What Could Mathematical Rigor Mean? Three Reactions to Godel in mid-20th Century Economics

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Mathematics and the Epistemological Warrant at Mid-Century

After a hiatus of four decades, it now appears to be possible to again raise the question of the role and function of mathematical expression in economic discourse.[1] In a sense, the length of the interval between the debates--dating the previous dispute from the 1954 *Review of Economics and Statistics* symposium--is a tribute to the effectiveness of Koopmans' *3E* in the interim. For most economists, and certainly for every student intending to become an economist, the question of the status of mathematical expression was "black-boxed" in order to get on with the more important scientific work in the 1950s through the 1980s. The whole structure of recruitment into the discipline was shifted in favor of equipping students with the mathematical "tools" in the three areas of microeconomics, macroeconomics and econometrics; the implication being that one worked "outwards" from the mathematical tools to the empirical materials, which would vary from case to case.

Now that the question of mathematics in economics can be reopened, it becomes possible to recast these events in their larger historical context. Far from representing some extension of a generic scientific method, we shall suggest that the neoclassical orthodox stance, as exemplified by Koopmans' *Three essays in the state of Economic Science (3E)*, was the outcome of one of a number of incipient reactions to deep problems of the justification of mathematics which arose in the 1930s.[2] Very briefly, the defense of the central importance of mathematics in economics must be divided into two distinct periods: the first, running from the mid-nineteenth century to the 1930s, coincides with the first phase of the development of neoclassicism. In this phase, proselytization for mathematical economics was unselfconsciously identified with the acceptance of the neoclassical model (Mirowski, 1989); for the major players, marginalism was mathematical economics. When pressed to defend the need for such a novel format of analysis, responses generally assumed one of two genres: either it was claimed that the subject matter of economics was intrinsically quantitative, thus requiring mathematical expression, or else various analogies were drawn from physics to suggest that somehow mathematics was inextricably bound up with the very method of science. Since neoclassical formalisms had themselves been appropriated from mid-century physical energetics, there was some basis for the recourse to physical analogies. However, neither retort was taken much beyond a superficial level, probably because the challengers rarely had any familiarity with mathematics or the physical sciences, nor were they philosophical adepts. For instance, the putative quantitative character of prices was not explored; nor were there serious attempts to discuss how mathematics was being used in the physical sciences (Mirowski, 1986).

It is therefore the second phase of the defense of mathematical discourse in economics, dating from the 1930s, which is our present concern. This period coincides with the widespread incorporation of mathematics into economic journal articles (Mirowski, 1991b); but the change in character of the justifications was not merely a supply response. The ratcheting upwards of the level of competence in economic discourse coincided, probably entirely accidentally, with a profound upheaval in the philosophy of mathematics and the subsequent self-understandings of the goals and aspirations of practicing mathematicians. This story has been sketched in popular venues (Davis & Hersh, 1981; Kline, 1980; Aspray & Kitcher, 1988); it is commonly associated with the devastation of the formalist program of meta-mathematics by the incompleteness theorems of Kurt Godel (Nagel & Newman, 1958; Shankar, 1989). One might generally suspect that results as abstruse and complex as the inescapable character of formally undecidable propositions in the first order predicate calculus would take an extremely long time to have any impact upon such a seemingly distant realm as economics, if indeed they ever would bear any relevance at all; but it is our thesis that, contrary to all such expectations, the impact was swift and far-reaching. In a nutshell, at least three of the major strains of orthodox mathematical economics in the latter half of the twentieth century all can be traced as reactions to Godel's theorems. Tjalling Koopmans, by rendering the initially favored response (via Bourbaki and Gerard Debreu) in a palatable and popularized form in *3E*, set the tone and the agenda for all later economic debate.

Since this narrative is complicated, it would be best to clarify what is and is not being claimed at the outset. We are not stating that economists in general were themselves concerned with meta-mathematics, nor that they regarded Godel-numbering as an especially apposite metaphor for the economy, or anything else of the sort. Further, we are not claiming that economists' responses to Godel's theorems followed any of the major philosophical schools of meta-mathematics in their responses; as far as we can tell, there was no "intuitionist" position in economics, no conventionalist logicism, no detectable Platonism. Instead, we are asserting that problems raised by Godel's work concerning both the images of mathematical endeavor and the content of mathematical practice (Corry, 1989) called forth a number of pragmatic shifts in foundations, and that in at least three cases, those responses were translated directly into novel styles and practices in the constitution of mathematical economics. Much of this activity was channeled through the institution of the Cowles Commission in the 1940s/50s and thereafter diffused outward through the economics profession. For each of our three cases--John von Neumann and game theory, Herbert Simon and bounded rationality/artificial intelligence, and Gerard Debreu and Tjalling Koopmans and "general equilibrium theory"--can be linked to a deep concern with the justification of mathematical formalization in the wake of Godel's upheaval.

While much of our subsequent narrative will operate at the rarified level of ideas and concepts, it may not be amiss here to note that in two of the three cases, there were also a few more personal links to Godel. It has been sometimes noticed (Weintraub, 1985, pp.64,70) but not analyzed that Godel was present at the revival of Walrasian general equilibrium theory in Karl Menger Jr.'s Mathematical Colloquium in Vienna in the late 20s/early 30s. He was one of the editors of the *Ergebnisse* in which Abraham Wald's first papers on the solvability of the Walrasian equations was published, and indeed attended the seminar where the results were announced, putting one of his own comments on record (Baumol & Goldfeld, 1968, p.293). The *Ergebnisse* also contained von Neumann's 1937 paper on the expanding economy; but Godel's relationship to von Neumann and his economist collaborator Oskar Morgenstern were much closer than that. Von Neumann was very impressed with Godel's theorems, as explained below, and was subsequently responsible for bringing him to Princeton's Institute for Advanced Studies in 1933. There Godel periodically came in close contact with both von Neumann and Morgenstern, discussing with them, among other things, their path-breaking work on the development of game theory.[3]

Other than in these specific instances, Godel's influence upon mathematical economics was more indirect, but not any less portentous for that fact. To put the case with somewhat misleading brevity, each of the three major developments of 20th century mathematical economics were conditioned reactions to the aftermath of Godel's results. For game theory, it was part of the realignment of von Neumann's ambitions for mathematics after the destruction of Hilbert's formalist program. For Herbert Simon, it was the re-assertion of a revived reductionist logicism in the form of artificial intelligence. And for Gerard Debreu, and thus by inheritance and inclination, Tjalling Koopmans, it was the recasting of the program of Walrasian general equilibrium into a Bourbakist idiom. While all three are fascinating instances of the unintended consequences of changes in one field feeding back into the construction of the subject matter of another, we shall allot only brief attention to the first two cases, for purposes of comparison and contrast, the better to concentrate upon the third.[4]

