the similarity between the notion of a metatheory and the modeling pattern of modern general equilibrium theory. Indeed, the idea of the metatheory as a "handbook" of rules that can be applied to investigate certain sets of abstract objects and to demonstrate that the objects, their assumed properties and the theorems derived through them constitute a mathematical system that satisfies the fundamental requirement of consistency – an idea that entails the abandonment of the classic one-to-one relation between every mathematical model and reality in favor of the view where a model is simply the attribution of an interpretation to the primitives of a purely formal mathematical system – this idea seems to find an almost perfect counterpart in the way general equilibrium analysis was interpreted in the 1930s, especially in Karl Menger's *Mathematical Colloquium*.

As a result of their strong commitment to Hilbert's formalism – so the story goes (see Punzo 1989; 1991) – the participants to the Colloquium transformed the Walrasian model into the metatheory of the whole economic analysis. This in turn had an overwhelming influence on the subsequent history of neoclassical

economics: the key requirement of any metatheory – namely, its being consistent – combined with the economists' view of equilibrium as a state of mutual compatibility of economic variables to grant a foundational status to the existence proof of a general economic equilibrium.

However, the way such a proof is usually carried out only shows the theoretical possibility of a certain mathematical property of a given set of economic relations. As a consequence, it is argued that rather than start from the empirical data about a given economic phenomenon and look for their analytical description, economic analysis in the general equilibrium tradition has reduced itself to a mere mathematical exercise showing that it is possible to take a given formal structure (the Walrasian metatheory) and prove that it is not unreasonable to claim that one of its offspring may have generated the phenomenon in question. In short, economic models have become totally non-descriptive – or, as Ingrao and Israel (1990, 182) put it, <<...abstract schemata of possible contents...>>.

The same argument has been applied to the birth of modern game and decision theory, in particular to von Neumann's role in it. Two common statements in the literature are, first, that von Neumann's overall contribution to economics should be read as just one further application of the meta-theoretical approach of his mentor Hilbert (see e.g. Punzo 1989), and, second, that there have been *two* von Neumanns, a pre- and a post-Gödel one, the latter being quite disillusioned with respect to the power of formalist methods. In support of the first statement both the bold simplifying assumptions of his 1937 general equilibrium model and the apparent unrealism of the axioms of expected utility theory are usually called forth. In support of the second, reference is made to von Neumann's own words in his well-known 1947 essay "The mathematician" – where he warned against the risk of pushing too far the search for absolute purity, lest mathematics would become a mere aesthetic exercise (von Neumann 1961 [1947], 9) – and to an alleged difference between the philosophical underpinnings of his 1928 (i.e., pre-Gödel) and 1944 game theory.[4]

I believe that a more correct rendition of Hilbert's axiomatic program allows us to better appraise the validity of the mentioned literature. Take for instance the idea of the Walrasian model as the metatheory of modern neoclassical economics. The problem with this interpretation is that it does not seem to apply to the very case of the author who has most emphatically defended the view that even in economics an axiomatic theory must be totally emptied of its empirical references, <<...logically entirely disconnected from its interpretations...>> (1959, viii) and perfectly neutral with respect to its possible applications. This author is

[4] This difference is the cornerstone of Mirowski's narrative in Chapter 3 of *Machine Dreams*: for a critic see Giocoli 2003b.

of course Gerard Debreu and the simple reason he does not fit in the metatheoretical story is that his intellectual reference for mathematical formalism was not David Hilbert, but Nicholas Bourbaki.

As everybody knows, the latter was the pseudonym adopted in the mid-1930s by a group of young French mathematicians who aimed at no less than rebuilding the whole of mathematics. Their peculiar approach has exercised an enormous influence on the 20^{th}-century history of the discipline: for example, it is impossible to neglect the role played by Bourbaki as a group and by its individual members on the development of modern algebra and topology. Despite proclaiming themselves the legitimate heirs to Hilbert's formalism, the group's central idea was that mathematics is an autonomous subject with no need of any input from the real world. Hence, it was Bourbaki who actually brought to the extreme the separation of mathematics from its applications and sources of inspiration. Axioms and reality had no necessary link: what reality could do was, at best, to suggest some of the axioms, but afterwards mathematics had to cut all ties with it. Only at the very last stage – a stage which did not belong to the mathematician, but to the applied scientist – the formal results of mathematical analysis could be associated to the laws of nature. In short, it was Bourbaki who talked of mathematics as a *storehouse of abstract forms* (Bourbaki 1996 [1948; 1950], 1276). However, such an extreme attitude towards the role of axiomatics was compensated by a relativistic view as to what mathematics could (and should) achieve. While Hilbert's approach did pursue the axiomatization of the one *true* mathematics, Bourbaki believed that mathematics was always relative and avoided to enter deep philosophical disputes. Indeed, the group supported no explicit philosophy of mathematics and characterized its approach as that of the *working mathematician*, whose attention concentrates on the solution of problems or the formulation of new theorems and theories, but who has no direct interest in philosophical or foundational issues (see e.g. Dieudonné 1970, 143).

