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The Pure and the Applied: Bourbakism Comes to Mathematical Economics

The Argument

In the minds of many, the Bourbakist trend in mathematics was characterized by pursuit of rigor to the detriment of concern for applications or didactic concessions to the nonmathematician, which would seem to render the concept of a Bourbakist incursion into a field of applied mathematics an oxymoron. We argue that such a conjuncture did in fact happen in postwar mathematical economics, and describe the career of Gérard Debreu to illustrate how it happened. Using the work of Leo Corry on the fate of the Bourbakist program in mathematics, we demonstrate that many of the same problems of the search for a formal *structure* with which to ground mathematical practice also happened in the case of Debreu. We view this case study as an alternative exemplar to conventional discussions concerning the "unreasonable effectiveness" of mathematics in science.

> The fact is that I am unique. I am not interested in what one man may transmit to other men; like the philosopher, I think that nothing is communicable by the art of writing. Bothersome and trivial details have no place in my spirit, which is prepared for all that is vast and grand; I have never retained the difference between one letter and another.
>
> Jorge Luis Borges, "The House of Asterion"

> Before the contemporary period of the last five decades, theoretical physics had been an inaccessible ideal towards which economic theory sometimes strove. During that period, this striving became a powerful stimulus in the mathematicization of economic theory. The great theories of physics cover an immense range of phenomena with supreme economy of expression. . . . This extreme conciseness is made possible by the privileged relationship that developed over several centuries between physics and mathematics. . . . The benefits of that special relationship were large for both fields; but physics did not completely surrender to the embrace of mathematics and to its inherent compulsion towards logical rigor. . . . In these directions, economic theory could not follow the role model offered by physical theory. . . . Being denied a sufficiently secure experimental base, economic theory has to adhere to the rules of logical discourse and must renounce the facility of internal inconsistency.
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> Gérard Debreu, "The Mathematization of Economic Theory"

1. Purity and Danger

Constructing the history of a modern area of "applied mathematics" such as twentieth-century mathematical economics would appear doomed from the start. The project is fettered first by Sisyphean suspicions about writing a history of something whose mode of expression is thought to have no history, since mathematics represents for many the epitome of timeless truth. Further, mathematicians themselves harbor an implicit hierarchy of the "pure" over the "applied" in their fields of endeavor, rendering the latter almost invisible in the narratives of their heroes and villains. And then there is the problem that the history of mathematics persists as the most stubbornly internalist subfield of the history of science, defending its purity against realist and relativist alike. As if all this were not bad enough, existing histories of economics either manage to neglect the mathematical component altogether or else treat it as an unproblematic foreshadowing of modern orthodox economics, devoid of any relationship to the history of mathematics — or indeed anything else.

The first author of this paper has argued persistently over the last decade that the history of modern economics must take into account the history of mathematics (Weintraub 1985, 1991, 1992). The second author has endeavored to place the history of modern economic theory in a frame-tale of the history of such sciences as physics, psychology, and cognitive science (Mirowski 1989, 1993, 1994). Both of our concerns converge upon the construction of the postwar American hegemony in mathematical economic theory in the period of the 1930s to 1950s, a history that remains largely unwritten. Such a history could not exist in a vacuum since both in that period and in the present, one of the greatest bones of contention within economics has been the impact and significance of the substantial racheting upward of standards of mathematical sophistication within the profession (Beed and Kane 1992; Klamer and Colander 1990; Debreu 1991). Nevertheless, these methodological disputes have been prosecuted in a profoundly ahistorical and internalist fashion, doubtless due to their inflammatory nature. Hence the factors militating against a more satisfactory historical narrative appear so daunting as to preclude any audience for such an exercise.

In this paper we propose to try to break the semiotic impasse by recasting both the question and the audience. Instead of attempting to mollify any of the above constituencies, this work is directed to a generalist audience of science studies scholars who may be interested in a case study of how one distinctive mode of mathematics could make inroads into a seemingly distant field and subsequently transform that field's self-image, as well as its very conception of inquiry. To be more precise, we shall present a narrative of how the Bourbakist school of mathematics rapidly migrated into neoclassical mathematical economics. Crossing this disciplinary boundary established, for economists, the imposing edifice of Walrasian general equilibrium theory, the landmark of high theory in economics for the next four decades.[1] As it happens, our narrative is rendered tractable by the

[1] "The edifice of General Equilibrium Theory has been compared to the great gothic cathedrals.... If Walras and Pareto are generally credited with being the architects of the General Equilibrium Theory, it becomes apparent from the reprinted contributions in this volume that Debreu is the great master builder of that edifice" (Hildenbrand 1983b, 29). The Bourbakist influence on economics was first noticed in Ingrao and Israel 1990, chap. 9; it was explicitly acknowledged in Debreu 1992a.

fact that the story can be told primarily by means of a single metonymy, the intellectual biography of an outstanding individual actor — Nobel prize winner Gérard Debreu.

Why should the story of the activities of one economist bear any significance for the science studies community? We believe that the answer lies in the way it illustrates the intersection of technical, philosophical and historical concerns when it comes to telling what happens when the sublimity of pure mathematics (the "music of reason," as Dieudonné has called it) meets the impurity of scientific discourse — a confrontation that can only be postponed, never altogether prevented. Too often these problems are treated merely as matters for metamathematics, or perhaps the odd speculation about reasons for the "unreasonable effectiveness" of mathematics in the physical sciences. But as any reader of Mary Douglas can attest, reflection on the impure involves reflection on the relationship of order to disorder. It may even be that there is a hidden mathematical metaphor in her own insistence that "rituals of purity and impurity create unity in experience. . . . By their means, symbolic patterns are worked out and publicly displayed. Within these patterns disparate elements are related and disparate experience is given meaning" (Douglas 1989, 2–3).

For our purposes, the school of Bourbaki will serve to represent the champions of purity within the house of twentieth-century mathematics. While Bourbaki is hardly a household word among historians, many mathematicians would more or less acquiesce in our attribution:

> For a few decades, in the late thirties, forties and early fifties, the predominant view in American mathematical circles was the same as Bourbaki's: mathematics is an autonomous abstract subject, with no need of any input from the real world, with its own criteria of depth and beauty, and with an internal compass for guiding future growth. . . . Most of the creators of modern mathematics — certainly Gauss, Riemann, Poincaré, Hilbert, Hadamard, Birkhoff, Weyl, Wiener, von Neumann — would have regarded this view as utterly wrongheaded. (Lax 1989, 455–56)

And again,

> The twentieth century has been, until recently, an era of "modern mathematics" in a sense quite parallel to "modern art" or "modern architecture" or "modern music." That is to say, it turned to an analysis of abstraction, it glorified purity and tried to simplify its results until the roots of each idea were manifest. These trends started in the work of Hilbert in Germany, were greatly extended in France by the secret mathematical club known as "Bourbaki," and found fertile soil in Texas, in the topological school of R. L. Moore. Eventually, they conquered essentially the entire world of mathematics, even trying to breach the walls of high school in the disastrous episode of the "new math." (Mumford 1991)