The reaction of von Neumann to Godel's results is well-known in the history of mathematics. Von Neumann was in attendance at the original announcement in Konigsburg of Godel's first theorem, immediately appreciated its import, and was just barely beaten out by Godel himself in reporting the second theorem (Dawson in Shankar, 1989, pp.77-8). He made it crystal clear in any number of subsequent commentaries that he regarded Godel's theorems as "a landmark which will remain visible far in space and time" (in Shankar, 1989, p.60), one that had rocked the very foundations of mathematics. His famous talk "The Mathematician" explained in almost confessional tones the wrenching experience of having his world turned upside-down:

> ..the very concept of 'absolute' mathematical rigor is not immutable. The variability of the concept of rigor shows that something else besides mathematical abstraction must enter into the makeup of mathematics... My personal opinion, which is shared by many others, is that Godel has shown that Hilbert's program is essentially hopeless...I know myself how humiliatingly easily my own views regarding the absolute mathematical truth changed during this episode, and how they changed three times in succession...At a great distance from its empirical source, or after much 'abstract' inbreeding, a mathematical subject is in danger of degeneration... Whenever this stage is reached, the only remedy seems to me to be a rejuvenating return to the source: the rejuvenation of more or less directly empirical ideas. (von Neumann, 1961, pp.4,6)

While there is some controversy over the specifics of the interpretation, an argument can be made that the approach of von Neumann in 1928 and that in 1944 to the theory of games was transformed by this intervening event.[5] What was initially intended as a further exemplar of Hilbert's program of the axiomaticization of all formal disciplines to settle once and for all their completeness and consistency in 1928, was converted in 1944 to a rejuvenation of a degenerate formalism by infusing some direct empirical inspiration. But what precisely was the empirical knowledge brought to this later incarnation of the theory of games? It was surely not anything the economists had to offer: that was made clear by the numerous disparaging remarks about neoclassical theory in the text, and the even sharper dismissals that von Neumann made in private (Mirowski, 1992; Leonard, forthcoming). Throughout his career, Morgenstern was never of a confirmed empiricist bent, preferring rather to make theoretical criticisms. The empirical inspiration of von Neumann, never made explicit in the 1944 text, was the ambition to ground mathematics in the logical operations of the brain. *The Theory of Games and Economic Behavior* was composed in the middle of a major shift in von Neumann's interests towards computers, MacCulloch & Pitts' abstract models of the neuron, the theory of automata, new probabalistic logics, and their accoutrements. Historians of mathematics have noticed the analogies to the theory of games (Aspray & Burks, 1987, pp. 367-8); while historians of economics have yet to grasp their significance. In short, game theory was one relatively minor component in a larger program to simulate logic as it was lived by the individual organism, rather than to ground it in some absolute Platonic Rationality. This, we believe, explains von Neumann's unrelenting insistence upon probabalistic formalisms; his focus of the "reliability" of components or inferences; his dislike of "non-cooperative" solution concepts; and his disdain for the neoclassical imitation of classical mechanics. He wrote, "All of this will lead to theories which are much less rigidly of an all-or-none nature than past and present formal logic. They will be of a much less combinatorial, and much more analytical, character. In fact, there are numerous indications to make us believe that this new system of formal logic will move closer to another discipline which has been little linked in the past with logic. This is thermodynamics" (in Aspray & Burks, 1987, p.407). Instead of the neoclassical version of nineteenth century energetics, here we have a more up-to-date version for the 20th century.

Since so many historians have misrepresented game theory as an obvious extension of the earlier Hilbert program (Punzo, 1991; Ingrao & Israel, 1990), it will be important to distinguish it from other attempts to ground mathematics in logic or rigor or abstract truth. First, it was designedly empirical, in the sense that it was intended to be based upon physiological aspects of the individual brain. Second, it could still be axiomatic in format, but this was only for the sake of convenience, and was not to be treated as an end in itself. "He treated the individual elements of automata, whether neurons or vacuum tubes, as black boxes having certain well-defined, outside, functional characteristics and assumed to react to certain unambiguously defined stimuli, by certain unambiguously defined responses. The electrochemical processes within the neuron or the electrical processes in the vacuum tube that underlie the external behavior of these elements are not disclosed by the axioms" (Aspray, 1990, p.191). Third, the designated "economic" relationships should be treated as just another layer of parallel (or multiplexed) processes between the individual transactors, superimposed upon the processes within the brain: more unambiguously defined response sets linked to more unambiguously defined stimuli. Isomorphisms between levels were treated as individual instantiations of Turing machines, and not as any manifestation of some deeper "reality": computers can simulate neurons, neurons can simulate computers; computers can simulate the playing of games, gaming can simulate brain processes. The central importance of mathematics derives from the pervasive metaphor of calculation at all levels, and not from its embodiment of any particular logic. Fourth, the system is finite, fallible and unfinished--the very opposite of the goal of Hilbert's formalism. In game theory, automata and neurons, von Neumann held out the hope that the evolution of the processes themselves could eventually be described, leading to a more dynamic notion of rationality.

Thus stood the legacy to economics of one of the greatest mathematicians of our century; though the extent of the faithfulness of contemporary game theorists to this legacy is a question best left for another venue. We instead here turn to the second response to Godel's bombshell, that of Herbert Simon. The superficial similarities of their trajectories might at first mislead us: both von Neumann and Simon never had a major disciplinary identity in economics; both ended up doing something which might be called "artificial intelligence"; both had very good relationships with the Cowles Commission, yet both firmly rejected the neoclassical theory they found there. Whatever the resemblances, from the vantage point of the justification of mathematical formalization in economics their programs were as different from one another as they were different again from the Cowles orthodoxy represented by Debreu and Koopmans. Plumbing the depths of these differences will help us to understand the true shape and character of that orthodoxy when we encounter it in the next section.