As Mirowski (2002, 394) put it, <<the marriage of Cowles ... and Bourbaki was a match made in heaven.>>. Debreu had been trained in mathematics by Henri Cartan, a member of the Bourbaki group, and joined the Cowles Commission when the latter's research director was Tjalling Koopmans, who had close relations with the members of Chicago Mathematics Department, the stronghold of Bourbakism in the US. The warm reception of Bourbaki's message at the Cowles Commission was also favored by the major changes that had been going on there from the late 1940s:[5] as I said before, most Cowles members were disillusioned with their early empiricist work, so the emphasis in their research

[5] On the history of the Cowles Commission, see Christ 1952; Hildreth 1986; Mirowski 2002, especially Ch. 5.

agenda was shifting towards more theoretical work. More generally, Bourbakism provided an authoritative backing for those very few mathematical economists who in the early 1950s were pursuing the goal of the discipline's generality and conceptual integrity – a goal which, as I argue in the next §, eventually marked the transformation of postwar neoclassical economics.

Yet, it would be very misleading to associate the entry of the axiomatic method in economics exclusively with the Cowles Commission. While the latter did get married with Bourbaki, most neoclassical authors seem at least bigamous. Already in the interwar years there had been in fact other prominent examples of application of the axiomatic method, such as the first efforts to axiomatize demand theory carried out by Ragnar Frisch (1995a [1926]) and Hermann Wold (1943–44). The former is especially relevant because the founder of modern econometrics explicitly defended the view that in order to give an empirical content to economic theory the correct scientific method was to imitate what had been done by David Hilbert in his axiomatic foundation of geometry. In particular, following Hilbert's lesson of the complementarity between the axiomatic and the empirical method, Frisch proposed in a 1932 lecture that the axioms of economic theory be deduced from a number of imaginary experiments capable of capturing the essential features of economic reality (Frisch 1995b [1932], 489). Definitely, Frisch's axiomatic bride was not the same as Debreu's!

Even in the case of von Neumann, the existing renditions are weakened by their imperfect acknowledgement of Hilbert's philosophy of mathematics. For example, I have argued elsewhere (Giocoli 2003a) that von Neumann very carefully distinguished between models, like the 1937 general equilibrium one, where non-constructive proofs – unable of positively connecting with the empirical substratum – would suffice and models where constructive – viz., computable – arguments were also required, like the crucial demonstration of the minimax theorem in the 1944 *Theory of Games*. Furthermore, it is apparent to any reader of the latter book that the authors' position on the axiomatic method was much closer to Frisch than to Debreu. Evidence for that is provided by statements such as the following one:

<<Although an assertion [...] is required by common sense, it has no validity within the theory [...] unless proved mathematically. To this extent it might seem that rigour is more important than common sense. This, however, is limited by the further consideration that if the mathematical proof fails to establish the common sense result, then there is a strong case for rejecting the theory altogether. Thus the primate of the mathematical procedure extends only to establish checks on the theories – in a way which would not be open to common sense alone.>> (von Neumann and Morgenstern 1953, 361).

Indeed, in their axiomatic characterization of a game, von Neumann and Morgenstern strictly adhered to what they called the *classic* – that is, the Hilbertian – approach to axiomatics, which, in their words, aimed at achieving <<...an exact formulation for intuitively-empirically-given ideas...>> (ibid., 76). More than that, they clearly distanced themselves from the *modern* (Bourbakist?) axiomatic method, that is, from the requirement that mathematical concepts be formulated in a pure form, with no association with any intuitive perception (ibid., 74). As a consequence, von Neumann and Morgenstern took extreme care in ensuring that the names assigned to the pure concepts of their theory always recalled their intuitive background and even spent a few pages to defend the axioms in terms of empirical considerations[6] – hardly what a formalist die-hard would ever do!

Thus, it may be argued that, as far as the axiomatic method is concerned, the development of postwar neoclassical economics owes at least as much to Hilbert – the "true" Hilbert that I have tried to sketch in the previous § – than to Bourbaki or metamathematics. This should set the record straight with respect to, say, the frequent claims that neoclassical axioms cannot but be detached from reality and that economists should not worry too much about the interpretation of their analytical results – two statements with obvious historical and methodological implications. Yet, we still are not on target because even the finest appraisal of the kind of axiomatics employed in modern economics still falls short of capturing the real engine behind the latter's postwar transformation.