Thus Bourbaki came to uphold the primacy of the pure over the applied, the rigorous over the intuitive, the essential over the frivolous, the fundamental over what one member of Bourbaki called "axiomatic trash." They also came to define the disciplinary isolation of the mathematics department in postwar America. It is this reputation for purity and isolation that has drawn the wrath of many natural scientists in the last few years. For instance, the physicist Murray Gell-Mann has written: "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 Bourbakist persuasion, and by their reluctance to write up non-trivial examples in explicit detail. . . . Pure mathematics and science are finally being reunited and, mercifully, the Bourbaki plague is dying out" (Gell-Mann 1992, 7). Or one might cite the case of Benoit Mandelbrot, all the more poignant because of his blood relationship to a member of Bourbaki:

> The study of chaos and fractals . . . ought to provoke a discussion of the profound differences that exist . . . between the "top down" approach to knowledge and the various "bottom up" or self-organizing approaches. The former tend to be built around one key principle or *structure,* that is, around a tool. And they rightly feel free to modify, narrow down, and clean up their own scope by excluding everything that fails to fit. The latter tend to organize themselves around a class of *problems*. . . . The top down approach becomes typical of most parts of mathematics, after they have become mature and fully self-referential, and it finds its over-fulfillment and destructive caricature in Bourbaki. . . . The serious issues were *intellectual strategy,* in mathematics and beyond, and *raw political power.* An obvious manifestation of intellectual strategy concerns "taste." For Bourbaki, the fields to encourage were few in number, and the fields to discourage or suppress were many. They went so far as to exclude (in fact, though perhaps not in law) most of hard classical analysis. Also unworthy was most of sloppy science, including nearly everything of future relevance to chaos and to fractals. (Mandelbrot 1989, 10–11)

For many scientists, Bourbaki became the watchword for the chasm that had opened up between mathematics and its applications, between "rigor" and its alternative homeostat, the dictates of the concrete problem situation (Israel 1981). In such a world, would it not appear that a Bourbakist-inspired discipline of "applied mathematics" would constitute an oxymoron?[2] Could such a phenomenon be much more than a simple contradiction in terms? It is our thesis that such a thing did occur in economics and, indeed, that it took root and flourished in the postwar American environment. The transoceanic gemmule was Gérard Debreu;

[2] As Corry has written: "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 1992a, 319). This is one place where we differ with a paper that has otherwise inspired our own work.

the seedbed for economics was the Cowles Commission (Christ 1952, 1994) at the University of Chicago. While the natural history of mathematical economics will require a certain amount of detailed spadework, the resulting narrative may prove of wider interest to the science studies community, in that it may demonstrate just how the pure and the impure were constantly intermingled in mathematical practice, suggest some of the attractions and dangers that fertilized the transplant, and perhaps also open up the hothouse of mathematics to a historiographic search for the influence of Bourbaki and other such versions of "images of mathematics" (Corry 1989) on the whole range of the sciences in the twentieth century.

2. Pure Structures for an Impure World

Who or what was "Bourbaki" such that they could so utterly transform the staid world of mathematics? While primary materials are sparse and no comprehensive history in English exists, we shall base our brief narrative on the published texts by Bourbaki, some statements about Bourbaki by former members (Dieudonné 1970, 1982b; Cartan 1980; Guedj 1985; Adler 1988) and the important papers by Corry (1992a, 1992b). Our intention is primarily to set the stage for the appearance of our protagonist, Gérard Debreu, and not to provide anything like a comprehensive overview of the Bourbaki phenomenon.

In 1934–35, Claude Chevalley and André Weil decided to try to reintroduce rigor into the teaching of calculus by rewriting one of the classic French treatises. As Chevalley recalled matters, "The project, at that time, was extremely naive: the basis for teaching the differential calculus was Goursat's *Traité,* very insufficient on a number of points. The idea was to write another to replace it. This, we thought, would be a matter of one or two years" (Guedj 1985, 19). The project (which continues to this day) was adopted as the work of the original group of seven: Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné, Szolem Mandelbrojt, René de Possel, and André Weil; in the Bourbaki nomenclature they are called the "founders." Continuing an elaborate joke that had been played, over time, at the Ecole Normale, they gave themselves the name of an obscure nineteenth-century French general, Nicolas Bourbaki, and agreed to operate as a secret club or society. At the beginning, they agreed that the model for the book they wished to do was B. L. Van der Waerden's *Algebra,* which had appeared in German in 1930:

> So we intended to do something of this kind. Now Van der Waerden uses very precise language and has an extremely tight organization of the development of ideas and of the different parts of the work as a whole. As this seemed to us to be the best way of setting out the book, we had to draft many things which had never before been dealt with in detail. (Dieudonné 1970, 137)

The difficulty was that this project was an immense one. Thus "we quickly realized that we had rushed into an enterprise which was considerably more vast than we had imagined" (ibid.). The work was done in occasional meetings in Paris but mostly in "congresses" — the longest of which took place in the French countryside each summer. The rules of Bourbaki quickly became established, both the formal and informal ones. Of the formal rules, there was only one, which was that no member of the group should be over age fifty, and that on reaching that age, a member would give up his place. Nevertheless, certain behaviors became conventional. There came to be two meetings a year in addition to the longer congress. The work was done by individuals agreeing to submit drafts of chapters to the group for public reading and for tearing apart. If the result was not accepted, and acceptance required unanimity, the draft was given to someone else to be redone and resubmitted at a subsequent congress. Up to two visitors might attend the congresses, provided they participated fully; this was sometimes a way to see if a person might be thought of as a potential new Bourbaki.

> There never was an example of a first draft being accepted. The decisions did not take place in a block. In the Bourbaki congresses one read the drafts. At each line there were suggestions, proposals for change written on a blackboard. In this way a new version was not born out of a simple rejection of a text, but rather it emerged from a series of sufficiently important improvements that were proposed collectively. (Guedj 1985, 20)

The question of what kind of book they were to write quickly came to the forefront of their discussions. What distinguishes the Bourbaki project is the result of the Bourbaki decision to create a "basic" book for mathematicians.

> The idea which soon became dominant was that the work had to be primarily a *tool.* It had to be something usable not only in a small part of mathematics, but also in the greatest possible number of mathematical places. So if you like, it had to concentrate on the basic mathematical ideas and essential research. It had to reject completely anything secondary that had immediately known application [in mathematics] and that did not lead directly to conceptions of known and proved importance. . . . So how do we choose these fundamental theorems? Well, this is where the new idea came in: that of *mathematical structure.* I do not say it was a new idea of Bourbaki — there is no question of Bourbaki containing anything original. . . . Since Hilbert and Dedekind, we have known very well that large parts of mathematics can develop logically and fruitfully from a small number of well-chosen axioms. That is to say, given the bases of a theory in an axiomatic form, we can develop the whole theory in a more comprehensible form than we could otherwise. This is what gave the general idea of mathematical structure. . . . Once this idea had been clarified, we had to decide which were the most important mathematical structures. (Dieudonné 1970, 138, 107)

By 1939 the first book appeared, *Eléments de Mathématique I (Fascicule de résultats).* This book was the first part of the first volume, that of set theory. It presented the plan of the work and outlined the connections between the various major parts of mathematics in a functional way, or what Bourbaki called a structural manner. It contained

> without any proof all notations and formulas in set theory to be used in subsequent volumes. Now when each new volume appears, it takes its logical position in the whole of the work. . . . Bourbaki often places an historical report at the end of a chapter. . . . There are never any historical references in the text itself, for Bourbaki never allowed the slightest deviation from the logical organization of the work itself. (Cartan [1958] 1980, 8)

Thus instead of the division into algebra, analysis, and geometry, the fundamental subjects, from which the others could be derived, were to be set theory, general algebra, general topology, classical analysis, topological vector spaces, and integration. This organization shows up in the volumes themselves, because the first six books, each comprising several chapters with numerous exercises, correspond to these six divisions. The twenty-one volumes published by the late 1950s all belong to Part I, "The Fundamental Structures of Analysis."