Herbert Simon, unlike von Neumann and Debreu, was not trained as a mathematician, but rather as a political scientist. He tells us in his autobiography that he was largely self-taught in mathematics, but had the good fortune to have as a colleague and friend at the Illinois Institute of Technology the very same Karl Menger whom we encountered above as colleague of Kurt Godel in Vienna. He also tells us of an incident concerning Godel in his own words:

> Toward the end of my stay at IIT, I had a luncheon conversation with Karl Menger that I cannot forget. He had started his career, he said,with a deep interest in logic and the foundations of mathematics. The publication of Kurt Godel's famous Impossibility Theorem struck him a blow from which he never recovered... Even thinking about the subject depressed him, and as he recounted this story, he gradually subsided into a gloomy silence that continued through the lunch. (Simon, 1991, p.101)

Through an astounding sequence of coincidences, Simon was ushered into the inner sanctum of mathematical economics. By virtue of his first job being in Chicago, combined with some prior personal connections with William Cooper, he was invited to join the weekly seminars of the Cowles Commission. The accident of his then-current interest in urban migration got him included in a major government-funded project on the economic impact of atomic energy run by Jacob Marschak and Sam Schurr at Cowles, which in turn led to consulting work at RAND, which led to encounters with early computers, game theory, and John von Neumann. That, in turn, led to his unusually rapid encounter with *The Theory of Games and Economic Behavior*, which he says allowed him to write one of the very first reviews of the book (Simon, 1991, p.108). One out of the many things which is striking about this sequence of events is Simon's immediate principled opposition to the general program of game theory, a stance which he has maintained up to the present day.[6]

In his early career, Simon developed an enviable reputation as an organizational theorist, a political scientist, and perhaps an applied economist. Yet the one thing he would not have been considered at the time was a mathematical theorist; nor certainly an innovative methodologist of mathematics in economics. This changed in 1955, by his own account. After having spent twenty years researching how decisions were made and enforced in markets, firms and political organizations, under the influence of Albert Newell and Clifford Shaw he turned his attention to psychology, or more properly, to the modelling of logical decisions in electronic circuits. In contrast to von Neumann, he insists that this was *not* an attempt to mimic the operation of neurons; rather it posited an intercalated level of symbol processing situated midway between complex thought processes and neurobiology (Simon, 1991, p.192). What it resulted in, under the diverse confluence of Whitehead and Russell's *Principia Mathematica*, Alfred Lotka's *Elements of Physical Biology*, Claude Shannon's information theory, the cybernetics movement through Nicholas Rashevsky (Heims, 1991), and a host of other mid-century mathematical influences, was the first computer program that solved non-numerical problems by selective search. His paper, "A Behavioral Model of Rational Search" which had been written at RAND in 1952, was the first inkling of what later became better known variously as his theory of "satisficing" or "bounded rationality." The structural similarities of his early artificial intelligence (AI) program and the "bounded" alternative to neoclassical choice theory are transparent: that is because they share the same general philosophy of mathematical formalization.

Economists have very little idea of the range and scope of Simon's accomplishments, even after he was awarded the Nobel Prize in their discipline. He is accorded a much more exalted place in histories of artificial intelligence as one of the progenitors of that discipline, the author of the very first AI program (Crevier, 1993, pp.41 et seq). It is extremely significant that the output of this program was the "proofs" of the first 52 theorems of Russell & Whitehead's *Principia*.[7] Because mathematics was associated with rigorous thought in the minds of many, Simon conceived of a masterly method of confounding those expectations. He chose this landmark attempt by Russell and Whitehead to reduce all mathematical reasoning to logic--which, recall, had also been thoroughly dashed in the interim by Godel, a fact which Menger had bemoaned directly to him--to demonstrate that the project could apparently be justified by developing mathematics (really, algebra) from simple heuristics and blinkered goal-directed search instead. The project diverged appreciably from that of von Neumann, for whom basic mathematical logic was hard-wired into the neurons; for Simon, it was the product of heuristic search in the realm of symbolic manipulation. Buoyed by this success, Simon has moved from one grand hyperbolic prediction to another, recently claiming that not only do machines now actually think, they can also "discover" valid scientific theories (Crevier, 1993, p.108; Simon, 1992). His equation of all human thought with some version of formal logic does rather date his perspective in the modern context, nevertheless rendering him all the more a contemporary of other such figures like von Neumann, Debreu and Koopmans. There are many rival streams of AI now in existence which maintain that logic alone is not sufficient to adequately mimic human thought (Crevier, 1993; Simon, 1991, p.192). The importance of this work for our present narrative is that Simon managed to innovate a completely different response to the crisis precipitated by Godel, and reprocessed it into an alternative approach to economics. From the very beginning, Simon was driven by a conviction that the neoclassical economists he had encountered at the Cowles Commission and elsewhere were not all that serious about describing the formal foundations of rationality, whereas he was (1991, p.193). Sometimes he portrays this tension as a lack of interest on the part of neoclassicals in concerted empirical inquiry into how people actually make decisions (1992). But in one instance, especially significant for this biography, he traced his objections to the philosophy of mathematics.

> Among these good friends...the two of whom I think most often are Jascha Marshak and Tjalling Koopmans... My professional meeting ground with Tjalling was economics and econometrics. And on that ground we found the most profound difference between us. While we were both committed to 'hardening' the social sciences with the help of mathematics, mathematics meant something entirely different to Tjalling than it did to me. I discovered this--much to my amazement--at a dinner at our house... For me, mathematics has always been a language of thought... It is the tool I use to arrive at new ideas. This kind of mathematics is relatively unrigorous, loose, heuristic. It is physicists' mathematics or engineers' mathematics rather than mathematicians' mathematics. For Tjalling Koopmans, it appeared, mathematics was a language of proof. It was a safeguard to guarantee conclusions were correct, that they could be derived rigorously. Rigor was essential. (I have heard the same views in even more extreme form, expressed by Gerard Debreu; and Kenneth Arrow seems mainly to share them.) I could never persuade Tjalling that ideas have to be arrived at before their correctness could be guaranteed, and that the logic of discovery is quite different from the logic of verification. (Simon, 1991, pp.104-107)

Bringing a Newcastle to Cowles

In the earliest days of Cowles, there was no shared or fixed philosophical position with regard to mathematics, other than the conviction that it was somehow good for economics to have more of it. Indeed, the absence of a well-defined stance was necessary at that stage, given the diversity of figures and approaches attracted to the fledgling unit. It is also doubtful that Koopmans set out on his economic career equipped with a well-developed philosophical position on the role and significance of mathematics, since he gives no indication of having thought through the possible range of justifications in any of his earliest writings. One suspects, however, that many of his early implicit attitudes towards formalism were picked up from his mentor Hendrik Kramers. Kramers had made his own reputation in physics as a master of its formal aspects, particular with respect to statistical mechanics. And yet his biographer Martin Dresden suggests that this facility was also a burden, as Kramers would tend to get sidetracked into formal mathematical questions which were no longer at the center of interest in physics, regarding them more as a personal challenge (1987, p.318). This penchant took on a tragic aspect for Kramers, since he "kept reiterating throughout his life that just those who attempted to produce rational explanations of the natural world are most cognizant of the limitations of their efforts" (1987, p.193). This tension between the elevation of virtuosity and the personal suspicion of *l'art pour l'art* had become part of the deep structure of Koopmans' personality as well.[8]