§4. *Demonstro ergo sum*

Let me now focus on one of the main legacies of the Hilbertian school. Starting from the early 20th century, the old, Volterra-style notion of "empirical" rigor, has been progressively replaced by the new notion of axiomatic rigor. The process was spurred by Hilbert's definition of mathematical truth as logical consistency:

<<...if the arbitrarily given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of existence and truth.>> (letter to Frege, 29 December 1899, quoted by Corry 1997, 117).

Such a definition neatly separates the purely logical aspects of the application of the axiomatic method from the, possibly empirical or intuitive, origin of the

[6] See von Neumann and Morgenstern 1953, 24-9, 73-7, 628-32.

axioms themselves. Thus, an argument has to be called "rigorous", that is, correct or true, if and only if it is the consistent outcome of a deductive process applied to the assumptions. That this is exactly the current and, above all, *exclusive* meaning of the word "rigor" when used in science (see e.g. the entry in the *OED*) bears witness to the lasting impact of Hilbert's axiomatic program.

How does this transformation relate to the evolution of modern neoclassical economics? The key can be found in the following passage by Roy Weintraub:

> <<The idea of formalization as axiomatization, associated with Hilbert, [...] had the consequence that modeling a phenomenon, or constructing a theory of a phenomenon or set of phenomena, came to be associated not with surfacing the link between the model and the experimental data, *but rather with establishing the integrity of the formal reasoning chains* which were the engine for discovery of new knowledge in that scientific field.>> (Weintraub 1998, 1843, emphasis added).

What I wish to argue is that, beside and beyond the economists' dissatisfaction with the empirical power of their analysis, what really drove the transformation of modern neoclassical economics in the direction of formalism and axiomatics was the economists' desire to achieve the highest possible generality and conceptual integrity of their analysis.

Generally speaking, such a desire may be said to pre-exist in any scientist's mind, where it often is at least as powerful as the willingness to improve the explanatory power of her analysis. My point is that, as far as neoclassical economics is concerned, *both* desires were stimulated in the interwar and postwar years by new epistemological currents, such as logical positivism and mathematical formalism, but I also claim that, at the end of the day, *it was only the latter* that made itself felt in the eventual outcome of the analysis, so that even behind the strongest declarations of allegiance to, say, the operationalist or experimentalist method, what we actually find is just the pursuit of an ever-higher level of generality and conceptual integrity.

The full credit for this point must be given to Ivan Moscati, of Bocconi University. What Moscati has done in his Ph.D. dissertation (Moscati 2003; but also see Moscati 2002; 2003a) has been to apply the categories of the Neo-Kantian epistemology of the Marburg School to the evolution of 20th-century demand theory. Moscati underlines that one of the core principles of this epistemology is precisely that what really drives the scientist's intellect is not the goal of obtaining an ever improved representation of reality, but rather that of achieving the highest possible degree of systematization via the ordering of phenomena according to a stable and structured system of theoretical objects which exhibit the highest possible degree of exactness and generality. A corollary

of this view is that, while such an intellectual drive to systematization may well lead to a plurality of theoretical approaches to the same phenomena, it also provides a strong test for evaluating alternative theories: the availability of a more general, exact and systematic theory will always lead to the discarding of the less general, exact and systematic. A further corollary is that the most proper way to express the relationships among the elements of a theory is through mathematical relations. This because, according to the Neo-Kantians, mathematics is a science of relations, rather than a science of quantities. Hence, the more exact and determined the connections between the elements of a theoretical system, the better they can be expressed in mathematical form. The progress of a scientific discipline towards its systematization then inevitably manifests itself in terms of a tendency towards an increasing mathematization.

What I will do here is simply to take the thesis that the Neo-Kantian epistemology effectively captures the intellectual drive behind the evolution of neoclassical economics – especially of its so-called Neo-Walrasian version – as a working assumption for the final part of my lecture. Two issues arise. First, does the history of 20th-century economics give any evidence backing this assumption? Second, what does the assumption entail for the overall characterization of modern economic orthodoxy?

To answer the first query, let me refer once again to my book, where I claim that the history of 20th-century microeconomics provides ample support in favor of the strive-for-conceptual-integrity argument. Indeed, what I have done in *Modeling Rational Agents* has been to argue that the whole evolution of modern decision theory may be read as the story of the neoclassical economists' long struggle to achieve a rigorous and truly general systematization of the agent's rational behavior under both certainty and uncertainty conditions. Moreover, even the strange trajectory of modern game theory – which, after a seemingly warm welcome, rapidly fell into oblivion for more than two decades, only to make its triumphant comeback in the 1980s and eventually gain its current status of theoretical core of contemporary mainstream economics – even that may be explained in terms of my working assumption, because it was only when neoclassical economists had become acquainted with the formalism of modern decision theory (and of general equilibrium theory too!) that they could begin to appreciate the rigor and generality of its interactive version – viz., strategic analysis.