"An average of 8–12 years is necessary from the first moment we set to work on a chapter to the moment it appears in a bookshop" (Dieudonné 1970, 142, 110). The length of time seems to be a result of both the unanimity rule for the congresses and the complexity of the task itself.

> What was envisioned was a repertory of the most useful definitions and theorems (with complete proofs . . .) which research mathematicians might need . . . presented with a generality suited to the widest possible range of applications. . . . In other words, Bourbaki's treatise was planned as a bag of tools, a *tool kit,* for the working mathematician." (Dieudonné 1982b, 618)

This viewpoint led to the fundamental organizing idea of the work: "It was our purpose to produce the general theory first before passing to applications, according to the principle we had adopted of going 'from the most general (*généralissime*) to the particular'" (Guedj 1985, 20). Chevalley recalled the views of the "founders" of the project as to its import thus:

> It seemed very clear that no one was obliged to read Bourbaki . . . a bible in mathematics is not like a bible in other subjects. It's a very well arranged cemetery with a beautiful array of tombstones. . . . There was something which oppressed us all: everything we wrote would be useless for teaching. (Guedj 1985, 20)

It was to be through the Séminaire Bourbaki that the French mathematicians, after the war, reconnected to the world mathematical community. The project of the *Eléments* gained momentum, and the invitations to come to lecture in Paris were appreciated. The immense strength of the French mathematicians in a number of important areas made the project increasingly noted among mathematicians in the United States. The international nature of the mathematical community and the prewar connections of the few older men, André Weil particularly, facilitated recognition of the work. The mystery of Bourbaki, and the ambition of the project, probably attracted attention as well.

Bourbaki had the major problem, in writing the *Eléments,* of organization, of relating the various parts of mathematics one to another. This "problem" was approached through the notion of "mathematical structure" — of which more anon. The second issue Bourbaki had to face was that of the approach to be taken within each section of the whole, and that was handled by the rule "from the general to the specific." Thus as the books and chapters emerged from the publisher, and the immense project took shape in print over the decades, mathematics was presented as self-contained, in the sense that it grew out of itself — from the basic structures to those more derivative, from the "mother-structures" to those of the specific areas of mathematics. For example:

> In the logical order of the Bourbaki system, real numbers could not appear at the beginning of the work. They appear instead in the fourth chapter of the third book. And with good reason, for underlying the theory of real numbers is the simultaneous interaction of three types of structures. Since Bourbaki's method of deducing special cases from the most general one, the construction of real numbers from the rationals is for him a special case of a more general construction: the completion of a topological group (Chapter 3 in Book III.) And this completion is itself based on the theory of the completion of a "uniform" space (Chapter 2 in Book III). (Cartan [1958] 1980, 178)

What these organizing principles accomplished, in making the work itself coherent, cannot be underestimated. The choices Bourbaki made were reasonable ones for the immense task of writing a handbook of mathematics for working mathematicians. The imposition of this order, and coherence, led to a book with the elegance and grace of a masterwork, a modern version of Euclid's *Elements.* But the ideas of structure and the book's motion from the general to the specific had major consequences.

The word "structure," whether in French or in English, can mean many things to many people. The immediate temptation is to associate it with the erstwhile French philosophical and cultural movement known as "structuralism" (Caws 1988); and there is some justification for this inclination, such as the connections between André Weil and one of the gurus of the movement, Claude Lévi-Strauss. Indeed, the title of one of Bourbaki's very few explicitly methodological pronouncements was "The Architecture of Mathematics," published in French in 1948, anticipating the title and some of the content of Michel Foucault's own *L'archéologie du savoir* by two decades (Gutting 1989). Others insist on a more narrowly defined philosophical context of a specifically "mathematical structuralism" (Chihara 1990, chap. 7). We regretfully by-pass such tantalizing historical issues and opt to concentrate more narrowly on Bourbaki's own account of the meaning of structure, and the clarification of these issues provided by Corry (1992a).

The question that motivated Bourbaki was, "Do we have today a mathematics or do we have several mathematics?" ([1948] 1950, 221). Fear of disorder, or "dirt," as Mary Douglas would put it, was the order of the day, with Bourbaki wondering "whether the domain of mathematics is not becoming a Tower of Babel?" (Ibid.). Bourbaki would not want to pose this question to those usual underlaborers and binmen of knowledge, the philosophers, but rather to an ideal type, which they identified as the "working mathematician." This *homme moyen* was purportedly defined by his recourse to "mathematical formalism": "The method of reasoning by laying down chains of syllogisms. . . . To lay down the rules of this language, to set up its vocabulary and to clarify its syntax" (ibid., 223). Bourbaki goes on to state, however, that this is

> but one aspect of the axiomatic method . . . [which] sets as its essential aim . . . the profound intelligibility of mathematics. . . . Where the superficial observer sees only two, or several, quite distinct theories . . . the axiomatic method teaches us to look for the deep-lying reasons for such a discovery, to find the common ideas of these theories, buried under the accumulation of details properly belonging to each of them, to bring these ideas forward and to put them in the proper light. (Ibid.)

He then proceeds to suggest that the starting point of the axiomatic method is a concern with "structures" and *develops this idea of structure through examples.* According to the informal definition, "structure" is a

> generic name . . . [which] can be applied to sets of elements whose nature* has not been specified; to define a structure, one takes as given one or several relations, into which these elements enter# in the case of groups, this was the relation $z = xty$ between the three arbitrary elements); then one postulates that the given relation, or relations, satisfy certain conditions (which are explicitly stated and which are the axioms of the structure under consideration). To set up the axiomatic theory of a given structure, amounts to the deduction of the logical consequences of the axioms of the structure, excluding every other hypothesis on the elements under consideration (in particular, every hypothesis as to their own nature). (Ibid., 225–26)

This remarkable passage is in fact the linchpin of the enterprise, for it contains in it and outside of it, by what it excludes, Bourbaki mathematics.

