6 Equilibrium Proofmaking

with Ted Gayer

There is no Algebraist nor Mathematician so expert in his science, as to place entire confidence in any truth immediately on his discovery of it, or regard it as anything but a mere probability. Every time he runs over his proofs, his confidence encreases; but still more by the approbation of his friends; and is rais'd to its utmost perfection by the universal assent and applauses of the learned world.
—David Hume, *Treatise on Human Nature*

Each year, new economics Ph.D. students learn the proof of the existence of a competitive equilibrium as if a rite of passage. From the utility maximizing behavior of consumers and the profit-maximizing behavior of firms, neophyte economists soon can demonstrate that under certain conditions there exists a competitive market-clearing general equilibrium price vector. While there are a number of proofs that establish the existence of such an equilibrium, the validity of these proofs is indubitable. Indeed, economists with even scant knowledge of the history of economics can identify Kenneth Arrow and Gerard Debreu's 1954 *Econometrica* paper as having provided the proof that settled the issue.

That paper, "On the Existence of an Equilibrium for a Competitive Economy," appeared to bring closure to an argument that was (at least) two centuries old. The paper was cited in the award of the Nobel prizes to both Arrow and Debreu. The canonical account of the context and origin of the Arrow-Debreu paper suggests that its history may be traced through a series of different lines in several literatures, in several disciplines, on at least two continents, all of which converged to publication in 1954.[1]

The Arrow-Debreu model was a major accomplishment; it presented an economy composed of individual, self-interested agents—both utility-maximizing households and profit-maximizing firms—pursuing their own self-interest and whose actions produced an equilibrium in which all choices were potentially reconciled. Put briefly, the pursuit of individual self-interest could lead not to social chaos but to a coordinated social order. But how did a piece of work in mathematical economics actually settle an economic question? How did it come to pass that a particular paper, in a journal at that time read by very few economists, came to be accepted as having established a foundational truth about market economics? These are not questions economists typically ask. "The theorem proves that . . ." is enough information to persuade economists that the knowledge associated with the theorem is secure knowledge. Professional economists are confident about the result and the implications of the equilibrium proof, and no one needs to attend to the means of its construction: the validity of the equilibrium proof is incontrovertible. Economists-in-training must learn that the existence of a competitive equilibrium has been proved. All economists can make use of the proof of that result without subjecting it to incessant challenge and reassessment.

Scientists must take some components of their research as given; intellectual paralysis awaits the scientist who seeks to reopen every foundational issue every day.[2] For most economists the competitive equilibrium proof is a tool to use with little regard to how the tool was constructed. Those who study science use the idea of a "black box" for settled results that are locked up and impenetrable, and thus closed to current investigation.[3] For every science, black boxes are both healthy and necessary. But how do novel ideas get closed up into black boxes? By what means does a new claim of knowledge gain acceptance within a scientific community? Today, decades after the publication of the Arrow-Debreu proof, it is relatively easy to view it as both immutable and uncontroversial. Yet how was its validity assessed initially? More generally, how does a scholarly community determine that a proof is valid, especially when the proof is highly complex and when there are few people in the community with the technical skill to understand the proof? And what might "understanding a proof" entail?

We will address such questions by examining in some detail the circumstances that surrounded the assessment and the publication of the Arrow-Debreu paper. To examine the circumstances in which the proof became common knowledge does not, of course, diminish the proof itself. Understanding the world in which Newton lived and made his contributions offers insight into the formation and acceptance of his contributions without denying the truth of his theories. Likewise, by considering the community in which the Arrow-Debreu paper appeared, we seek to understand how the economics community assessed and established a claim to knowledge, not to denigrate Arrow and Debreu's exceptional contribution. To borrow from Bruno Latour (1987, 4), we will explore the distinction between "ready made science," which is represented by the valid and true equilibrium proof, and "science in the making," which is represented by the proof's construction, assessment, and initial acceptance by some members of the community. Thus this chapter will seek to uncover both the context in which, and the process by which, Arrow and Debreu's proof moved from being a novel claim within the small community of mathematical economists to being an established truth among the much larger community of economists.

Closing the Black Box

When was the black box closed? Did the economics community universally accept the validity of the proof immediately upon publication, or did it take several years for the proof to become known? One way to gauge its acceptance within the profession is to examine the standard microeconomics textbooks used in the training of advanced undergraduate students, and new Ph.D. students.[4]

If we start in the 1940s, years before the publication of Arrow and Debreu's proof, we find Sidney Weintraub's take on the existence of a competitive equilibrium. In his 1949 book, *Price Theory*, he wrote:

> Presumably we might be ready to concede that particular equilibrium analysis divulges some fundamental tendencies in the economy, the end-results of market processes that secure a balance. But we are much more reluctant to concur in the view that all markets are in balance

> simultaneously; admitting the tendency in individual markets is still a long way from subscribing to the proposition for all markets simultaneously, over any period or even any moment of time. But once we acknowledge that in each particular market and in each sector of the economy that there are certain equilibrating forces at work, there is no sensible reason to shrink from the view that the entire system, or a good portion of it, can settle down in an equilibrium of supply and demand. . . . Nevertheless, whatever violence the idea of general equilibrium does to our sense of reality, and even if we entirely reject it as an artificial image of the economic world, it is still incumbent upon us to demonstrate the conditions that need to be satisfied for the general equilibrium of production and consumption, and to explore the interdependence among markets. (Weintraub 1949, 127)

Thus, while not referring to a "proof" of the existence of an equilibrium, Weintraub informed the young Ph.D. student that "there is no sensible reason to shrink" from the view that an equilibrium exists.[5] And while questioning the "sense of reality" of the general equilibrium model, Weintraub stressed the importance of demonstrating the conditions that satisfy the existence of an equilibrium. This demonstration occurs a few pages later:

> Rather than rely on the verbal proof that general equilibrium is conceptually possible, the mathematical proof rests on the demonstration that for each price that is to be determined we have an equation. *If the number of equations is equal to the number of unknowns then the results are deemed to be determinate*; the counting of equations gives evidence that there is a set of prices that can establish simultaneous equilibrium in the several markets. Other properties of the structure of equations, such as the demonstration that the equations permit of only a unique set of prices, are regarded as a problem mainly of mathematics rather than of economics. The economic interpretation is often fairly simple. (130; emphasis added)

In 1949 the proof of the existence of an equilibrium (as presented to Ph.D. students) rested on the equivalence of the number of equations and the number of unknowns in the system of equations. This line of argument had a long history in economics, going back to Walras, and was reiterated in textbooks like those of Bowley and treatises like Hicks's *Value and Capital*. Even though a small number of economists in the 1930s had understood that establishing the existence of an equilibrium was a difficult mathematical problem, and even though there were some notices of the work by Wald and von Neumann to appear in the larger literatures, such analyses seemed not to have "crossed over" as it were into mainstream economics, instead being relegated to the backroom of "mathematical economics."[6] As our concern is with the transition process by which the understanding of a small coterie became the knowledge of the larger community, we will want to see how the idea of counting equations and unknowns was discarded and replaced with the existence proof presented by Arrow and Debreu.

Another competing textbook used at that time was George Stigler's *The Theory of Price*. In the first edition of 1946, Stigler mentioned general equilibrium briefly in a subsection of the introductory chapter. He first voiced skepticism concerning general equilibrium studies,[7] stating that "*general* equilibrium is a misnomer: no economic analysis has ever been general in the sense that it considered *all* relevant data. . . . The most that can be said is that general equilibrium studies are *more* inclusive than partial equilibrium studies, never that they are complete" (28; emphasis in original). He had little to say concerning the existence of an equilibrium, writing that "the outstanding characteristic of the conditions of equilibrium is that they are equal in number to the unknown quantities and prices which are to be determined. The conditions are, in mathematical terminology, the equations of the economic system, and prices and quantities are unknowns" (30).

In a substantially revised edition of 1952, Stigler added a chapter (the last one) on "General Equilibrium." While it was still two years before publication of Arrow and Debreu's proof, Stigler mentioned that "some beginnings have been made to a theory of general equilibrium" (287). This suggests that he had some idea that proof by counting equations and unknowns was unacceptable to some theorists.[8] In the meantime, as was done in Weintraub's text, Stigler set up the demand functions and supply functions, then counted the number of equations and the number of unknowns in the system, and concluded:

> The set of prices and quantities satisfying the equations constitute a general equilibrium: we have simultaneously fulfilled the conditions that quantity demanded equals quantity supplied in every market, taking full account of the fact that supply and demand in each market depend (in ways fixed by consumer and producer behavior) upon all the prices in the economy. A change in the demand for any commodity, or in the quantity of a productive service, or in any production coefficient, or the fixing of one price by fiat, will affect all other prices and quantities. (294).[9]

Though ignored in Stigler's textbooks, Arrow and Debreu's *Econometrica* article was cited in the 1958 first edition of *Microeconomic Theory: A Mathematical Approach,* by James M. Henderson and Richard E. Quandt. The chapter on "Multimarket Equilibrium" contained a subsection on "Existence Theorems," which summarized, with few details, the new proof. It is here that we first see the claim in a textbook that "Arrow and Debreu have proved that a competitive equilibrium solution exists" (155). Thus, doctoral students at the time were taught that an equilibrium exists under certain conditions, but they were not taught the proof itself in the textbook:

> Arrow and Debreu have considered the problem of existence for abstract multimarket systems similar to the one presented [previously]. . . . They employ set-theoretical techniques rather than differential calculus. Their assumptions for the first of the two cases which they consider are approximately as follows: (1) no firm realized increasing returns to scale, (2) at least one primary factor is necessary for the production of each commodity, (3) the quantity of a primary factor supplied by a consumer cannot exceed his initial endowment, (4) each consumer's ordinal utility function is continuous, (5) consumers' wants cannot be saturated, (6) indifference surfaces are convex with respect to the origin, and (7) each consumer is capable of supplying all primary factors. Arrow and Debreu have proved that competitive equilibrium solutions exist for all systems that satisfy these assumptions. They weaken assumption (7) in the second of the existence proofs. (155)

In the preface to their 1971 second edition, Henderson and Quandt listed the "proof for the existence of equilibrium in a competitive economy" among the "new material that appeared in the economic literature since the publication of the first edition or was considered too new or difficult for inclusion at the earlier time" (v). In this second edition the authors added a subsection on "The Existence of Equilibrium" to the chapter on "Multimarket Equilibrium." Unlike the first edition, the authors went on to provide a detailed account of a proof of the existence of an equilibrium for "particular sets of excess demand functions" (178) and for "the general problem of existence for a short-run version of the production and exchange system presented in [a previous section]" (178). Instead of using Arrow and Debreu's proof, they focused on Brouwer's fixed-point theorem to prove the existence for the restrictive case. For the general case they only offered an outline of Debreu's use of the Kakutani fixed-point theorem from his (1959) *Theory of Value*.