What I wish to do here, however, is to review neither the history of demand theory – on which, as I said before, I also address you to Moscati's works, which provided ample inspirations for my own rendition of the topic – nor that of expected utility theory (EUT) or game theory. What I will do is something bolder –

perhaps *too* bold – namely, to argue than even some of the achievements of modern experimental economics may be read through the same lenses. More exactly, I would like to direct your attention upon a feature of the few experimental outcomes that have made their way through the solid ramparts of neoclassical orthodoxy, namely, their being only, and precisely only, those that do not really threaten the strive for conceptual integrity of mainstream economics.

Let me immediately narrow the validity of such a claim. What I have in mind is just one of the two main experimental approaches to economics, namely, the one pioneered by Kahneman and Tversky (K/T) – the so-called heuristics and biases (H&B) program. Indeed, as far as I can see it, the alternative approach – that championed by Vernon Smith and Charles Plott – seems to be less prone to the pressure of intellectual systematization and thus much more promising in view of a real transformation of the research attitudes of contemporary economics.[7] The same, I fear, cannot be said of behavioral economics – the field that attempts to integrate the findings of psychology into economics and that, as is well known, draws much of its inspiration from K/T's research.

§5. Formal dressing (still) required

The fundamental claim of the H&B approach is that people make their choices by relying on a limited number of heuristic principles which, while useful in reducing to simpler judgmental operations the complex tasks of assessing probabilities and predicting values, may well lead to severe and systematic biases in the actual choices (Tversky & Kahneman 1974, 1124). Such a claim has found one of its most important application in economics with prospect theory – the new analysis of decision-making under risk developed by K/T in their classic 1979 *Econometrica* paper (Kahneman & Tversky 1979).

As originally conceived of by K/T, prospect theory models choice under risk as a two-phase process: in the first phase, the available prospects are "edited" using a variety of decision heuristics; in the second, choices among edited prospects are determined by a preference function which is represented by a simple decision-weighted utility function – that is, by a generalization of expected utility which allows for misperceptions or subjective weighing of objective probabilities to be captured by well-defined decision weights.

The long tradition of attempts to generalize the assumptions behind standard EUT[8] easily explains why neoclassical economists focused their attention on the

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[7] See Bergstrom 2003, Ortmann 2003, Altman 2004, Lee 2004.
[8] For a review, see Starmer 2000.

second phase. What they found was indeed quite significant: K/T's utility function exhibited a series of properties (reference point, diminishing sensitivity, loss aversion)[9] that effectively captured some of the experimental features that systematically falsified the predictions of EUT. Both these properties and the possible alternative forms of the weighting function have given rise to a large literature and have helped establish the names of Kahneman and Tversky as the patron saints of today highly fashionable behavioral economics. This culminated with the awarding to Daniel Kahneman (jointly with Vernon Smith) of the 2002 Nobel Prize in economics – the first ever been granted to a psychologist. Yet, what is remarkable is that economists have *not* given the same credit to K/T's first phase of the choice process, the editing of prospects.

In their 1979 paper, K/T argue that individuals edit their prospects using several heuristics, such as that allowing the coding of outcomes as gains and losses relative to the agent's reference point, or that enabling the simplification of prospects through their combination and cancellation, or the so-called dominance heuristic which allows the elimination from the choice set of stochastically dominated prospects. In short, the editing phase explains why K/T's prospect theory may be considered an instance of a *procedural theory* of decision making, that is, of a theory that seeks to model the processes that lead to choice by assuming that agents draw on decision heuristics or rules of one kind or another and by specifying the conditions under which a particular heuristic or rule is followed in preference of another (Starmer 2000, 350).

It hardly needs to be stressed that the experimental evidence provided by psychology in favor of a procedural view of decision-making is overwhelming. The point is that procedural models – including K/T's editing phase – have been largely ignored even by those economists who most fervently believe in the cross-fertilization between economics and psychology. For example, despite the ample space given to K/T's 1979 paper, editing is completely overlooked in Matthew Rabin's essay in the *Scandinavian Journal of Economics* celebrating Kahneman's Nobel Prize (Rabin 2003). Perhaps even more surprisingly, Kahneman and Tversky themselves seem to have downplayed the importance of editing in later versions of their theory (see e.g. Tversky & Kahneman 1992).