First, note the footnote attached to "nature" (*). Bourbaki comments that philosophical concerns are to be avoided here, in the debates on formalist, idealist, intuitionist foundations. Instead, "From this new point of view mathematical structures become, properly speaking, the only 'objects' of mathematics" (ibid., 225–226, note). That is, mathematics is concerned with mathematical objects, called structures if you will, and the job of mathematicians is to do mathematics attending to these structures. Bourbaki goes on to say, in the next footnote (#), linked to the word "enter," that "this definition of structures is not sufficiently general for the needs of mathematics" because of a need to consider higher-order structures or, in effect, structures whose elements are structures. The Gödel incompleteness issues are left to one side; for mathematicians simply do mathematics, and when an inconsistency arises, the rule is to face it and do mathematics around it almost in an empirical sense.

> What all this means is that mathematics has less than ever been reduced to a purely mechanical game of isolated formulas; more than ever does intuition dominate in the genius of discoveries. But henceforth, it possesses the powerful tools furnished by the great types of structures; in a single view, it sweeps over immense domains, now unified by the axiomatic method, but which formerly were in a completely chaotic state. (Ibid., 228)

In his 1949 paper Bourbaki lays out his actual program for foundations in the post-Gödel world of logic:

> What will be the working mathematician's attitude when confronted with such [Gödel] dilemmas? It need not, I believe, be other than strictly empirical. We cannot hope to prove that every definition, every symbol, every abbreviation that we introduce is free of potential ambiguities, that it does not bring about the possibility of a contradiction that might not otherwise have been present. Let the rules be so formulated, the definitions so laid out, that every contradiction may be most easily traced back to its cause, and the latter either removed or so surrounded by warning signs as to prevent trouble. This, to the mathematician, ought to be sufficient; and it is with this comparatively modest and limited objective in view that I have sought to lay the foundations for my mathematical treatise." (Bourbaki 1949, 3)

What we have here is an "admission" as it were that there is no more security to be found in the magisterial idea of "structure" than there was in the idea of "set" or "number" as the bedrock on which a secure mathematics could be built. Nonetheless, Bourbaki lays out in this paper the "sign language" of objects, signs, relations, etc., to end up with a language in which he can proceed to do mathematics. That this is not necessarily consistent is of no concern to the working mathematician, for it suffices to do the Bourbaki mathematics. We have then a disjunction between Corry's italicized *structure* and "structure."

Leo Corry (1989) suggested that mathematics should be set apart from other sciences because it persistently strives to apply the tools and criteria of its actual practices to itself in a meta-analytic manner, thus masking the distinction between the "body of knowledge," which is characteristic of a particular historical juncture, and the "images of knowledge," which are deployed in order to organize and motivate inquiry. For Corry, it is the images of knowledge rather than the actual corpus of proofs and refutations that gets overthrown and transformed whenever mathematical schools and fashions change over the course of history.[3] Corry's premier illustration of this thesis is his (1992a) description of Bourbaki's variant meanings of "structure" and *structure.*

In the 1939 *Fascicule,* hereafter cited as the *Theory of Sets,* the Bourbaki proposed to lay out in Chapter IV the foundations — the formally rigorous basis of their entire enterprise. This collection of formalisms, which Corry designates as the italicized word *structure,* involved base sets and an echelon construction scheme that was intended to generate *mother-structures,* which in turn would generate the rest of mathematics as Bourbaki saw it. Yet there was a disjuncture between this chapter and the rest of the book, as well as with all the other volumes of the Bourbaki corpus.

> Bourbaki's purported aim in introducing such concepts is expanding the conceptual apparatus upon which the unified development of mathematical theories would rest later on. However all this work turns out to be rather redundant since . . . these concepts are used in a very limited — and certainly not highly illuminating or unifying — fashion in the remainder of the treatise. (Corry 1992a, 324).

It seems the concept *structure* has no palpable mathematical use in the rest of Bourbaki's work, and the links between the formal apparatus and the working mathematician are largely absent. "No new theorem is obtained through the *structural* approach and standard theorems are treated in the standard ways" (ibid., 329). Yet, as we have already witnessed, the ideal of "structure" and the achievement of Bourbaki have remained identified in the minds of those who came after. How can this be?

Corry responds that this has to do with the difference between the actual body of results and the image of knowledge. "If the book's stated aim was to show that we can formally establish a sound basis for mathematics, the fascicule's purpose is to inform us of the lexicon we will use in what follows and of the *informal* meaning of the terms within it. The sudden change in approach, from a strictly formal to a completely informal style, is clearly admitted" (ibid., 326). This is the practical meaning of Corry's unitalicized "structure": Bourbaki's primary contribution had to do with the way mathematicians interpreted their mathematical work, and not the formal foundations of that work itself. It was, if you will, a matter of style, of

[3] The possible nature and content of "revolutionary episodes" in mathematics are discussed by various authors in Gilles 1992. One of the present authors is rather more taken with the interpretation of the knowledge/metaknowledge dichotomy as treated by Herbert Mehrtens in that volume: "Mathematicians turned formalization with their artificial sign-language into the centre of their productive work, then internal 'interpretations' became mathematical models or simply new theories, themselves part of formalization. Interpretation became a matter for other cultural fields like literature and history" (ibid., 47). The question then becomes the possible constitution of alternative, yet equally legitimate, "truths."

taste, of shared opinions about what was valuable in mathematics, of all those things that should not really matter to the Platonist or the formalist or the intuitionist.[4] If mathematics be the music of reason, then Bourbaki ended up being its Sol Hurok or Brian Epstein, and not its Pierre Boulez or Pink Floyd. (What applies to the collective need not apply to the individual members, however.) Or as Corry put it, "Bourbaki's style is usually described as one of uncompromising rigor with no heuristic or didactic concessions to the reader. . . . [But in the *Theory of Sets*] the formal language that was introduced step by step in Chapter 1 is almost abandoned and quickly replaced by the natural language" (ibid., 321).

The final legacy of Bourbaki is most curious. As Corry summarized in 1992b, (p. 15): "Bourbaki did not adopt formalism with full philosophical commitment, but rather as a façade to avoid philosophical difficulties." Others now concur in this assessment (see Mathias 1992). Bourbaki gave the impression of elevating their choices in mathematics above all dispute: but that was all it was — just an impression. "It is [now] clear that the early developments of the categorical formation, more flexible and effective than the one provided by *structures*, rendered questionable Bourbaki's initial hopes of finding *the* single best foundation for each mathematical idea and cast doubt on the initially intended universality of Bourbaki's enterprise. . . . [As Saunders Mac Lane wrote] good general theory does not search for the maximum generality, but for the right generality" (Corry 1992a, 336). But this realization took time, happening possibly as late as the 1970s; and in the interim, the Bourbaki juggernaut kept churning out further volumes. The timing of these events is of some significance for our subsequent narrative.

These details concerning Bourbaki's history and Corry's reading of it, seemingly so far removed from economics, are instead absolutely central to understanding its postwar evolution. The reason is that very nearly everything said about Bourbaki applies with equal force to Gérard Debreu.