Thus, as early as 1958 Henderson and Quandt were instructing economics students that under certain assumptions, Arrow and Debreu "have proved that competitive equilibrium solutions exist." Within a few years of the publication of Arrow and Debreu's *Econometrica* paper, the validity of their proof had gained widespread acceptance within the community of economists, although the details were not presented to the students in microeconomics textbooks. And by 1971, not only was the new proof of existence of an equilibrium universally accepted within the profession, but students were presented both with the details of a special proof based on Brouwer's fixed-point theorem under certain restrictive demand assumptions, and also with an outline of Debreu's less restrictive proof.[10]

If we are correct in assuming that Ph.D. textbooks reflect the consensus of what constitutes knowledge within a discipline (or instantiates the paradigm of normal science), then we can infer that the Arrow-Debreu proof was generally accepted as having established the existence of a competitive equilibrium by 1958, which was but a few years after publication of the article. If one further believes that it was the publication of the proof in *Econometrica* that signaled the acceptance of its validity by the community of mathematical economists, and thus convinced the broader community of the truth of the sentence, "There exists a competitive equilibrium market clearing price vector," then the historians' task is to examine the process by which *Econometrica* assessed the proof.

We do not mean to suggest that the broad acceptance of the validity of the proof was a discrete event, occurring one day at a seminar presentation or sometime in July of 1954 when the *Econometrica* volume containing the article was circulated. Indeed, we believe that the acceptance of a novel claim of knowledge is a dynamic process, as the lid to the black box descends, gains momentum, and ultimately slams shut. In 1949, the consensus within the nonmathematical economics community was that a competitive equilibrium existed, and it could be established by counting supply-demand equations and price unknowns. By 1958 Henderson and Quandt confidently asserted that Arrow and Debreu had proved the existence of a competitive equilibrium, implying that the result had not been established as true earlier.

Writing and Submitting the Paper

The story of the Arrow-Debreu paper is relatively well known (Weintraub 1983; 1985). The history of "existence of equilibrium" is a story of Abraham Wald's work in Vienna, following Schlesinger and perhaps Remak (in Germany), John von Neumann's 1936 masterpiece, and separate lines of attack that developed with Kenneth Arrow at Hotelling's Columbia, Gerard Debreu from Bourbaki's loins at the École Normale Supérieure, and Lionel McKenzie's "retooling" rebirth at Tjalling Koopmans's and Jacob Marschak's Cowles Commission over twelve months in 1949–50 (Weintraub 1985, 98–100). The Arrow-Debreu collaboration emerged from the work each of them did separately, and although their times at Cowles did not overlap, they eventually learned of each other's activity through the organization's working policies: in 1950–51, Debreu was given the paper by Arrow on "the fundamental theorem of welfare economics" to referee for the internal Cowles publications system, and was asked to "comment on the substance of the paper" (Weintraub 1983, 28). That paper was in fact quite similar to Debreu's own paper, written prior to June 1950, which was to appear in the July 1951 *Econometrica* as "The Coefficient of Resource Utilization" (ibid.). Both the Debreu and the Arrow papers set up the structure of the competitive model in a form that was to be used by each, in their own next papers, to establish an equilibrium. That is, by early 1951 both Arrow and Debreu were working with a model of an economy in which the definition of a competitive economy was developed in a fashion consistent with an approach for examining the equilibrium price system for that economy.

It was then for Kenneth Arrow that:

> According to my recollection, someone at RAND prepared an English translation of the [Wald] *Ergebnisse* papers to be used by Samuelson and Solow in their projected book (sponsored by RAND), which emerged years later in collaboration with Dorfman. I read the translations and somehow derived the conviction that Wald was giving a disguised fixed-point argument (this was after seeing Nash's papers). In the fall of 1951 I thought about this combination of ideas and quickly saw the competitive equilibrium could be described as the equilibrium point of a suitably defined game by adding some artificial players who chose prices and others who chose marginal utilities of the income for the individuals. The Koopmans paper then played an essential role in showing that convexity and compactness conditions could be assumed with no loss of generality, so that the Nash theorem could be applied.
>
> Some correspondence revealed that Debreu in Chicago [at Cowles] . . . was working on very similar lines, though he introduced generalized games (in which the strategy domain of one player is affected by the strategies chosen by other players). We then combined forces and produced our joint paper. (as quoted in Weintraub 1985, 104)

In similar vein, Gerard Debreu recalled that:

> [It] was when [the Koopmans monograph] was published that I learned of the existence of A. Wald's papers on general economic equilibrium, and only when the English translation appeared in *Econometrica* [October 1951], did I get acquainted with its contents. At that time, in the Fall of the 1951, I was already at work on the problem of existence of general economic equilibrium. . . . The influences to which I responded in 1951 were the tradition of the Lausanne school and, in particular, the writings of Divisia, Hicks, and Allais; the theory of the games and, in particular, the article of J. Nash; the [paper on fixed points by] Kakutani and the [1937] article of von Neumann . . . [as well as] the linear

was submitted for publication to *Econometrica*, probably in the first week of June 1953, it had to have been read by internal Cowles referees as well as those monitoring/refereeing the Office of Naval Research contract products. We have then a quite usual scholarly time frame for that pre-fax, pre-Xerox period: the paper was presented in late December 1952, it must have been retyped, and read by people at Cowles and perhaps Stanford in the winter and spring of 1953, and sent back and forth between California and Illinois with changes and corrections and emendations prior to submission to *Econometrica* on or around 1 June 1953.

Our evidence for the submission date is a letter, dated 15 June 1953, from Robert Strotz (managing editor of *Econometrica*) to Nicholas Georgescu-Roegen that dealt with three separate matters.[12] The third paragraph reads:

> I am enclosing three copies of a manuscript submitted by Arrow and Debreu which falls in your department [as Associate Editor]. I hope you will be good enough to arrange for the refereeing of this paper and to advise me on it. I should mention that a rather similar paper was submitted some time earlier by Lionel McKenzie and that it has not yet completed it [sic] processing. As a matter of fact it is being read at present by Leo Hurwicz and John Nash. I suppose, therefore, that these two readers should not be burdened further with the Arrow-Debreu paper.