Why did this happen? In other words, why do most economists – even those of a more experimentalist penchant – go on refusing to *fully* account for the evidence

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[9] The existence of a *reference point* imposes a kink to the shape of the utility function and entails that individuals evaluate gains and losses differently. *Diminishing sensitivity* means that the psychological impact of a marginal change decreases as we move further away from the reference point: this is captured by K/T's utility function being concave for gains and convex for losses. *Loss aversion* means that losses loom larger than the corresponding gains, as in the well-known endowment effect: this is captured by the utility function being steeper in the domain of losses.

offered by psychology? Why are they so little interested in describing the actual processes – not merely the outcomes – through which agents make their choices? I believe there is a straightforward answer: the adoption of a procedural view of decision making would entail too large a break with the economists' self-imposed goal of achieving the maximum generality and conceptual integrity of their theories, and thus too big a transformation in the overall image of economics as a scientific discipline (see next §). This instead is not the case when behavioral economists limit themselves to encompassing in standard models the formal properties of a heterodox utility function which is specifically designed to capture *just a bit* of the available experimental results.

Think again of the different fortune of the various parts of K/T's prospect theory. Take for instance the notion of a reference point. As Rabin has made clear in several papers (see e.g. Rabin 1998; 2002), adding a reference level to the arguments in the agent's utility function is a technically simple operation that allows the decision theorist to account for a score of meaningful empirical phenomena without requiring her to diminish the formal rigor of the analysis. It follows that working with reference levels has quickly achieved a remarkable popularity among behavioral economists. But take now K/T's dominance heuristic which, as I said before, is one of the key ingredients of the editing phase. This heuristic requires the decision maker to first scan the set of available options and then delete the dominated prospects *only if* they are detected. Hence, the dominance heuristic leaves open the possibility for some dominated prospects to survive its application: this of course aims at capturing a feature of the actual behavior of experimental subjects. However, the possible survival of dominated options also paves the way to potential violations of the transitivity and/or monotonicity of choices. Remarkably, the economists' reaction to the latter possibility has been fully in line with the strive-for-conceptual-integrity assumption: the potential violation of transitivity and monotonicity has been deemed <<...an undesirable result...>> of prospect theory (Quiggin 1982, 327) because the two axioms are considered the fundamental properties that any good theory of choice must satisfy. Or take another well-known feature of procedural models, namely, their often exhibiting a degree of indeterminacy in the decision rule. This is another unwelcome implication for neoclassical economists, because working with a set of decision rules, rather than with a single optimizing function, <<...complicate[s] the theoretical structure of models in ways that render them less compatible with the rest of economic theory.>> (Starmer 2000, 354).

In view of these difficulties, we can easily understand why economists have been so far quite selective in their efforts to encompass the experimental evidence. Several models have been developed that may at the same time account for some

of the empirical regularities of prospect theory, warrant the preservation of the key axioms and enjoy the neatness and tractability of single-function optimization. A prominent example is John Quiggin's 1982 rank-dependent EUT – still one of the most popular models that allow non-linear decision weights *à la* K/T to be built into a preference function that obeys the conventional axiomatic desiderata.[10]

The point is that the models in this class effectively get rid of the procedural elements of the editing phase: that they have nonetheless enjoyed a considerable fortune shows that success in modern economics is still not necessarily dependent on a theory's explanatory power with respect to experimental results. Indeed, the development of rank-dependent models has meant a *reduction* of this power, since important phenomena such as the well-known framing effect cannot be encompassed without explicitly dealing with procedural elements. So it is somehow ironic, as well as highly revealing of the attitude of most behavioral economists, that in the celebrating essay after Kahneman's Nobel, one may read that

<<…framing effects are more difficult to reconcile with and embed within standard economic analysis than most of Kahneman's other research topics…>> (Rabin 2003, 174),

as if the whole point of the economists' newly (re)discovered propensity to avail themselves of the insights from experimental psychology would just amount to finding a way to reconcile these insights with core neoclassical principles.[11]

The skepticism about the willingness of behavioral economists to escape from the straitjacket of formal rigor and conceptual systematization becomes ever stronger if we pay attention to the fact that the same H&B approach that has been so successful in economics has been subjected to severe criticism in its own field, psychology. Remarkably, the critiques have been addressed against both the empirical emptiness of the approach and its propensity to privilege a purely formal account of the rules of behavior. According to Gerd Gigerenzer, K/T's heuristics are

<<…mere verbal labels, or one-word explanations. There is no process model […] There is no explication of the characteristics of the situations in which a heuristic is successful and in which it would fail.>> (Gigerenzer 2005, 42).

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[10] See Starmer 2000 for further references.

[11] This actually seems to be Rabin's viewpoint: see e.g. what he says in Rabin 2002, 658, fn.1, where he establishes a parallel between the rise of psychological economics and the advent of modern game theory. The parallel is illuminating, if only for the lack of knowledge it reveals of the latter's history!