3. Gérard Debreu and the Making of a Pure Economics

When the place of mathematics in economics is broached, it is Debreu who is always mentioned with awe, and not a little apprehension. "Debreu is known for his unpretentious no-nonsense approach to the subject," writes Samuelson (1983, 988). "Debreu's contributions might appear, at first glance, incomprehensibly 'abstract.'. . . In this respect Debreu has never compromised, just as he has never followed fashions in economic research," writes his memorialist Werner Hilden-

[4] This, as we shall see, is the primary reason why the interpretations of Bourbaki as the culmination of Hilbert's formalist program found in Punzo 1991 and Ingrao and Israel 1990 are misleading and further complicate the understanding of the role of axiomatization in modern mathematical economics. The attitude of others such as John von Neumann or Herbert Simon toward axiomatization in mathematical economics was entirely different and led to a different sort of mathematical economics program.

brand. "Debreu presents his scientific contributions in the most honest way possible by explicitly stating all underlying assumptions and refraining at any stage of the analysis from flowery interpretations that might divert attention from the restrictiveness of the assumptions and lead the reader to draw false conclusions" (Hildenbrand 1983b, 2–3). When George Feiwel tried to conduct an oral history, he was reduced to prefacing many of his questions with the clause, "For the benefit of the uneducated. . . ." In response to the question, "Why is the question of existence of general economic equilibrium so profoundly important?" Debreu shot back, "Since I have not seen your question discussed in the terms I would like to use, I will not give you a concise answer" (Feiwel 1987, 243). However, when one of the present authors interviewed him in 1992, he was gracious and forthcoming in answering many questions about his career.[5]

Debreu is perhaps best known for his 1954 joint proof with Kenneth Arrow of the existence of a general competitive Walrasian equilibrium (Weintraub 1985), and his 1959 monograph *The Theory of Value*, which still stands as the bench-mark axiomatization of the Walrasian general equilibrium model. In retrospect, the 1959 book wore its Bourbakist credentials on its sleeve, though there may have been few economists at that juncture who would have understood the implications of this statement:

> The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics. The effort toward 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, and this has not failed to happen in the present case. It may also lead to a radical change of mathematical tools. In the area under discussion it has been essentially a change from the calculus to convexity and topological properties, a transformation which has resulted in notable gains in the generality and the simplicity of the theory. Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations. In order to bring out fully this disconnectedness, all the definitions, all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics; moreover, the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions: "in the language of the theory," "for the sake of the theory," "formally." Such a dichotomy reveals all the assumptions and the logical structure of the analysis. (Debreu 1959, x)

While it was the case that most economists would have been unfamiliar at that

[5] Weintraub interviewed Debreu at his Berkeley office over two days, 4–5 May 1992. The material cited as Debreu 1992b in the text is taken from a lightly edited version of the transcript of that interview.

time with the novel tools of set theory, fixed point theorems, and partial preorderings, there was something else that would have taken them by surprise: a certain take-no-prisoners attitude when it came to specifying the "economic" content of the exercise. Although there had been quantum leaps of mathematical sophistication before in the history of economics, there had never been anything like this. For instance, few would have readily recognized the portrait of an "economy" sketched in the monograph:

> An economy E is defined by: for each $i = 1, \ldots, m$ a non-empty subset x_i of R^1 completely preordered by $\lesssim_i$; for each $j = 1, \ldots, n$; a non-empty subset of y_j of R^1; a point ω of R^1. A state of E is an $(m+n)$-tuple of points of R^1. (Ibid., 75)

While more than one member of the profession might have thought this species of economist had dropped from Mars, in fact, he had merely migrated from France. The way that this happened might go some distance in explaining the otherwise totally unprecedented character of this kind of mathematical economics.

Gérard Debreu was born on 4 July 1921 in Calais, France. He experienced a successful early school career, preparing for the baccalaureate by studying physics and mathematics. His plans to study at a lycée for entrance into one of the Grandes Ecoles were disrupted by the beginning of the war, but he did manage further preparation in mathematics at Grenoble; he won the Concours Général in physics in 1939, and later admission into the Ecole Normale Supérieure.

The group entering the Ecole Normale Supérieure was divided roughly in half, with around fifty students each in the humanities and sciences. Around twenty were thus mathematics students.

> The sciences were divided basically between mathematics on one hand and physics and chemistry on the other (the two went together) and there was a third possibility (but very few students went that way), that was biology. And I imagine that in our group maybe only one or two went the way of biology whereas the division between mathematics and physics and chemistry was about even. All science students took the same examination [to] enter the school, and then we decided which way to go. In mathematics it was normally a three year course and in physics I think it was four. And at one point I thought I wanted to take my distance from mathematics because it was very abstract, and as I wrote somewhere else I was interested in several other directions. One of them was economics as you well know, but one was astrophysics, though I did not go very far. The problem in astrophysics was that first of all, the faculty at the University of Paris was depleted during the Second World War. I think some of them were Jewish and it was unwise for them to stay in Paris. And others were communists (and some were both!) So what happened in astrophysics is that when I looked around, I found — maybe my search was not long enough, deep enough — but I had the impression that there was no faculty so it was not a very promising field

> because I would have had to study entirely on my own to stay in that field. (Debreu 1992b)

The mathematical training that Debreu received at the Ecole Normale Supérieure was very different from that which he had had earlier. Instruction was carried out, in mathematics, in a complicated fashion.

> It's very strange. Again, it is unique. If you take another Grande Ecole like the Ecole Polytechnique, they have all their teaching within the school only for students there. Not at the Ecole Normale Supérieure. It is close to the Sorbonne, geographically close, and we were supposed to take the standard courses at the Sorbonne. And what we had at the Ecole Normale Supérieure was very small seminars; that is where we were taught by Cartan. There was no fixed curriculum, and it was attended by about 10 people whereas in the fundamental courses at the Sorbonne the attendance was at first in the hundreds. I do remember a course taught by the physicist . . ., I believe he is the father of a Prime Minister, and I found that since there were so many students (and the lectures were available in writing) that I stopped going to them altogether. What was lacking in them then was the enthusiasm that Cartan generated. (Ibid.)

The instructor Debreu remembers best is Henri Cartan, one of the Bourbaki "founders."

> It was very likely I met him in 1941 but I may be off by one year. In any case I was aware of what Bourbaki volumes had already appeared, which in fact by 1941 was very little, I think it only two volumes. And even then, one was a summary. (Ibid.)

For Debreu the mathematical work was interesting, but he already had some idea that he was perhaps going to be more involved with mathematics in another discipline. Perhaps this was because of his earlier success in physics, perhaps it is because he reached a limit as it were in his ability to sustain interest in pure mathematics under the conditions of wartime Paris. At any rate, Debreu seems to have understood, fairly early in his career at the Ecole Normale Supérieure, that his own path was to be a bit different from those of the mathematicians.

> The objective at the Ecole Normale Supérieure was basically to produce teachers of mathematics; and that was understood in the days when I was there, to mean teachers of mathematics at the Mathématiques Spéciale Préparatoire and Mathématiques Spéciale level. Students had to make decisions then whether they wanted to become teachers or research workers, and some of them went one way and some went the other. I do not know whether the decision was made as we entered, or whether we discovered two years later we might want to do research. After a year or so (I entered in the fall of 41), I began to wonder whether mathematics, at that time, was

> becoming too very abstract under the influence of Bourbaki — though not so very dominant as it later became (though maybe I anticipated that development). I had to decide whether I wanted to spend my entire life doing research in a very abstract subject. You must also remember that during that last year of high school when I was influenced by my physics teacher I had thought that physics was going to be my field. (Ibid.)