Thus, Georgescu-Roegen was given little advice on whom to choose as referees, only being told not to choose Hurwicz or Nash (and by implication, not McKenzie). The choice of referee is complicated by the tradeoff between finding a qualified referee and finding an impartial referee. It is a rare referee who reads every line and every calculation of a paper. As noted by the mathematicians Philip Davis and Reuben Hersh, "[Only one] whose interest and training are very close to the author's would be willing and able to do this kind of checking" (Davis and Hersh, 61). Yet, a referee with such interests may be prejudiced toward publication and thus might be a poor referee.

Reading that past from this present, the process at *Econometrica* was troublesome. If the associate editor had been charged to find individuals with little or no connection to either Arrow or Debreu to referee the paper, he was going to find that to be a difficult assignment. Certainly all of the Cowles people were "disqualified." Likewise the people at RAND who were connected to Arrow by that time were not going to be able to help. But there were not many mathematical economists in 1952 outside those groups. Except for a very few places like Chicago, MIT, or Stanford, the community of mathematical economists hardly existed in the early 1950s. For example, we know that Sidney Weintraub, who had taken at most one calculus course in his entire undergraduate and graduate career, was implored by his chairman Raymond Bowman, and agreed, to teach the graduate course in mathematical economics at the University of Pennsylvania in 1950–52. Finding a mathematical economist to appraise the mathematically complex paper by Arrow and Debreu was not easy since mathematical economists tended, like individuals in any other marginalized subdiscipline, to send their writings to each other before submitting them for publication. Their papers were most often presented in conferences sponsored by the Cowles Commission, the Econometric Society, or by the RAND Corporation.[13] With this in mind, Georgescu-Roegen's choices for referees appear less curious.

The Referees

The two referees selected by Georgescu-Roegen were William Baumol of Princeton University's Economics Department, and Cecil Glenn Phipps of the University of Florida's Mathematics Department.[14] The request to serve as a reader went out on 23 June, and on 17 July 1953 Baumol duly sent his report off to Georgescu-Roegen. After some preliminary comments on another matter, Baumol wrote:

> I think this is a very important paper indeed, and have not the slightest doubt that it ought to be published. My only major suggestion is that, despite its length [forty-seven double-spaced pages], it would be useful to the reader to have something more explicitly said about the fundamental lemma on page 16.[15] So much is built on it and the reference to Debreu's derivation [in his earlier 1951 *Econometrica* paper] is not readily accessible. The extra space which would be required would be well worth it.

Baumol then went on to note four specific "minor suggestions" on issues like missing bracket signs and omitted circumflexes. We may thus assume that Baumol read the paper carefully enough to do some proofreading, and he believed that the paper was "very important" although he did not, in the report, discuss why this might be the case.

The report of the second referee, Cecil Phipps, has not been found. We do, however, have the account of that report that Georgescu-Roegen provided to his editor, Robert Strotz, in a letter dated 8 October 1953: "Phipps has complained many times that the mathematics of economists is faulty and I thought he would thoroughly check the mathematics of the argument. He did not do as I had hoped. Instead, he concentrated on the discussions of the axioms. Phipps is emphatically against publication, until the paper is revised. I think his comments should be sent to the authors. Perhaps they will be able to make more of them than I was."

Given the trouble he was causing, it is not unreasonable to ask "who was Phipps, and why had he been selected?"[16] We have met Phipps in the previous chapter so we will not repeat that treatment here. We reiterate though that there is no evidence that Phipps published any mathematical research. Rather he was a mathematics teacher in a small, teaching-oriented, segregated Southern public university. Somehow, though, following military service in the Second World War, Phipps got interested in mathematical economics, and became the leader of a small group of faculty and graduate students with common interests at the University of Florida. We have no written record of the meetings of this group, but we have the evidence of Phipps, who refers to his being "a member of a group" in his correspondence with Don Patinkin discussed earlier.

His student Miller's dissertation, "The Mathematics of Production and Consumption in a Static Economy," is an excellent window into Phipps's views on general equilibrium theory.[17] Miller writes of the "errors and misconceptions" that occur in the new science of mathematical analysis as applied to economics, and tells us that "nowhere in the literature have I been able to find a complete and correct mathematical treatment of the general case of production and consumption" (ii). Miller then proceeds to develop a theory of consumption, and production, and to link them with a theory of competition to produce a solution of what are, in effect, the equations of general equilibrium. The problem though is that Miller's analysis, written at the end of the 1940s for the 1951 thesis, is incoherent with respect to then current economics. And since Miller was supervised closely by Phipps, and since it was Phipps who was to take it upon himself to stop publication of the Arrow-Debreu paper based on his own understanding of how to do general equilibrium theory, it is worth pausing another moment in our story to reconstruct Phipps's beliefs, which it is fair to assume are expressed by Miller, about this cornerstone of mathematical economics.

The thesis had seven chapters. The first three contained routine reviews of optima and restricted optima, and homogeneous equations. Also, there is a discussion of what are termed "independent functions," where a "set of functions is dependent if, when values are assigned to some of the functions in the set, the values of one or more of the other functions are determined." The distinction between local and global properties seems to be ignored here in this imitation of linear independence. In any event, following this basic material, Miller goes on to apply it to economics, nevertheless ignoring the entire published literature in mathematical economics: although he has references at the end to Samuelson, R. D. G. Allen, and Hicks, etc., Miller seems not to use these books in any of his chapters. For example, he writes that marginal productivity theory is not a theory that is "both complete and correct from a mathematical point of view" (58), yet he does not point to any mistake in any other author and develops what he calls his theory (which in fact was quite standard in economics textbooks like Weintraub's and Stigler's) with only two variables! Moreover, for a mathematical treatment there was no recognition of the problems associated with non-negativity constraints, and this after the Cowles conference on programming.