In short, the H&B approach is charged with being merely descriptive and devoid of any real explanatory power. But,

<<If the psychology of judgment ultimately aims at an understanding of how people reason under a bewildering variety of circumstances, then descriptions, however meticulous and thorough, will not suffice. In place of plausible heuristics that explain everything and nothing – not even the conditions that trigger one heuristic rather than another – we will need models that make surprising (and falsifiable) predictions and that reveal the mental processes that explain both valid and invalid judgment.>> (Gigerenzer 1996, 595)

The idea itself, underlying the whole of K/T's analysis, that decision-makers are systematically flawed bumblers has been challenged by those psychologists who, drawing on notions of *bounded rationality*, argue that individuals develop simple and effective decision rules that serve them well in many contexts, given the constraints under which the choice is made in terms of time, knowledge and cognitive ability. The focus in this alternative view is on *learning processes*: what is argued is that people do eventually converge to effective, and possibly rational, outcomes if only they have enough time and a high enough stake to think about them (Ortmann 2003, 569).

It is noteworthy that from the viewpoint of the bounded rationality, or learning, approach the difference between standard neoclassical decision theory and K/T's theory tends to vanish: given that both theories equate rationality with consistency and both call an error – i.e., irrational behavior – any deviation from consistency, what characterizes the latter with respect to the former is just the idea that decision makers are never so smart to be able to abide by the tight standards of perfectly consistent behavior (Altman 2004, 11). The learning perspective thus helps us understand why the H&B approach managed to conquer an audience in economics at the same time it was beginning to lose ground in psychology. As remarked by Andreas Ortmann (2003, 569), what economists have done has been to take K/T's results at face value, without acknowledging their disputed status. Even worse, we know they have done that quite selectively, by keeping just those results which fitted well within their consolidated framework of conceptual integrity and discarding all the rest. Indeed, as Rabin put it:

<<…psychological economics clearly expands the range of phenomena economists can successfully study, and does so in what clearly is *the spirit of economics*. >> (Rabin 2002, 658, fn.1, emphasis added).

That there may be more than one way to summon "the spirit of economics" – for example, by embracing the learning approach, which has an old, though always minor, tradition in our discipline – seems to be of no concern to someone who in a highly influential *JEL* survey has proclaimed his distaste for methodological disputes by deliberately limiting his review to

<<…what psychologists and experimental economists have learned about people, rather than *how* they have learned it.>> (Rabin 1998, 12; also see Rabin 2002, 659).

What Rabin fails to recognize is that his is precisely the attitude that – though probably involuntarily[12] – helps preserve the influence of another spirit, that of Nicolas Bourbaki, even upon modern behavioral economics.

§6. Might the ~~Force~~ Relation be with you

The paradoxical case of K/T's approach is just the latest instance of the long list of economic theories, when not whole sub-disciplines, that in the last 50 to 60 years have been shaped by the intellectual urge of so many economists to achieve the highest generality and integrity of their analysis. As I have tried to show in this lecture, it was mathematical formalism – especially in its Bourbakist version – that gave the decisive impulse to dislodge the other urge, that for empirical realism. The change in the notion of rigor bears witness to the catalytic influence upon economics of the transformation going on in the mathematical realm: in the sign of the axiomatic method, an economic model came to be said rigorous only when built upon a cogent axiomatic base, while the criterion for assessing its truthfulness became the mutual consistency of its formal relations (cf. Weintraub 2002, 100).

What we have ended up with is neoclassical economics as a logical, rather than empirical, science, whose role model is the mathematician, not the physicist, and whose sociology and value system have been borrowed from the mathematicians' community. As I have argued at length in my book, a large part of the most significant advances in postwar microeconomics have been formal in essence,

[12] I say "probably involuntarily" because if due attention is paid to the frequency with which Rabin resorts to numerical methods in order to show the implausibility of some traditional orthodox assumptions (such as exponential discounting: see Rabin 2002, 670-1), one might even argue that he is effectively back to using an "empirical", rather than formal, notion of rigor. However, he does not seem to realize that such computational arguments are quite alien to "the spirit of economics" – or at least to the latter's postwar axiomatic version he apparently supports.

though often concealed behind an empiricist façade. What has really mattered for successive generations of neoclassical economists has been first and foremost to make their way towards the deduction of ever more rigorous theories, so that even when progress towards a greater realism or empirical accountability has been made – like in K/T's case – it has been received in the literature only after it has proved instrumental in expanding, or defending, the generality of the axiomatic theory.