His training at the Ecole Normale Supérieure was at the highest university level, and in fact can better be compared to the work done at the graduate level at most other universities, because the students had to do the standard university mathematics curriculum on their own, as it were. At the Sorbonne

> it was lectures, fairly polished lectures. I faithfully attended the lectures by Garnier, and Garnier taught differential geometry. Valiron taught classical analysis, and later on I took lectures by Gaston Julia on Hilbert Space. I'm sure I took other lectures as well. And then in the seminar [at ENS] it was a mixed bag; we occasionally had a lecture by Elie Cartan, the father of Henri, who was of course already at that time a very revered mathematician. We had a lecture by De Broglie, the physicist, Nobel Prize winner. So the seminar was a little of different things by different people. Henri Cartan was still young, and did great things later, and the seminar was simply supposed to review mathematics, and it did that; it was also to give us a taste of a variety of mathematical researches, and no text was used. On my own, I read most of Goursat. (Ibid.)

The point of the story, of course, is that Debreu was as well trained in mathematics as was possible for any student to have been at that time. He had the remarkable good fortune to be at the place, at the time, when mathematics itself was being re-represented by Bourbaki as a discipline defined by its pursuit of the implications of, and the investigation and exposition of, the idea of mathematical structure. In this mathematical hothouse, isolated because of the war and the dislocations it produced, Debreu pursued mathematics but did not want to have it define his intellectual life. But there were no real alternatives; he was a mathematics student first, and other possibilities would have to be deferred to the end of the war, since the only applied alternative that he might have considered at ENS, astrophysics, seemed to be ruled out by the absence of any real instructional program — the professor was not present. Stuck in mathematics, around 1943 he looked at possibilities for later work:

> When I became interested in economics as a possibility (as before I had become interested in astrophysics) I got hold of the standard text studied by students of economics at the university. I don't remember who the author was but it was very non-theoretical (somebody I never met). I know the textbook was popular then — I don't believe I have kept a copy — but in any case, my first impression of economics was very disappointing because I was

> coming from a world of very sophisticated and rarified mathematics and found only a very pedestrian approach to economics. (Ibid.)

Debreu has recounted, in a couple of places where autobiographical material is available, the happenstance of his receiving a copy of Allais' 1943 book, *A la recherche d'une discipline économique*. In conversation he noted that his move to economics was a feature of his own intellectual changes as well as a circumstance of the times:

> To a large extent it was pure chance because Allais had sent his book to a friend of mine, who was a humanist, who was the president of his class at ENS — he was actually not in my own class but one year after, but we were friends and he gave me his copy. I suppose otherwise if I had persevered in my interest in economics that I might not have been aware of the Allais book for months, and maybe by then it would have been too late. But one part of my interest in economics — although it was not too elegant a field — was simply that the war economy in France was special. We believed, though we found it a long time coming, that Germany was going to be defeated. It was clear that there would be a lot of reconstruction in particular in France. There would be a lot of reconstruction work to do after the war and it proved to be the case. And that may be why I came to know people like Pierre Massey, who was at one time president of Electricité de France, but who also wrote a book on stock management. I came to know him very well and I saw him regularly until his death. He was succeeded as President of Electricité de France by a friend of mine, Marcel Boiteaux, who also had his career disturbed by the war. He was an officer in Italy somewhere and later on after the Aggrégation cast his lot with Electricité de France. We shared the extraordinary story of the coin-tossing for the Rockefeller Fellowship. So a number of random events were surely very important [in my move to economics].

The book by Allais arrived in Debreu's hands at a very crucial moment, for Debreu was searching for meaningful work, as many young people search at that age. The Allais book had been more or less just sent around to individuals; not many copies had been printed. It was very much outside the established French economics channels. In retrospect, it is not only remarkable that it was able to be printed under those wartime conditions, but also that it received any attention at all, given the number of unusual books that merely drop out of sight. Even had it gone to all those interested in mathematical economics at that time, there would have been problems. It was very primitive mathematics from a Bourbaki point of view, though it was more sophisticated than most neoclassical texts. Nevertheless in Debreu it found a friendly reader.

> First of all I saw that mathematics could be used in economics in a rigorous way, even though it was not the kind of mathematics I was most fond of. And

> maybe I felt that there was a lot to be done with more sophisticated mathematics in economics. My interest in economics wasn't ready made. I became interested in economics in 1943 or maybe a year before. And the circumstances were such that Bonpère gave me that book around April of 1944. But I think that the circumstances were not sort of ideal for me to read it just then — recall that D-Day was June 6. It was only in September I suppose when it was clear first of all that I would not start another academic year normally. Things were too chaotic in France and it was then I think that I became serious about economics; I may have looked at it when Bonpère gave it to me but I did not study it. Since a few people got hold of copies, my guess would be that most of them must have been repulsed by all the characteristics of the book, it was lengthy, technical, et cetera. But remember that I was in a very bizarre situation because it had become clear to me that I would not be a professional mathematician. It was late when I decided that, and my career was disturbed by events of the war. I was supposed to take the final examination in the spring of 1944 after 3 years, but that was exactly when D-Day occurred. I took that examination eventually in 1946. So I finished my studies two years late: I was supposed to finish in 1944 and I actually finished in 1946. So it is difficult to say what would have happened if D-Day had not occurred because that two year delay (part of which time I spent in the French Army) gave me a chance to get much better acquainted with economics than I would have been if my curriculum had followed its normal course. But in any case when I took the Aggrégation, it was a pure mathematics examination. It was a somewhat bizarre situation. It was an examination of a somewhat scholastic nature, which was all the more so for me; it was very classical whereas Cartan in particular had done contemporary mathematics, which was not the case with the Aggrégation, so I had a number of problems. (Ibid.)

The story, in outline, is clear at this point. Debreu was a very well-trained research mathematician forged in the Bourbaki mold. However, he also fits a rather common profile of many key figures in political economy from the 1930s and 1940s: someone with very little background in economics moving over into that field following a thorough academic training in physics or mathematics (Mirowski 1991). The engineer autodidact Allais was not at all representative of the state of political economy in France at the time, and as a consequence Debreu subsequently had to find his own way around the mathematical economics literature; he was, by his own account, particularly impressed by John von Neumann and Oskar Morgenstern's *Theory of Games and Economic Behavior* (1944) in this period. From 1946 to 1948 he occupied the position of research associate at the Centre National de la Recherche Scientifique; and upon being awarded a Rockefeller travel fellowship, he toured Harvard, Berkeley, Chicago, Uppsala, and Oslo: the most fateful of those visits was the sojourn at the Cowles Commission in Chicago.