The thesis builds to a final chapter in which the material on production, and that of consumption, is joined to produce a model of a closed competitive economy. Miller sets out to establish "the equilibrium point at which the economy has 'settled down': i.e., of determining the amounts of the n X's [quantities] and the n-1 price ratios of these commodities in terms of the fixed capital assets and their distribution. The solution will also embody known production and utility functions" (142). What follows is a careful rendering of all the equations of utility maximization, and profit maximization, together with assumptions about competitive markets, albeit with no recognition of the problems associated with non-negativity constraints on prices. Miller ends up with an enormous number of equations, and one less price. He then states that he can eliminate variables ending up with 2n-1 equations that suffice to determine the n amounts and n 1 price ratios, and thus establish the competitive equilibrium mathematically.

As with the textbooks by Weintraub and Stigler in the 1940s, Miller's and Phipps's view of a proof of a competitive equilibrium rests on counting equations and unknowns. Their idea of what constituted a proof is similar to the closed black box of the previous decade. Given that Arrow and Debreu's proof changed this conception of the black box of a standard proof, we can anticipate Phipps's reaction (which we will discuss later) to this challenge of what he perceived as irrefutable. It takes one's breath away. In 1951, in a mathematics department, in a thesis with references to Samuelson, Cecil Phipps and his student William Miller have recreated the equation counting argument used in microeconomic textbooks in the 1940s and sneered at in the open literature by Morgenstern ten years earlier (Morgenstern 1941). It is as though Wald had never solved the problem stated earlier by Schlesinger, and that von Neumann's paper had never been published, let alone translated into English. This was to be Phipps's contribution to the existence of general equilibrium literature, a failure to read the literature.[18] As Patinkin was to write to him, at about that same time (12 April 1950) in another context: "I am firmly convinced that you and your group must spend at least one or two years learning the basic fundamentals of mathematical economics before any worthwhile criticism will be forthcoming."

In any event, this was the intellectual framework that was to shape referee Phipps's response to the Arrow-Debreu paper.

The Decision to Accept the Paper

The first stage of the review process thus ended with Georgescu-Roegen's report to Strotz of 8 October. That report shows that the associate editor did his own appraisal of the paper, effectively refereeing it himself in light of the two reviews he had received.[19] He is quite certain about his judgment, and his six-page single-spaced letter deserves to be quoted at length:

> There is no doubt in my mind that the paper deserves to be published. Therefore the comments which follow should be interpreted simply as suggestions . . . and not as belittling the authors' contribution.
>
> After I received the manuscript, I read it superficially to decide to whom it should be sent for refereeing. My first impression was that the mathematics was rather intricate even for the top econometricians, and this opinion was reinforced after having recently read the article more carefully. In addition, the mathematics and the economics are so much inter-woven in the argument that I found it difficult to think of many referees who would be at the same time economists and mathematicians so that the critical reading of the paper would not impose upon them a tremendous task. I have asked Baumol and Phipps to comment upon it.

Georgescu-Roegen goes on to present his views on Phipps's report, as noted above, and then states Baumol's comments in favor of publication. He says he is "glad to have one of the referee's opinions to add support to my favoring the publication, so much more since this comes from an econometrician like Baumol." Nonetheless, he informs Strotz that Baumol's remarks are "trivial," and that "he did not check the argument in detail." He continues, "I do not blame him for choosing not to spend the rather considerable time required by the job." He also admits to Strotz that he, too, did not give the manuscript an exceedingly careful reading, but instead based his decision at least in part on the reputations of the authors. "I also decided that to go over the manuscript as I used to do in the past would have taken too much time. I felt that the following remarks would be more valuable to the authors than a thorough checking of the mathematics by me. I have the highest opinion of the authors and I trust Debreu's mathematics, yet I recommend that somebody check the mathematics. This could be done while the authors revise the present version, thus saving considerable time." It is not clear whom Georgescu-Roegen expected to "check the mathematics" of this admittedly complex paper.

Before going on to present Strotz, who would be the one to communicate with the authors, with specific recommendations, Georgescu-Roegen would make the following plea for simplifying the paper: "Would it not be possible either to make the proof more elementary and simpler or to present it as elaborated consequences of other well-known theorems? I heard at Kingston[20] the paper given by McKenzie and was impressed by the very small place occupied by the technical mathematical proof in the argument."[21]

In his next set of seventeen numbered remarks to Strotz about the paper, covering four pages, Georgescu-Roegen more or less set the stage for many of the issues which would be subsequently involved in methodological discussions of what has come to be called the Arrow-Debreu model.[22] He notes for example (point #3) that the authors call one of their assumptions "highly unrealistic" and suggests that it be shortened and given less emphasis. In point #1, he is clear in his view that the paper should separate the "mathematical proofs of the abstract lemmas and theorems from the economic interpretation of the result," a call to rethink, as it were, the nature of an argument in mathematical economics.

Other points question the relation of the model to Leontief's model, or the issue of stocks versus flows, or the issue of the number of firms being fixed in advance of the equilibrium discussion. In point #8, for instance, he notes that the paper explicitly avoids the question of uniqueness of equilibrium, and suggests that a similar mention be made about the stability of equilibrium. Of most interest to future methodologists perhaps is #6:

> The paper leaves the reader with the definite impression that the existence of equilibrium for an economic system requires rather strong assumptions. If one would like to derive some realistic conclusion from this, this conclusion would be that very likely the real system would be deprived of such assumptions and of an equilibrium, also. What is the reaction of the authors to such an interpretation?

The associate editor's report was duly sent on to Strotz. We do not have any follow-up letters to the authors, but can surmise that they were given the gist of the reports. We can also surmise that Strotz's conditions for final acceptance and publication were based on Georgescu-Roegen's letter. A comparison of the draft version in Georgescu-Roegen's files and the final published article shows that there were virtually no changes to the article between submission and publication. We believe that the response to the authors and the resubmission was done over the course of the next several months, and that the final version of the paper was ready by spring 1954, and that the editor so informed the referees. That timing is then consistent with the remarkable letter, and enclosure, that Strotz received at the end of the summer of 1954.