While Koopmans had no particular motivation to assume the mantle of philosopher before Cowles, the situation had changed in the late 1940s. The widespread attention to the "Measurement Without Theory" paper and the subsequent controversy with the NBER did much to make his name known in more diverse economic circles; and his opinions as a "methodologist" began to be sought. His accession to the research directorship of Cowles in 1948 forced him to confront many competing conceptions of "good research" in ways he had hitherto managed to avoid or sidestep. Within Cowles, there was the wrenching change of direction away from econometric endeavor. There had been the rejection of Katona's stress upon psychology and survey research methods, and some backpedalling from the Keynesian orientations of Lawrence Klein and Donald Patinkin. And then there were the frictions with outsiders of various degrees of remove: the hostility of members of the Chicago economics department, paroxysm of discontent in the larger economics profession, and the doubts of outside funding agencies. All demanded to a greater or lesser extent some more explicit renderings of account of the goals and successes of the mathematical program in economics; and given the various shifts in personnel and in allegiances at Cowles, the task primarily fell to Koopmans.

We might present a sampling of evidence to illustrate the varieties of external pressures with regard to the use and abuse of mathematics. One illustration of the tensions with the Chicago economics department comes from some correspondence between Koopmans and Milton Friedman over the acceptability of the thesis work of a Ph.D. student in the department.[9] Friedman wrote:

> If proofs and analyses have been given in elementary mathematics, it may be desirable to translate them into higher mathematics, but surely this translation does not qualify as a contribution to knowledge of a kind that should satisfy the requirements for a doctoral thesis. On the other hand, if in the process of translation either from higher mathematics to more elementary mathematics, something is added to the economic analysis or a flaw in the proof is detected and rectified, this does seem to me to be a contribution... Indeed, it seems to me really intolerable to state or imply that there are flaws in earlier proofs or that this is the first rigorous proof of a proposition, without documenting the statement in question.

This complaint, which we might crudely reference as "form vs. content," would come back to haunt Cowles persistently. The same problem arose in Cowles' attempts to secure long-term funding outside of the Cowles family. Should the academic unit be structured along the lines of identifiable problem areas and existing social science disciplines, or should it predicate its very existence upon the mathematical competence of its members? This problem was addressed in the application to the Ford Foundation of 1951, written largely by Koopmans. It begins by giving some examples of what Cowles would argue were the "new methods of analysis": Arrow's *Social Choice and Individual Values*, von Neumann and Morgenstern's theory of games, Norbert Wiener's cybernetics, and some work done on linear programming for the Air Force. The problem with these examples were that only the first and the last were actively promoted at Cowles; and in Arrow's case, one might suggest the results were more of a negative character than representing the kinds of breakthroughs Cowles wanted to promote to the funding agencies. But this was not the tack taken in the proposal; rather than utilitarian principles, these exemplars were praised on different grounds:

> While the examples quoted are only first beginnings in the theory of social and economic behavior and organization, they hold the promise of introducing rigor and clarity into the social sciences... It may be added that at present it is no longer necessary (and sometimes ineffective) to urge upon social scientists emulation of the example of the physical sciences in regard to rigor, clarity and method. The studies referred to in this section can stand the test of comparison in these respects, and in addition go to the heart of social science problems through logical and mathematical methods that are appropriate to these problems (rather than being borrowed from the physical sciences). Using the universal language of logic and mathematics to link behavior postulates with observable consequences, these studies provide cumulative and verifiable knowledge of social phenomena.[10]

Here we observe the position developed in *3E* in its embryonic form. No longer does Cowles promise specific projects, specific problem areas for research, specific empirical techniques, nor indeed even any specific commitment to any disciplinary identity. The standards of the physical sciences are invoked, but now in an entirely new guise, as exemplars of the rigor and clarity which is the natural fruit of mathematical inquiry. Greater epistemic weight is shifted onto the mathematics themselves, to such an extent that Cowles was unashamed to admit to its prospective sponsors that, "With one possible exception (Simon) the present staff of the Commission...can claim no special competence in the tomes of the social science literature, nor do we think that this should be a primary criterion in the selection of additional staff" (p.14). But then, why should anyone believe that randomly recruited mathematical adepts should have any better chance of transforming economics than perhaps randomly recruited particle physicists, or corporate managers? If the Cowles program of simultaneous econometric estimation had stalled, then what precisely were the fruits of mathematical rigor? This was the methodological conundrum faced by the research director of Cowles in the late 1940s. Further events forcing Koopmans to enunciate a methodological position were the rising chorus of complaint with regard to the mathematicization of the economic literature in the early 1950s. Much of this literature is now forgotten, but Koopmans himself did acknowledge quite a lot of it, citing among others Waugh (1953) and Stigler (1950) and even some complaints in *Econometrica* itself, such as those by Clark (1947) and Allais (1954). Events were apparently brought to a head by a symposium arranged by Seymour Harris in the November 1954 *Review of Economics and Statistics*, precipitated by a *cri de coeur* of one David Novick. In a nasty ambush, eight longer "responses" were commissioned by Paul Samuelson to chasten Novick; Samuelson himself called it a "slugfest". One wonders how strong the case of the mathematicians was on its own merits, given the overkill of all eight of the respondents being unvarnished advocates of mathematical discourse in economics, and the unabashed scorn of their contributions. Mr. Novick with his modest two-page note didn't stand a chance; but a little justice might be injected retrospectively if we point out that it did touch most of the exposed nerves of the mathematical community, for instance, asking why the methods of the physical sciences should simply be adopted without measured justification; querying under what conditions the mathematical symbol "refers" to the underlying economic entity; wondering what the exercise portends if the underlying data is in principle empirically inaccessible; asking how mathematics differs from logic in the vernacular; and noting that there "is a tendency to assume that expressing these same theories in mathematical form creates absolute knowledge and eliminates [any] challenge" (p.358). Epigoni of the modern "Rhetoric" movement in economics may wish to contrast the modest, unassuming tone of Novick's contribution with the rude derision and contempt of his detractors; with the worst offender then being one of the fellow-travelers of that movement now, Robert Solow.