His appearance at Cowles in 1949 was fortuitous. The Cowles Commission up to that point had been primarily known as a center for the development of econometrics — the application of mathematical statistics to empirical economic questions — but various crises having to do with disappointments in their program of structural estimation and turf battles with the economics department at Chicago were causing the unit to contemplate a change in research direction (Epstein 1987, 110; Mirowski 1993). The research director at the time was Tjalling Koopmans, a Dutch refugee from quantum physics whose prior work had primarily involved statistical estimation. The reorientation of research away from empirical work and toward mathematical theory had already begun under Koopmans by 1949, but it clearly lacked direction.[6] Debreu felt right at home among the mathematically sophisticated advocates of neoclassical economics, many of them also European expatriates with degrees in the natural sciences. However, there was another serendipitous side to Chicago. Spurned by the economists, Koopmans had begun making overtures to the mathematics department to establish a mathematical statistics unit. The chairman of the department was Marshall Stone, one of the main boosters of Bourbakism in the American context. Stone had been reshaping the department in a Bourbakist direction since 1947, attracting André Weil and building a first-class mathematics research faculty (Stone 1989, Browder 1989). Koopmans kept in close contact with Stone through the Committee on Statistics.

The exact vectors of influence are unclear, but after Debreu permanently joined the Cowles Commission in June 1950, Bourbakism quickly became the house doctrine of the Cowles Commission. We would identify the primary philosophical texts asserting this turning point as Koopmans' *Three Essays on the State of Economic Science* (1957) and Debreu's *Theory of Value* (1959). The former was the classroom primer of the new approach, with explicit methodological discussions of the nature of mathematical rigor and the relation of economics to practices in physics; whereas the more austere *Theory* was intended to show how cutting-edge research would be done in the future. Debreu explicitly signposted *Three Essays* as facilitating the understanding of his own work (Debreu 1959, x). While Koopmans and Debreu were the main proponents of this new approach, both subsequently winning Nobel prizes for their work dating from this era, one can also observe the new orientation in the work of others associated with Cowles in this period: John Chipman, Murray Gerstenhaber, I. N. Herstein, Leonid Hurwicz, Edmond Malinvaud, Roy Radner, and Daniel Waterman. When the Cowles Commission moved to Yale in 1955, the Bourbakist attitudes toward mathematical theory began to spread throughout American graduate education in

[6] "When I joined the group in June 1950, it seemed to me very theoretical. In particular, the Cowles Commission monograph on estimation had been written, published, and Koopmans himself made a fairly drastic change because in the days when this book was developed he was deeply involved. But from the time when I knew him, he was never, I believe, working actively on estimation methods, and he had become an economic theorist" (Debreu 1992b).

economic theory, as the Cowlesmen fanned out into the major economics departments.

We cannot even begin to address adequately exactly why the Bourbakist orientation toward mathematical economics spread so rapidly within the American context, once it had been crystallized within the Cowles Commission; that would require a much more ambitious history of mathematical economics in America. But a few generalizations might be suggested, before we once more focus directly on Debreu. First, since Cowles had become disillusioned with its earlier empiricist commitments, the Bourbakist program of isolation of theory from its empirical inspiration proved both convenient and timely. Skepticism about the quality of economic empiricism became a hallmark of those who took up the program of mathematical formalization. Second, it is sometimes forgotten that the 1940s was a period of much rivalry and dissension among diverse schools of economic thought, and that Cowles often found itself in the thick of controversy. For instance, Cowles itself was battling Wesley Clair Mitchell's institutionalists at the NBER for funding and legitimacy in the "Measurement without Theory" debate; Keynesians like Lawrence Klein at Cowles were at odds with Milton Friedman and others in the Chicago economics department. Bourbakism held out the promise of rising above it all, offering a vantage point from which one might stand aloof from the Babel that threatened to drown out reasoned discourse. It almost became a badge of honor to suggest that one was not familiar with previous traditions in economics: for instance, Koopmans wrote in a proposal to the Ford Foundation, "With one possible exception (Simon) the present staff of the Commission . . . can claim no special competence in the tomes of social science literature, nor do we think that this should be a primary criterion in the selection of additional staff. . . . We intend in staffing to give greatest weight to the combination of creative imagination and rigorous logical and/or mathematical treatment of problems" ("Application to Ford Foundation," submitted 17 September 1951, p. 14; Cowles Foundation Archives, Yale University). And third, one should not forget that Bourbakism was sweeping the American mathematics profession in this same period. Many of the social sciences made concerted efforts to mathematize their doctrines in the immediate postwar era, but it was only economists who seemed to be doing mathematics of a sort that a mathematician would recognize. Indeed, Debreu was named to a position in the mathematics department at Berkeley in 1975, over and above the professorship in economics he had held there since 1962.

The rise of mathematical formalism in economics is not a simple phenomenon of the imperative of the subject matter, as is sometimes claimed; rather, it is the product of contingencies of the intersection of diverse disciplines and, as Debreu is the first to acknowledge, of numerous personal accidents and fortuitous encounters.

4. Setting the Structures Aright

When Debreu was awarded the Nobel prize in 1983, many reporters and commentators were flummoxed by their encounter with this austere economist. His work was abstruse and impenetrable, his demeanor reserved, and his resistance to using the bully pulpit to comment on current economic events unprecedented. Many within the economics profession have likewise found his program inscrutable, because they insist on trying to frame it in their own local terms. We should like to suggest that better interpretative headway could be made if the analogies to the Bourbakist program in mathematics were taken much more seriously. Indeed, many aspects of the Bourbakist program covered in section 2 of this paper find direct correspondences in the details of Debreu's version of mathematical economics.

It seems clear that Debreu intended his *Theory of Value* to serve as the direct analogue of Bourbaki's *Theory of Sets,* right down to the title. Debreu's monograph was to establish the definitive analytic mother-structure from which all further work in economics would depart, primarily either by "weakening" its assumptions or else by superimposing new "interpretations" on the existing formalism. But this required one very crucial maneuver that was nowhere stated explicitly — namely, that the model of Walrasian general equilibrium was the root structure from which all further scientific work in economics would eventuate. As perceptively noted by Bruna Ingrao and Giorgio Israel: "In Debreu's interpretation, general equilibrium theory thus loses its status as a 'model' to become a self-sufficient formal structure" (Ingrao and Israel 1990, 286). The objective was no longer to represent the economy, whatever that might mean, but rather to codify the very essence of that elusive entity, the Walrasian system. This fundamental shift in objective explains many otherwise puzzling features of Debreu's career, such as the progressive shift away from his early dependence on game-theory concepts, his disdain for attempts (like that of Kenneth Arrow and Frank Hahn) to forge explicit links between the Walrasian model and contemporary theoretical concerns in macroeconomics or welfare theory, and his self-denying ordinance in dealing with issues of stability and dynamics.[7]

Just as with Bourbaki, the problem was to justify the initial identification of the *structures.* In Debreu's case, one must insist that this was not a foregone conclusion: Walrasian theory was not widely respected in France or America; there were alternative versions of the neoclassical program, like the Marshallian apparatus of demand and supply, with more substantial adherents in America in that era; there existed some rivals to the neoclassical orthodoxy, like Marxism and institutionalism; and it was only with Joseph Schumpeter's (1954) *History of Economic*

[7] Each of these connections is noted in Ingrao and Israel 1990. What we add to their account is an appreciation for the historical specificity and subsequent fate of the Bourbakist program in mathematics, described by writers such as Corry, and their implications for Debreu's economics.