An Objection to Publication and *Econometrica*'s Response

On 18 September 1954 Cecil Phipps submitted a letter to the editor of *Econometrica*, Robert Strotz, criticizing the validity of Arrow and Debreu's article. In his cover letter Phipps expressed his displeasure with *Econometrica* for having publishing the article. "I do not feel that this article should go unchallenged before the readers of *Econometrica*. Otherwise, economists will accept its conclusions at face value and quote it in substantiation of other arguments, perhaps ones of economic policy affecting all of us." Phipps's letter makes little mention of the actual proofs used by Arrow and Debreu.[23] Instead, he criticizes the way they set up the model of a competitive economy, their definition of an equilibrium, and some of their assumptions about consumers and firms. Phipps begins by claiming that there "are only three parts to the problem instead of the four into which the authors divide it. The first concerns the individual firm whose inputs and outputs are functions of the fixed set of prices as parameters. The second concerns the individual consumer whose income is determined by the labor he performs and the material he has or received. . . . The third part . . . may be stated as follows: If the differences between the demand and production of all but one of the commodities are specified, can the prices at which these differences exist be found from the excess supply functions?" Thus Phipps offers what he believes to be the proper way of establishing the existence of a competitive equilibrium; however, he does not offer a proof of his own.

Phipps also criticizes certain assumptions used by Arrow and Debreu. For example, he thinks it is incorrect to postulate that firms (consumers) maximize profits (utility) for a given set of prices. Instead, "the maximum in this case must be attained for any permissible set of prices, not just the final equilibrium prices as they state." He also claims that treating inputs as negative components "becomes very awkward when the output of one firm becomes the input of another. . . . The argument of the authors would have to be changed slightly to care for the change in signs." He disapproves of normalizing the vector of prices by requiring that the sum of its coordinates be 1, stating that it "has no connection with the question of a solution for these prices . . . [and] serves merely to give a unique value to the prices after the solution for the relative prices has been accomplished."

Before assessing the responses to Phipps's letter, it is useful to keep in mind Philip Davis and Reuben Hersh's observation about influences on referees' judgments: "Do the methods and result 'fit in,' seem reasonable, in the referee's general context or picture of the field? Is the author known to be established and reliable, or is the author an unknown, or worse still, someone known to be unoriginal or liable to error?" (Davis and Hersh 1987, 61). Two studies by Douglas P. Peters and Stephen J. Ceci (Peters and Ceci 1980; Ceci and Peters 1982) offer evidence that referees do consider some of the questions posed by Davis and Hersh. In their study of psychology journals, they found that a paper by an unfamiliar author at a low-status institution (two characteristics that Phipps fits well) is more likely to be rejected by the journal.[24]

There is also some anecdotal evidence, in economics, to support the claim that the prestige of the referee carries weight in the editorial decision. Paul Samuelson wrote of one occasion on which, after writing a critical referee report for the *American Economic Review*, the editor asked him if it was acceptable to give out his name to the author, for the author had stated that "I would like to know who the referee is. For if it is Milton Friedman, I must take it seriously." Samuelson replied, "I authorize you [the editor] to tell the author that the referee was not Milton Friedman" (Shepherd 1995, 20–21). Herbert Gintis recalled an instance when his paper received one five-and-a-half page, single-spaced, referee report suggesting publication, and a second referee report of thirteen lines that recommended rejection. Gintis claims that this second report was "vague, sloppy, and incorrect," yet the editor decided to reject since "the Board has great respect for the opinion of Referee #2" (ibid., 73). Thus, to the extent that editorial decisions are based on the prestige of authors and referees, we would expect Phipps to have a difficult time convincing *Econometrica* of his objections, independent of its merits.

In order to decide whether to publish Phipps's letter to the editor, Bob Strotz solicited the written opinions of Ragnar Frisch (editor of *Econo-*

metrica), Lionel McKenzie, Kenneth Arrow, Gerard Debreu, Hukukane Nikaido, Tjalling Koopmans, and Nicholas Georgescu-Roegen.

Lionel McKenzie was highly critical of Phipps's letter to the editor. On 28 September 1954 he wrote to Strotz, "This letter is extremely feeble and does not deserve serious consideration! . . . I think it would be a terrible thing to have this letter appear in *Econometrica*." His suggestion to Strotz on how to deal with Phipps's letter was "either (A) tell Phipps the material was not appropriate for a letter but you had it refereed as a note and it was rejected, [or] (B) tell him the material is inappropriate for a letter but if he wishes to submit a note you will then have the note refereed." McKenzie also claimed, as Georgescu-Roegen did earlier, that the complexity of the article precluded a careful examination on his part. He proclaimed, as had Georgescu-Roegen previously, an implicit trust in Debreu's mathematical abilities. "Even if there are correct points in it [Phipps's letter], they are no doubt trivial, and it would take me a month of Sundays to find them! Debreu, of course, is far too competent to commit such silly errors as Phipps seems to think he finds."

Ragnar Frisch's response to Phipps's letter was somewhat more sympathetic. While agreeing that "this letter contains much irrelevant and trivial talk," he tenuously suggested that Phipps might have a point. He wrote to Strotz on 28 September 1954, "I do not feel convinced that it is all sheer nonsense. I have a feeling that Phipps is perhaps touching upon some of the same fundamental difficulties that I have treated in my big paper to appear in *Économie Appliquée*. . . . I have a feeling that the kind of approach used by Arrow and Debreu could perhaps be criticized by an argument similar to the one I followed in this paper."[25] However, as Georgescu-Roegen and McKenzie did before him, he intimated that the complexity of the Arrow-Debreu paper would take too much time to examine the specific criticisms. "To find out whether this is actually so, is not a quick job, it would mean going through the paper in July 1954 *Econometrica* very carefully." Frisch was uncertain about how to advise Strotz on the matter, and wished to obtain "the reactions of Georgescu-Roegen, Lionel McKenzie, Tjalling Koopmans, and Gerard Debreu before reaching a final decision."