My argument can be effectively synthesized referring to the notion of *image of knowledge* that has been proposed by the historian of mathematics Leo Corry.[13] According to Corry, the appraisal of every scientific discipline requires that two different kinds of questions be tackled: questions of the first kind concern what he calls the *body* of knowledge, i.e., a discipline's theories, facts, methods and open problems (Corry 1996, 3); those of the second kind deal with the image of knowledge, i.e., with the discipline *qua* discipline. More specifically,

<<[t]he images of knowledge determine attitudes concerning issues such as the following: Which of the open problems of the discipline most urgently demands attention? What is to be considered a relevant experiment, or a relevant argument? What procedures, individuals or institutions have authority to adjudicate disagreements within the discipline? What is to be taken as the legitimate methodology of the discipline? What is the most efficient and illuminating technique that should be used to solve a certain kind of problem in the discipline? What is the appropriate university curriculum for educating the next generation of scientists in a given discipline?>> (ibid., 3-4).[14]

The images of knowledge exercise a considerable influence upon the creation, growth, relative evaluation and eventual oblivion of (parts of) the body of knowledge. Thus, Corry believes that the main task for the historians of a certain discipline is to identify the image of knowledge prevailing in a given period, to account for its evolution through time and to explain its interaction with the body of knowledge as an important factor in the discipline's development (ibid., 7).

Armed with Corry's dichotomy, we may eventually tackle the second issue that I left open in §4, namely, what does my working assumption of the economists' strive for conceptual integrity entail for the overall characterization of modern neoclassical orthodoxy? My answer is that in the interwar and, especially, postwar years, such a strive was boosted by the rise of mathematical formalism, so much so that it triggered a radical transformation of the discipline's image.

[13] The dichotomy has been first applied to the history of 20th-century mathematical economics in Weintraub 1998; 2002.

[14] Cf. von Neumann 1961 ?1947?, 7: <<What is the mathematician's normal relationship to his subject? What are his criteria of success, of desirability? What influences, what considerations, control and direct his effort?>>.

The two poles of the transformation were, on the one side, the traditional image of economics as a discipline dealing with *systems of forces* and, on the other side, the new image of economics as a discipline dealing with *systems of relations*. According to the system-of-forces view, economics is a discipline whose main subject is the analysis of the economic processes generated by market and non-market forces, including – but by no means exclusively – the processes leading the system to an equilibrium. According to the system-of-relations view, instead, economics is a discipline whose main subject is the investigation of the existence and properties of economic equilibria in terms of the validation and mutual consistency of given formal conditions, but that has little if anything to say about the meaningfulness of these equilibria for the analysis of real economic systems.[15]

From this fundamental distinction there follow other crucial differences. Take Corry's list of typical questions raised by the image of knowledge. Which of the open problems of economics most urgently demands attention? According to the system-of-forces image, the answer was the explanation of how and why a certain equilibrium had been reached, while according to the system-of-relations image it is the demonstration of existence of an equilibrium, though <<...not of [its] actual, empirical existence but of [its] conceivable, logically or mathematically non-contradictory "existence".>> (Hutchison 2000, 19). What is to be considered a relevant argument? In the system-of-forces image, a rigorous explanation of economic phenomena was one that explicitly accounted for the influence of all those market and non-market forces that could be identified via empirical observations; in the system-of-relations image, the fundamental requirement is the axiomatic rigor of the argument, that is, its logical robustness and economy of assumptions. What is the most efficient technique that should be used to solve economic problems? In the system-of-forces image, the mathematics was that of classical mechanics, i.e., the traditional tools of calculus; in the system-of-relations image, it is topology and, more generally, all the tools that privilege the requirement of consistency over that of effective calculability. Finally, what is the appropriate university curriculum for educating an economist? The role model in the system-of-forces image was the physicist *à la* Volterra, while that in the system-of-relations image is the (Bourbakist) mathematician. This entails a radical modification in the incentive, rewarding and formative system of the economists' community: for example, finding a new application of a well-known concept or an elegant generalization of an established result has become the safest – though sometimes extremely difficult – way to obtain an academic payoff,

[15] The two views have been originally proposed in Dardi 1983.

much more than the never-fully-exhaustive effort to explain a particular feature of economic reality.

Actually, the new image and role model have also carried with them a major change in the organization of economic research. John von Neumann once remarked that the typical attitude of the mathematicians is to split their subject into a great number of sub-fields, with no practitioner capable of handling more than a very small fraction of them. Conversely, the subject of physics is usually very concentrated, on account of the objective nature of its open problems. While in fact a key puzzle in physics *must* be answered to avoid leaving something unexplained or contradictory in the way we depict the functioning of nature, a mathematician is basically free to abandon even the most important problems in her sub-field and turn to something else (von Neumann 1961 [1947], 8). The same difference applies to economics: the system-of-forces image favored the concentration of the research efforts upon the few crucial issues – such as value, distribution, production, consumption – that most clearly displayed the working of real world economic forces; the system-of-relations image, instead, gives the researcher a much larger freedom to apply her tools to a wider and more heterogeneous range of problems – sometimes of a purely analytical kind – that neither necessarily nor directly arise from economic reality and that are seldom truly essential for our overall understanding of it.