Analysis that Walras was identified as "the greatest economist of all time." To believe that the structure of *all* analytical economics lay half-obscured in the relatively dormant Walrasian/Paretian variant in 1950 was a bold leap of faith. One consideration that may have rendered the leap less unlikely was the fact that Walras presented his own work as the *Elements of Pure Economics.* Here we find a well-articulated notion of the separation of pure theory from its applied aspect; this was sure to resonate with Debreu's inclination to accept a separation of pure from applied mathematics. But another factor, operant for Allais and many of the members of the Cowles Commission, was the similarity of the Walrasian mathematics to structures used in physics (Mirowski 1989).

The importance of the analogy between extrema of field theories in physics and constrained optimization of utility in neoclassical economics was acknowledged on a number of occasions by Koopmans (Mirowski 1991) and, as can be observed from one of the epigraphs that preface this paper, by Debreu. Since many of the expatriates had little background in economics, the similarities in mathematics initially served to expedite their migrations into the field. Yet the analogy could cut two ways, in that unlike the cases of such individuals as Edgeworth and Jevons, no one in the twentieth century wanted to maintain that utility and energy were ontologically identical. This left the Walrasian program bereft of an explanation of the similarities with physics. Cowles developed an interesting response to this conundrum — namely, that the novel mathematical techniques imported by Koopmans, Debreu, and others liberated economics from its dependence on classical calculus and physical analogies. Debreu, as noted, has taken this position even further by claiming that his Bourbakist program marked the definitive break with physical metaphors, since physics was dependent for its success on bold conjectures and experimental refutations, but economics had nothing to fall back on but mathematical rigor. This is entirely consistent with the Bourbakist creed, which acknowledges that mathematical inspiration may originate in the special sciences, but that once the analytical structure is extracted, the conditions of its genesis are irrelevant.

In sum, the format of each book mirrors that of the other, with *Theory of Value* exemplifying the ideal of uncompromising rigor, devoid of all heuristic or didactic concessions to the reader. Just as Bourbaki was interested in, and regarded his project as providing a handbook for the working mathematician, Debreu is best read as providing a handbook for the working economic theorist of the neoclassical components of economic theory. In retrospect, it is hard to read *Theory of Value* as anything else, since it also provides no "new" theorems or results; it is Chevalley's "very well arranged cemetery with a beautiful array of tombstones" (Guedj 1985, 20). Thus Debreu's evident enthusiasm in Chapter 7 over his capacity to incorporate "uncertainty" into the axiomatized model by keeping the identical mathematical formalisms but redefining the "interpretation" of the commodity should not be regarded as a new contribution to the economic theory of risk or ignorance; rather, in this reading, Debreu developed it as ratification of the *structural* character of his axioms. Nevertheless, in a manner undoubtedly not intended by Debreu, the monograph also shares many of the same problems of *structures* and "structures" experienced by Bourbaki.

The impediment, if we may put it thus, is multilayered but essentially similar at each level. Bourbaki had asserted that the *structures* they considered to be fundamental all shared some analytical unifying characteristics; but their claim ultimately did not hold water. Debreu appeared to argue that the Walrasian general equilibrium theory should be treated as possessing the same privileged *structural* status in economics as groups do in "algebraic structures" and as the order relation has in "topological structures"; but this assertion was ultimately problematized by the generation of mathematical economists trained to Debreu's standards of rigor — we refer here to what are often cited as the "Sonnenschein/Mantel/Debreu" results, the importance of which were rendered general currency in the 1980s. But of course in both cases the set of practices had by that late date gathered its own momentum, to the extent that both Bourbakist and Debreuvian formalism had come to represent a certain refined taste and style in mathematical expression, such that the "structural" aspects of their innovation still exemplified best-practice activity, long after they had dropped the role of providing philosophical grounding for the program. And then there was the simple issue of phase lag between the disciplines of mathematics and economics: the disillusion with Bourbaki was evident in the 1970s in mathematics; a similar soul-searching is only now coming to economics. When Debreu first read the *Fascicule* in the 1940s he had no way of knowing how the Bourbakist *structural* program would turn out in the 1960s. This perhaps helps explain the rather reserved tone of Debreu's latest pronouncements on the place of mathematics in economics (see Debreu 1991).

Debreu, as noted above, has never seemed very interested in describing the dynamics of convergence of an economy to Walrasian equilibrium. The issue of motion could not be avoided forever, however, and there was a long interval in the postwar period in which "dynamics" were redefined to mean "stability" within the mathematical economics community (Weintraub 1991). In that context, the question was posed by Hugo Sonnenschein whether the basic "structure" of Walrasian general equilibrium models placed any substantial restrictions on the uniqueness and stability of the resulting equilibria, and he proposed the startling answer: no, outside of some trivial and unavailing global restrictions.[8] The devastating effect this had on Debreu's older "structural" program has been nicely captured by his major protégé, Werner Hildenbrand:

> When I read in the seventies the publications of Sonnenschein, Mantel and Debreu on the structure of the excess demand function of an exchange

[8] The trivial restrictions placed on the excess demand functions are continuity, homogeneity and "Walras' identity." The key papers are Sonnenschein 1972, Mantel 1974, and Debreu 1983, Chap. 16. It is interesting that Debreu contributed to the clarification of these mathematical results but has never been willing to comment on the significance of these results for his entire research program.

> economy, I was deeply consternated. Up to that time I had the naive illusion that the microeconomic foundation of the general equilibrium model, which I had admired so much, does not only allow us to prove that the model and the concept of equilibrium are logically consistent, but also allows us to show that the equilibrium is well determined. This illusion, or should I say rather this hope, was destroyed, once and for all, at least for the traditional model of exchange economies. I was tempted to repress this insight and continue to find satisfaction in proving existence of equilibrium for more general models under still weaker assumptions. However, I did not succeed in repressing the newly gained insight because I believe that a theory of economic equilibrium is incomplete if the equilibrium is not well determined. (Hildenbrand 1994, ix)

This impasse is something more substantial than the sorts of obstacles that are periodically met in the course of any vibrant science; these results have been seen as damaging precisely because they call into question the entire Walrasian framework as the appropriate *structure* for the elaboration of mathematical economic theory. The Bourbakism propagated by Cowles had rendered this identification something well beyond the usual adoption of some characteristic models by an academic school; it had become conflated with the very standard of mathematical rigor in economic thought. Indeed, this defined the Cowles program from its inception: Why precisely should the Walrasian framework be taken as the sole "structure" from which all mathematical work should depart? And just what was the "correct" Walrasian model? Was it the one actually found in the texts of Walras or Pareto or Edgeworth or Hicks or Allais? Or, to put it in Saunders Mac Lane's terms: Was it not better to make a case for the "right" level of generality than to claim one had attained the maximum level?

The answer for Debreu, as in the case of Bourbaki, was that rigor was more a matter of "structure," of style (and politics, as Mandelbrot rightly insisted), and of taste; but ultimately styles and tastes change, for reasons that can only partly be accounted for by the internal criticisms generated by the activities of the closed community of mathematicians. While Debreu hoped that raised standards of mathematical economics would put economic discourse on a more stable basis, there was never any *formal* reason to believe it would be so.

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