On 5 October 1954 Arrow and Debreu responded to Strotz about Phipps's criticism. They claimed that in order to prove his point Phipps must do one of two things: "1) to point out, with reference to page and line, where we make an inference which is not warranted by our assumptions or by logic, [or] 2) to present a model satisfying all our assumptions and having demonstrably no equilibrium in our sense." They dismissed Phipps by concluding, "As he does neither it is very difficult for us to take his comments seriously." While they believed Phipps's argument "exhibits throughout the grossest mis-understanding of our paper," they acknowledged the delicacy of the matter. "We understand that it is very delicate to suppress any scientific criticism and only ask for a chance to have a brief reply published alongside his letter if it is eventually accepted."

Nikaido similarly dismissed Phipps's letter. His 7 October 1954 letter to Strotz states that Phipps "has failed . . . to understand the version of Arrow-Debreu [sic] article; he seems to confuse argument of economic relevance with mathematical argument to confirm the former. It does not matter, in my opinion, whether mathematical arguments used to achieve the existence of economically relevant solutions admit some economic interpretation." Nikaido reiterated this sentiment later when he submitted comments to Strotz on Phipps's letter to the editor. In this 20 October submission Nikaido offered his point-by-point evidence that Phipps "has not succeeded in apprehending the version and the basic framework of the [Arrow-Debreu] article. In reading such an article as that of Arrow-Debreu, one should take much care that the economic formulation of a problem is not the same thing as the mathematical processing carried out to achieve a solution corresponding to the former."

On 19 October 1954 Tjalling Koopmans wrote up his opinion for Strotz. Koopmans acknowledged that "some of Phipps' comments point up inadequate explanations of the relation of the authors' (Arrow and Debreu) model to economic realty [sic] as well as yet unsolved problems." Nevertheless, he believed that Phipps "does not start from their premises to point out any specific errors in their chains of reasoning. Rather, he argues how he would have gone about this problem, and notes various differences, which he then describes as failures of the authors." Koopmans then gave a point-by-point analysis of Phipps's letter in an attempt to "help remove misunderstandings and thus conserve space in *Econometrica* for discussion of essential difficulties and unsolved problems."

By 3 November 1954 Strotz had received all solicited reports. In a letter to Georgescu-Roegen, Strotz requested his opinions of Ragnar Frisch's suggestions on how to handle the matter. Frisch's letter containing these suggestions is missing from Georgescu-Roegen's files. There is also no record of Strotz's correspondence with Phipps informing him of the ultimate decision. What is known is that Phipps's "Letter to the Editor" was never published in *Econometrica*, nor did its contents ever appear as a note.

From Belief to Knowledge by Proof

At what stage can economists be said to believe that a proof of a proposition in mathematical economics, or economic theory more generally, actually establishes the result that is claimed? When is a proof a proof? The eminent Cambridge mathematician, G. H. Hardy, addressed this question in his 1940 book *A Mathematician's Apology*. Hardy compared a mathematician to an observer gazing at a distant range of mountains. "His object is simply to distinguish clearly and notify to others as many different peaks as he can. . . . When he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he points to it, either directly or through the chain of summits which led him to recognize it himself. When his pupil also sees it, the research, the argument, the proof is finished" (Hardy 1992 [1940], 17). According to Hardy, a proof is a means of persuasion, it in some part consists of "rhetorical flourishes designed to affect psychology" (ibid.). More generally, of course, the design of proof "to affect psychology" acknowledges the essential social nature of proof, the outward-looking nature of the activity of the proof-maker in attempting to convince another member, or other members, of the disciplinary community, that a particular knowledge claim should be accepted into the community's stock of truths.

In the book that reported the papers given at a conference in West Berlin in 1979, the historian of mathematics Herbert Mehrtens provided an overview of the issues that the historian faces in giving an account of such a process:

> We have to construe mathematics as both a body of knowledge and a field of social practice at the same time. These are not halves of a circular area embedded in the larger area equally divided into science

> and society. While the social practice of mathematics is determined by the nature of mathematics as a special type of knowledge, the historical process of extension and change of mathematical knowledge is a social process inseparably embedded in the societal environment. An individual new idea in mathematics is brought forward as a "knowledge claim." This is an act of communications subject to specific social regulations. The evaluation of such a knowledge claim within the community of mathematicians again is a process of social interaction. . . . The inclusion of an interaction into the dogmatized body of taught mathematics, its dissemination into areas of application and other mathematical or scientific sub-disciplines are social processes as well as subject to regulations imposed by norms and institutions. (265)

Mehrtens's point directly touches our own discussion about a major theorem in an applied mathematics discipline, a contribution to the body of mathematical knowledge in economics.

In this examination of the reception of the Arrow-Debreu proof, the community of economics scholars became persuaded of the validity of the proof, thus closing it up in a black box. Did the persuasion occur before Arrow and Debreu submitted the article for publication at *Econometrica*? Both Arrow and Debreu were involved in the internal publication system at Cowles, where their paper quite likely was circulated among the mathematical economists who were members at the time. They presented the paper at the Econometric Society meetings in Chicago with, among others, McKenzie, Koopmans, Beckmann, and Chipman in attendance. It is safe to say that many, if not all, of the most adept mathematical economists were at least somewhat familiar with Arrow and Debreu's proof before it was submitted for publication.