Two of the more even-handed respondents were Jan Tinbergen and Tjalling Koopmans. Tinbergen admitted that "there are examples of engineers or physicists hunting 'analogies' between physics and economics and thereby biassing their theories" but great mathematicians would not make that mistake; nevertheless one might wish to look at cases less realistic but more amenable to mathematicization first, simply for convenience. But he continued, "Being myself a mathematician of only modest knowledge I often experience considerable difficulties when reading Cowles Commission stuff. The general recipe I venture to recommend here is that a new method or a new idea should always be illustrated by the simplest conceivable case"(1954, p.367). It would have been difficult for Koopmans not to read this as a gentle reprimand from his former mentor. For his own part, he chose to directly engage in analogies with the discipline of physics, referencing his personal experiences on this occasion.

> There is remarkable similarity between the present stage of economics and the situation which arose in physics in the 1930s. The intensive use of matrix algebra and group theory by the developers of quantum mechanics gave rise to strongly felt protests on the part of experimental, general and even some theoretical physicists. Alarm was expressed at the increasing tendencies to a formalism of which the function was neither generally apparent, nor even yet fully visible to the developers of the new theories. However, since that time the clamor has abated and quantum mechanics has become an accepted and 'fruitful' part of physical theory. In fact, the headstart of physics over the political and social arts and sciences has become the major threat to contemporary civilization. (1954, p.377)

It is not clear whether Koopmans understood that in this instance he was not so much answering Novick as fobbing him off: in effect saying, "let us emulate physics a bit longer and you will see that we will eventually behave just like physicists." What is perhaps more significant is that after almost two decades of experience in empirical economics, Koopmans made no reference to a track record of empirical success in his response. Rather, when reaching for examples of recent triumphs of mathematical inquiry, it was the models of Wald, von Neumann, and Arrow/Debreu which got the nod. As if to pre-empt the obvious retort, he wrote, "The appropriateness of mathematical reasoning in economics is not dependent on how firmly or shakily the premises are established" (p.378). These assertions of *noblesse oblige* were tempered at the end of the article by the peace offering: "Questions of relevance and motivation of any piece of economic analysis are the concern and competence of all economists. It will be good for mathematical economists to be faced with specific challenges of this kind." It appears that many economists took Koopmans at his word, for after this he received a fair amount of correspondence, offprints and broadsides on epistemological questions and the bearing of mathematical expression upon them.[11]

No one of these events would have been all that difficult to bear for the research director of Cowles, but there is evidence that their sum total was growing burdensome. The Chicago period was the era of most emotional strain for Koopmans in his years at Cowles. The pressures, such as they were, could have resulted in a bunker mentality; Cowles could have just raised the drawbridge and withdrawn into the depths of its obscure analytical citadel. But with the move to Yale in 1955 some of the political and logistical pressures were lifted; and in any event, Koopmans was not temperamentally suited to brusque dismissal of opponents. This, we believe, constituted the major motivation for the composition of the last book of Koopmans' career, *Three Essays*. But explaining motivation does not, in this instance, explain content. Koopmans needed a well-developed methodological doctrine to buttress the shift away from econometrics and towards neo-Walrasian mathematical theory at Cowles, as well as to respond to its detractors. It was largely provided by Gerard Debreu.

Bourbakism Comes to Cowles

The history of Gerard Debreu is another object lesson in the vast incalculable importance of the great exodus of European intellectuals to the United States in the 1930s/40s.[12] Debreu was trained to the very highest standards of French mathematics at the Ecole Normale Superièure. To put it in his own words: "Entering the Ecole Normale Superièure in the fall of 1941 meant another initiation, this time into living science. The three years in which I lived and studied [there] were rich in revelations. Nicholas Bourbaki was beginning to publish his *Elements de Mathématique*, and his grandiose plan to reconstruct the entire edifice of mathematics commanded instant and total adhesion. Henri Cartan, who represented him at the Ecole Normale, influenced me them as no other faculty member did... The new levels of abstraction and purity to which the work of Bourbaki was raising mathematics had won a respect that was not to be withdrawn" (in Szenberg, 1992, pp.108-9).

After wavering between astrophysics and economics, the vicissitudes of war and a chance encounter with a book by Maurice Allais tipped him in the direction of economics in 1944. In 1948 he was awarded a Rockefeller Fellowship to visit Universities in the United States and Scandinavia; in retrospect, Debreu claims he found his intellectual home at Cowles in 1949. Koopmans recruited him that fall, and he formally became a member in June 1950. The timing was significant; this was the cusp of the great change at Cowles, from econometric unit to theoretical powerhouse. Debreu was no small part of this transformation.

The role of Debreu in these weighty events has escaped notice until now. Doubtless this is due to Debreu's own reticence in commenting upon anything other than narrowly defined formal results in mathematics, though this has begun to change since his reception of the Nobel Prize in 1983. From the very start, Debreu practiced a kind of mathematical economics not hitherto seen at Cowles, partly because it had previously been largely dominated by mathematical statisticians, but also because none possessed the austere vision of rigor he brought to their proceedings. This is still the way his work is discussed in tones of awe amongst neoclassical economists: you can always tell a Debreu proof, they say. Some of this has to do with the types of mathematics he brings to his work; but mostly it is in the *style*. And that style, as he admits, was pure Bourbaki.

Who was, or is, Bourbaki? Not a person at all, but a pseudonym adopted in the 1930s by a talented group of French mathematicians seeking the reform of what they considered to be the backward state of French mathematics. The founding members of the floating seminar were Jean Díeudonné, Henri Cartan, André Weil and Szolem Mandelbrojt, amongst others. The first of many Bourbaki conferences was held in July 1935; the purpose of which was to assign individual drafts and collectively redraft what was intended to be the definitive rigorous codification of the whole of mathematics. The fear which drove the project was that mathematics was tending to become, "a tower of Babel, in which autonomous disciplines are being more and more widely separated from one another, not only in their aims, but also in their methods and even in their language" (Bourbaki, 1950, p.221). The French horror of the corruption of their language is, of course, notorious, as is their preferred solution: to have a committee of notables reimpose order by fiat. As we have noted, the dangers were rather more insistent than they were when Hilbert attempted to do something similar with his formalist program: Godel's theorems had intervened. The aim of Bourbaki was to present the whole of mathematics in a comprehensive treatise from a superior point of view; and this unification was to be achieved through the building up of each subsequent volume squarely upon its predecessor--*The Theory of Sets, Algebra, General Topology, Functions of a Real Variable* and so forth--and the rock-bottom foundation of this ever-rising edifice was to be the theory of *structures*.[13]

"Structuralism" was a rubric under which a myriad of French intellectual movements spawned and miscegnated in the mid-20th century (Caws, 1988). The importance for historians is that this reveals a cultural propensity to believe in the existence of hidden formal structures in most areas of intellectual endeavor, be it myth or language or mental preferences, and not just amongst the mathematicians. Nevertheless, amongst the Bourbakists it was intended to be meta-empirical: a formal theory of the forms from which all mathematics could be built. This sounds suspiciously like the logicist program which was demolished by Godel; how did these most sophisticated of mathematicians propose to circumvent this looming obstacle?