§7. When rigor becomes a penalty

The new image of economic knowledge is not devoid of practical consequences. So let me conclude my lecture by telling you the story of the potentially lethal danger to which the system-of-relations view has recently exposed one of the most fashionable – and remunerative – sub-disciplines of neoclassical economics. Starting from 1993, in a series of antitrust cases known in the literature as the Daubert cases, the U.S. Supreme Court has stated the rules for expert testimony to be admitted in courts. The Court has held that the trial judge must serve in a <<gatekeeping role>> by making a <<...preliminary assessment of whether the reasoning or methodology underlying the testimony is scientifically valid and of whether that reasoning or methodology properly can be applied to the facts in issue.>>. The expert testimony is admissible only if it is <<...sufficiently tied to the facts of the case that it will aid the jury in resolving a factual dispute...>>.[16]

In a later (1997) ruling the Supreme Court has added that an expert testimony should not be admitted if the court concludes that <<...there is simply too great

[16] Werden 2003-04 surveys the Daubert cases and gives the references of Supreme Court rulings.

an analytical gap between the data and the opinion proffered.>>. According to the Court, in fact, the subject of an expert's testimony must be <<scientific knowledge>>, and <<in order to qualify as "scientific knowledge" an inference or assertion must be derived by the scientific method>>. More specifically, the Court has listed since 1993 five criteria to determine when a theory is indeed scientific knowledge: 1) whether the theory can be, or has been, tested; 2) whether it has been subjected to peer review and publication; 3) what is its known or potential rate of error; 4) whether there exist standards controlling the theory's operation; 5) whether the theory has gained widespread acceptance.

It is hardly surprising that these Supreme Court principles caused a certain turmoil in the economics profession. Our US colleagues were struck by the new rules which threatened to curtail, if not put an end to, a very lucrative activity, that of acting as experts in antitrust and other legal cases. The point was, in fact, whether economics satisfied the standards of scientific reliability set by the Supreme Court. Brutally, is economics really "scientific knowledge"? What kind of economic theories, if any, can be said to satisfy all the above-mentioned criteria? Think of the most sophisticated models of collusion in industrial organization theory, which are all based on dynamic game theory, that is, on notions and tools such as Bayesian Nash equilibrium, intertemporal optimization and the likes. Does any of these meet all the criteria? Or think of the most sophisticated econometric techniques: while they may surely be admissible in court if competently applied, their conclusions would never stand the Supreme Court scrutiny even for relatively simple tasks, such as determining whether a given relationship between prices and costs is evidence of an illegal inter-firm agreement.

The alarm bell rang even louder when some US district courts started excluding economists because their testimony, <<...although thorough, sophisticated and often well-grounded in the relevant scientific literature...>>, suffered from <<excessive speculation>>, or contained <<...too many assumptions and simplifications that are not supported by real-world evidence.>>.[17] These words reveal that, to say the least, US courts seem not so willing to be summoned beneath the sign of the axiomatic method... Now, assume you were a lawyer. Would you expose yourself to the risk of being ridiculed in court by presenting a mathematical economist's expert testimony as a piece of real "scientific knowledge"? Or assume you were an economist whose expert testimony has been excluded on account of its unsatisfactory scientific foundations. How is this going to affect your future job opportunities as a court consultant?

[17] See again Werden 2003-04 for the references of these quotations.

Fortunately, the crisis has been solved by another (1999) Supreme Court ruling which has stated that similar admissibility rules also apply to *technical*, not just scientific, knowledge. This effectively saved the day for US economists, because it allowed them to part from the embarrassing company of the true scientists, such as the physicists, the chemists or the biologists, and join the ranks of the mere technicians, such as the accountants and the engineers – or the plumbers and the carpenters – i.e., of all those professionals whose standards of admissibility in court require the conformity to the best, viz., the most *rigorous*, practices in their respective field, rather than the ability to provide real "scientific knowledge". But as the underdog David Novick reminded us in 1954, a technician may also be defined as somebody who applies her, usually practical, knowledge on a case-by-case basis, that is, by referring to the very specific data (and numbers) of the situation under scrutiny. The moral of this story is that US economists did manage to keep a profitable business alive, but only by swallowing their scientific pride via the tacit admission that their most sophisticated axiomatic models cannot be marketed as "science". Or, if you like, it may well be true that "*in Debreuviano signo vinces*", but when it is real money that is at stake, you'd better stick to the good, old Gerolamo Boccardo.

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