> Historically speaking, it is of course quite untrue that mathematics is free from contradiction; non-contradiction appears as a goal to be achieved, not a God-given quality that has been granted us once for all... Contradictions do

occur; but they cannot be allowed to subsist if the distinction between true and false, proved and unproved is to keep its meaning. Absence of contradiction, in mathematics as a whole or in any given branch of it, thus appears as an empirical fact, rather than as a metaphysical principle... What will be the working mathematician's attitude when confronted with such dilemmas?... Let the rules be so formulated, the definitions so laid out, that every contradiction may most easily be traced back to its cause, and the latter either removed or so surrounded by warning signs as to prevent serious trouble. This, to the mathematician ought to be sufficient...(Bourbaki, 1949, pp.2-3)

The theory of "structures" was to lend a bit more backbone to what might otherwise first appear a damp squib. These "structures," fabricated from Cartesian products of sets and power sets, were supposed to serve as a generic template for the three "mother-structures" of all mathematics: algebraic-, topological-, and ordered-structures, respectively. However, we need not delve any deeper into the specifics here, because as Leo Corry has so persuasively argued, "*The Theory of Sets* was meant to provide a formally rigorous basis for the whole of the treatise, and the concept of *structure* represented the ultimate stage of this undertaking. The result, however, was different: *Theory of Sets* appears as an ad-hoc piece of mathematics imposed upon Bourbaki by his own declared positions about mathematics"(1992, p.320). In other words, the mathematical entity "structures" is hardly ever used in the rest of the edifice of texts Bourbaki continued to build ever-skyward well into the 1980s. But timing is everything here. The young Bourbaki, flushed with reformist enthusiasm, issued a "Fascicule" in 1939, proclaiming a new standard of rigor of definition and development of mathematics predicated upon the concept of "structure." *The Theory of Sets* only appeared in 1954/1957, at which time it was more apparent that the formal "structure" was not playing its promised starring role, though of course the rhetoric of structure and rigor remained. By the third edition of 1968 there is an explicit acknowledgement that the notion of "structure" was only playing a bit part, a heuristic role, especially as category theory had come to usurp much of its subject matter. By the later 1980s, the entire Bourbakist project was beginning to be regarded more as a quaint period piece than a successful unified presentation of all of mathematics. Indeed, most relevant for our history is that in retrospect it appears, "algebra and topology were probably the branches on which Bourbaki exerted his most profound influence, while logic and most fields of applied mathematics seem not to have been aware of or influenced by Bourbaki at all" (Corry, 1992, p.319). Excepting a not insignificant corner of neoclassical economics, of course.

It is extremely crucial to understand what this all portends, especially since the least-defined primitive in the lexicon of the mathematician is "rigor". Bourbaki did innovate a certain austere style of mathematical proof which caught the fancy of many a mathematician in the postwar era. Their work on axiomatization in topology and set theory still sets a standard of elegance today. The style might be characterized as "take no prisoners": give no didactic or heuristic concessions to the reader, but in turn, hold oneself to the highest standards of criticism *from within the* *recognized mathematics* community. As for the outsiders, the student, the scientist, the seeker of applications; well, they must look elsewhere, for this community does not exist to be in any way beholden to them. Mathematical truths are timeless, located beyond transient petty human concerns, beyond the degradation of debate and dissention, beyond criticism. But this is where the failure of formal "structures" strikes at the heart and soul of the Bourbakist project. Structuralism is about hierarchy: results proven for more general mathematical objects will hold good for their numerous specific manifestations, thus revealing a unity previously obscured by minuet and irrelevant detail. Yet because Bourbaki was unable to carry out this program of formal hierarchy, even in that subset of mathematics which they have justly claimed as their own, all that remains is an *informal* notion of unity, largely maintained by force of *style*: style of notation, style of proof, style of disparaging vernacular, style of no concessions to didactics. "[T]he link between the formal apparatus introduced in the *Theory of Sets* and the activities of the 'working mathematician' which was supposedly Bourbaki's real concern is tenuous and intuitive" (Corry, 1992, p.327).

So the grand project of unification was frustrated; but in its wake there was still that body of fine mathematics, which can stand as its own justification. The Bourbakist style did sweep large segments of the mathematical community up into a similar enthusiasm; but we outsiders should not forget that there persist other styles, other types of mathematics. Henri Cartan, Debreu's teacher, once wrote that, "a general formulation can justify its existence only when it can be applied to several special problems and when it can really be an aid in saving time and thought" (quoted in Corry, 1992, p.330). If we take this seriously, then it should become apparent that it is we, and not the mathematicians, who can be the only judge of the relevance of Bourbakism to our own project. The "ideal type" math/econ representatives of alternative reactions to Godel, von Neumann and Simon, understood this point very well, as did Saunders MacLane, a mathematician critical of Bourbaki, who wrote, "good general theory does not search for the maximum generality, but for the right generality." An endemic problem of the heritage of Bourbakism is that it tends to elevate insularity into a virtue (Kline, 1980). The epitome of this weakness is the fact that, at least in *The Theory of Sets*, "No new theorem is obtained through the structural approach and the standard theorems are treated in the standard ways" (Corry, 1992, p.329). There is a pronounced tendency for Bourbakism to appropriate mathematical results that have been obtained by other "nonrigorous" means and methods, clean up the notation and streamline the proofs, cast them at the level of maximum generality, and leave them mounted in beautiful settings, frozen, sterile, like butterflies in a glass case.

Given that impressions of the role of mathematics in physics loom so large in conventional methodological discussions, it may be useful to register here that many, perhaps most, physicists regarded Bourbakism as a disaster on wheels. One particularly vocal mathematical physicist was Murray Gell-Mann: "It has turned out that the apparent divergence of pure mathematics from science was partly an illusion produced by the obscurantist, ultra-rigorous language used by mathematicians, especially those of a Bourbakian persuasion...When demystified, large chunks of modern mathematics turn out to be connected with physics and other sciences... Pure mathematics and science are finally being reunited and, mercifully the Bourbaki plague is dying out. (In the Soviet Union, they never succumbed to it in the first place.)" (1992, p.7)

Debreu, as we have noted, freely admits Bourbaki as his formative intellectual influence. More to the point, his programmatic statements, however brief and infrequent, are also pure Bourbaki. Take, for instance, the Preface to Cowles Monograph No.17, *The Theory of Value*:

> The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics. The effort towards rigor substitutes correct reasonings and results for incorrect ones, but it offers other rewards too. It usually leads to a deeper understanding of the problems to which it is applied... It may also lead to a radical change in mathematical tools. In the area under discussion it has been essentially a change from the calculus to convexity and topological properties... Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations... Such a dichotomy reveals all the assumptions and the logical structure of the analysis. It also makes possible immediate extensions of the analysis without modification of the theory by simple reinterpretation of concepts...(1959, p.x).

We should inject here, once more, that timing is everything. Recall Debreu the student would just have been present to witness the first flush of structuralist enthusiasm of Bourbaki in Paris; Debreu the Cowles economist would have produced most of this work well before the appearance of *The Theory of Sets*; and, of course, the admission that "structures" were not the grand unifying devices of axiomatic mathematics still lay far in the future. But an economist, then or now, would have no inkling of any of this.

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NOTES

1 See, for instance, (Rosenberg, 1992; McCloskey, 1991; Beed & Kane, 1992; Katzner, 1991; Mirowski, 1991). It should be noted that much of this literature is treated with extreme disdain by mathematical economists, and that in some cases that response can be traced to their perceptions of weaknesses of grounding in the relevant literatures. In particular, the penchant of some modern British economists and others pining for the lost Marshallian orthodoxy to quote Marshall's famous dictum to "burn the mathematics" has provided severe obstacles to understanding the phenomenon.

2 This represents a revision of the analysis presented in (Mirowski, 1986), which did cite Godel's theorems, but did not identify the epistemological rupture in justifications of the use of mathematics in economics. It also is a reaction to the narrative found in (Ingrao & Israel, 1990), which identifies a transformation in the neoclassical program in the 1930s towards a more formalist character.

3 According to Morgenstern's diaries {OMPD, Perkins Library, Duke University} on the second discussion of games with von Neumann on 5 April 1940, Godel's Theorem was mentioned. On Sept. 17, 1944, upon seeing the new book on the theory of games, Godel told Morgenstern that it was very important for science to construct a theory of games. Godel spent a fair amount of time with Morgenstern from 1949-53 attempting to preserve Leibniz's unpublished manuscripts. Morgenstern repeatedly tried himself to grapple with Godel's theorems and their import for economics, writing to himself, for instance in Jan. 1, 1963, that the theorems "showed" that no complete formalization of society was possible {Morgenstern Papers, Box 15}. For the sequence of events leading up to the composition of *The Theory of Games and Economic Behavior*, see (Weintraub, 1992).

4 One reason for the imbalance is that the first case is given greater attention in (Mirowski, 1992). We are not aware of anyone paying close attention to the role of Herbert Simon in the history of mathematical economics as yet, though he is more accessible as the only member of the three to leave us extended personal memoirs (1991). Another reason for provisionally neglecting the first two cases is that their implications are only being played out today in the economics profession. Game theory has only recently usurped the Debreu/Koopmans tradition in graduate training in microeconomics; and there is some reason to argue that game theory in turn is in process of being eclipsed by cognitive science. It is not prudent to write a history of all three programs prematurely, in the heat of current events.

5 The relationship of game theory to Hilbert's conception of mathematics as a "game", plus a survey of the altered image of games in the 1944 volume is provided in (Mirowski, 1991; 1992). An alternative reading of events is found in (Leonard, forthcoming).

6 See, for instance, (Simon, 1992, pp.15, 25). There is a letter written from Simon to Oskar Morgenstern, dated 20 August 1945, in the von Neumann papers {National Archives, Washington, D.C., Von Neumann papers, Box 32} in which Simon rather precociously informs his seniors of many of the flaws of their work, including a large number of perceptive observations about the weaknesses of minimax strategies, the stability of imputations, the incoherence of the notion of a solution in the context of a single play, and the observation, "I got the distinct impression from discussion with [von Neumann] that his preference for the definition you used was based largely on aesthetic and formal grounds. Being a social scientist rather than a mathematician, I am not quite willing for formal theory to lead the facts around by the nose to quite this extent..." What is striking is that many of these criticisms were later broached by others within the game theory literature; though of course much later.

7. Of late, Simon tends to play down the importance of the choice of the premier attempt of the 20th century to ground mathematics in logic as the definition of "intelligence": "That autumn [1955], we had considered three tasks for the program. Our original intention was to start out with chess, and then we also considered doing geometry....So we went to logic, for no deeper reason than that I had the two volumes of *Principia* at home... We were not looking for an efficient way to prove theorems. We were looking at how humans, by selective heuristics, found the right thing to do next." (Simon quoted in Crevier, 1993, p.44)

8. "Koopmans was seriously opposed to fine writing in economics, not a common crime in our field. According to his code of scientific honor, mere elegance must not give ideas an

unfair boost" (Samuelson, 1988, p.323). Although we shall not do so here, the same argument could be made for Gerard Debreu, contrary to many impressions of his work. In (Weintraub, forthcoming) he reveals a dissatisfaction with the inwardness and aestheticism of the Bourbakist program, and suggests he moved into economics in order to find a more concrete subject matter upon which to deploy his talents.

9 Koopmans papers, Sterling Library, Yale University; letter dated March 2, 1955. For those interested in methodology and Milton Friedman, there is another passage worth quoting: "You are entirely right in supposing that Lionel Robbins and myself take diametrically opposite positions on methodology. What this seems to me to suggest is a proposition that has long seemed very plausible, namely, that there is little relation between people's explicit methodological comments and their actual procedure in their own work."

10 "Application to the Ford foundation by the Cowles Commission for research in Economics, Submitted Sept.17, 1951", Cowles Archives, Yale University; pp.4-5.

11 See, for instance, Box 23, Koopmans papers, Sterling Library: letter from J.M. Clark to Koopmans, dated Jan. 22, 1956; Precis of talk given by Kenneth Boulding entitled "The Limitations of Mathematics: An Epistemological Critique" dated 15 December, 1955.

12 Much of this section is based upon the pathbreaking work of Roy Weintraub on the history of mathematical formalization in economics, especially (Weintraub, forthcoming). Biographical data on Debreu is also available in (Szenberg, 1992, pp.107-114).

13 The history of "structures" in Bourbaki is covered in a fascinating article by Corry (1992), which is indispensable for the understanding of the history of mathematics at the Cowles Commission, and is the major source of the remainder of this section.

REFERENCES

Allais, Maurice. 1954. "L'Utilisation de l'Outil Mathematique" *Econometrica*, (22):58-71.

Aspray, William. 1990. John von Neumann and the Origins of Modern Computing. Cambridge: MIT Press.

Aspray, William & Burks, Arthur, eds. 1987. Papers of John von Neumann on Computing and Computer Theory. Cambridge: MIT Press.

Aspray, William & Kitcher, Philip. 1988. History and Philosophy of Modern Mathematics. Minneapolis: University of Minnesota Press.

Baumol, W. & Goldfeld, S. eds. 1968. Precursors of Mathematical Economics.

Beed, C. & Kane, O. 1992. "What Is the Critique of the Mathematization of Economics?" *Kyklos*, (44):581-611.

Bourbaki, Nicholas. 1949. "The Foundations of Mathematics for the working Mathematician," *Journal of Symbolic Logic*, (14):1-8.

Blaug, Mark. 1992. The Methodology of Economics. 2nd ed. Cambridge: Cambridge University Press.

Caws, Peter. 1988. Structuralism. New Jersey: Humanities Press.

Clark, J.M. 1947. "Mathematical Economists and Others," *Econometrica*, (15)

Corry, Leo. 1989. "Linearity and Reflexivity in the Growth of Mathematical Knowledge," *Science in Context*, (3):409-440.

Corry, Leo. 1992. "Nicholas Bourbaki and the Concept of Mathematical Structure," *Syntheses*, (92):315-48.

Crevier, Daniel. 1993. AI: The History of the Search for Artificial Intelligence. New York: Basic.

Davis, P. & Hersh, R. 1981. The Mathematical Experience. New York:

Debreu, Gerard. 1959. The Theory of Value. New Haven: Yale University Press.

Debreu, Gerard. 1984. "Economic Theory in the Mathematical Mode" *American Economic Review*, (74):267-278.

Debreu, Gerard. 1986. "Theoretical Models: Mathematical Form and Economic Content," *Econometrica*, (54):1259-1270.

Debreu, Gerard. 1991. "The Mathematization of Economic Theory," *American Economic Review*, (81):1-7.

Dresden, Martin. 1987. H.A. Kramers. New York: Springer.

Dreyfus, Hubert & Dreyfus, Stuart. 1986. Mind over Machine. New York: Free Press.

Gell-Mann, Murray. 1992. "Nature Conformable to Herself," *Bulletin of the Santa Fe Institute*, (7):7-10.

Heims, Steve. 1991. The Cybernetics Group. Cambridge: MIT Press.

Ingrao, B. & Israel, G. 1990. The Invisible Hand. Cambridge: MIT Press.

Katzner, Donald. 1991. "In Defense of Formalization in Economics" *Methodus*, (3):17-24.

Klamer, Arjo. 1990. "The Textbook Presentation of Economic Discourse," in Warren Samuels, ed., Economics as Discourse. Boston: Kluwer.

Klamer, Arjo & Colander, David. 1990. The Making of an Economist. Boulder: Westview.

Kline, Morris. 1980. Mathematics: The Loss of Certainty. New York: Oxford University Press.

Koopmans, Tjalling. 1954. "On the Use of Mathematics in Economics" *Review of Economics and Statistics*, (36):377-80.

Koopmans, Tjalling. 1985. Scientific Papers of Tjalling C. Koopmans. vol.II. Cambridge: MIT Press.

Koopmans, Tjalling. 1991 [1957]. Three Essays on the State of Economic Science. Fairfield, NJ: Augustus Kelley.

Leonard, Robert. 1993. "In the Spirit of Exact Science," unpublished working paper, Universitè du Quèbec.

McCloskey, Donald. 1991. "Economic Science: A Search Through the Hyperspace of Assumptions?" *Methodus*, (3):6-16.

Malinvaud, Edmond. 1972. "The Scientific Papers of Tjalling C. Koopmans," *Journal of Economic Literature*, (10):798-802.

Mirowski, Philip. 1986. The Reconstruction of Economic Theory. Boston: Kluwer.

Mirowski, Philip. 1989. More Heat than Light. New York: Cambridge University Press.

Mirowski, Philip. 1991a. "When Games Grow Deadly Serious," C. Goodwin, ed., Economics and National Security. Durham: Duke University Press.

Mirowski, Philip. 1991b. "The When, the How, and the Why of Mathematical Expression in the History of Economic Analysis," *Journal of Economic Perspectives*, (5):145-157.

Mirowski, Philip. 1992. "What Were von Neumann and Morgenstern Trying to Accomplish?" in (Weintraub, 1992).

Mirowski, Philip. 1994. "What Are the Questions?" in Roger Backhouse, ed., Handbook of Economic Methodology. London: Routledge.

Nagel, E. & Newman, J. 1958. Godel's Proof. New York: New York University Press.

Niehans, Jurg. 1990. A History of Economic Theory. Baltimore: Johns Hopkins University Press.

Novick, David. 1954. "Mathematics: Logic, Quantity and Method," *Review of Economics and Statistics*, (36):357-8.

Rosenberg, Alexander. 1992. Economics--Mathematical Politics or Science of diminishing Returns? Chicago: University of Chicago Press.

Samuelson, Paul. 1965. Foundations of Economic Analysis. New York: Atheneum.

Samuelson, Paul. 1988. "The Passing of the Guard in Economics," *Eastern Economic Journal*, (14):319-326.

Shankar, S. ed. 1989. Godel's Theorem in Focus. London: Routledge.

Simon, Herbert. 1991. Models of my Life. New York: Basic.

Simon, Herbert. 1992. Economics, Bounded Rationality, and the Cognitive Revolution. ed. M. Egidi & R. Marris. Aldershot: Edward Elgar.

Stigler, George. 1950. Five Lectures on Economic Problems.

Szenberg, Michael, ed. 1992. Eminent Economists. New York: Cambridge University Press.

Tinbergen, Jan. 1954. "The Functions of Mathematical Treatment" *Review of Economics and Statistics*, (36):365-72.

von Neumann, John. 1961-63. Collected Works. 6 vols. New York: Pergamon.

Walter, Maila. 1990. Science as Cultural Crisis. Stanford: Stanford University Press.

Wang, Hao. 1987. Reflections on Kurt Godel. Cambridge: MIT Press.

Weintraub, Roy. 1985. General Equilibrium Theory. New York: Cambridge University Press.

Weintraub, Roy, ed. 1992. Toward a History of Game Theory. Durham: Duke University Press.

Weintraub, Roy. Forthcoming. "Formalist Mathematics and Economic Theory" unpublished working paper, Duke University.