But presentation of the paper to mathematical economists did not necessarily establish the proof's validity beyond all reasonable doubt. To think otherwise is to suggest that the refereeing process in this case or similar cases is merely a confirmation of what everyone (or everyone who matters) already knows. A more forgiving view of the refereeing of the paper is that those involved viewed the process as a means of assessing rigorously the validity of the proof in order to determine whether it would merit wider dissemination among economists. The validity of the proof had to be first accepted by the small community of mathematical economists before gaining acceptance as knowledge among the larger community of economists. Yet the transition from presubmission dissemination of the Arrow-Debreu paper in the small group, to the more open refereeing process, presented a potential problem. The editors at *Econometrica* ostensibly wanted referees who were not biased by previous exposure to the paper, but who were also mathematically adept enough critically to evaluate the paper. These two sets were virtually nonoverlapping. The result was that one referee report emphatically recommended acceptance based on a not very thorough reading of the paper, and another referee report emphatically recommended rejection, reflecting the outdated beliefs of an obscure mathematician. Did, then, this refereeing process achieve its goal of assessing the proof's validity?

The irate response by Phipps to the editor of *Econometrica* raises another question. The ability to persuade someone of the validity of a proof (as suggested by Hardy) rests in part on the mathematical sophistication of the individual being persuaded. In this case we have a paper that is so mathematically complex as to make it difficult for most economists to read it, let alone evaluate it. Yet a mathematician challenged this paper. Are the arbiters to be persuaded by the proof of Arrow and Debreu or by the criticism of the proof by Phipps? And on what grounds are they to be persuaded?

The responses to Phipps's letter point to, among other things, the role of trust in assessing claims to knowledge. The identification of trustworthy agents is essential in assessing and establishing a body of knowledge. As the distinguished sociologist of science Steven Shapin wrote, judging someone's claim to knowledge involves asking, "What are their circumstances and characteristics? What, in general and in this case, do those circumstances and characteristics testify about the likely reliability of what they say?" (Shapin 1994, 38). These, of course, are not the only factors by which individual beliefs become communal knowledge. Yet, in the case of Phipps's challenge to Arrow and Debreu's complex proof, the arbiters frequently contrasted the prestige and mathematical experience of Arrow and Debreu with the lack of prestige and lack of eminence of Phipps. Using the language Shapin (1994), Phipps stood outside the moral economy of truthmakers, and his marginality itself made his view of the proof untrustworthy, and thus finally inconseqential.

Saying this is not to belittle the criticisms of the arbiters who denigrated Phipps's arguments, for they are in good company in their reliance on trust and prestige in assessing a claim to knowledge. Paul Hoffman offered a justification for this reliance on trust in his recounting the life of the mathematician Paul Erdos:

> Today upwards of a quarter million theorems are published a year. . . . But who reads all these theorems? Proof by authority still goes a long way—that you believe a proof because you believe in the person who did the proving or the person who examines the proof. Even Erdos would say "I believe thus-and-such because so-and-so says it's true." Erdos accepted the truth of the four color map theorem because someone he trusted checked the proof. (Hoffman 1998, 200)

Our look inside the black box of the competitive equilibrium proof has uncovered a very messy process. We have seen that, given the limited number of people qualified to assess the proof, the community of economists was largely persuaded of the proof's correctness by the trustworthiness and distinction of its authors. That the subcommunity was so persuaded was strong enough evidence that the proof was correct that the larger community of economists deemed it to be incontrovertible too. This change in what had been taken to be true knowledge, as knowledge about equation-unknown counting changed to knowledge about fixed-point techniques, took place within a few years of publication of the Arrow-Debreu proof. "Arrow and Debreu have shown that there exists a competitive equilibrium" was black-boxed by the late 1950s. It is then fitting that the summation can be left to one of the protagonists in this story, Kenneth Arrow: "To suggest that the normal processes of scholarship work well on the whole and in the long run is in no way contradictory to the view that the processes of selection and sifting which are essential to the scholarly process are filled with error and sometimes prejudice" (as quoted in Shepherd 1995, vii).

during his Ph.D. oral exams. Tilley writes, "The next day I went to his office and asked him what was his statement of the theorem. He had a stack of about 8 new Calculus texts on his desk. He told me that he would show me the theorem from one of them. He took the top one off and looked up the theorem. He read it to himself and tossed the book into the waste can. He did that with each of the remaining books! He was not satisfied with any of the statements of the theorem. I never did find out exactly what he wanted as he was so unhappy with those texts that he walked out of his office after throwing them all in the trash" (personal communication 1996).

6 The articles he criticized were Patinkin (1948), Tintner (1948), and Friedman (1952a).

7 These early views on models prefigure some interesting recent work by philosophers and historians of economics (see Morrison and Morgan 1998). Morgan's discussion there opens up a new set of issues concerning modeling itself as a compelling objective for economic analysis, as she argues that economics is not well characterized by the division between theory and applications, but rather by modeling in all its complexity.

8 According to Kuhn's view, episodes of paradigm shifts lead to partial breakdowns of communication between the proponents of different theories. In *The Structure of Scientific Revolutions,* Kuhn writes that "the proponents of competing paradigms practice their trades in different worlds. . . . [They] see different things when they look from the same point in the same direction. . . . Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations on to the other" (150). As mathematics and economics are different disciplines, not simply alternate paradigms within a discipline, it is not Kuhn but rather Stanley Fish's (1980) modification of Kuhnian incommensurability that we adapt to our argument.

9 All references to the Patinkin-Phipps correspondence shall be understood to be from The Don Patinkin Papers, in the Special Collections Library of Duke University.

10 Phipps once initiated a correspondence with Nicholas Georgescu-Roegen concerning what Phipps perceived to be the use of faulty mathematics in Georgescu-Roegen's work. Georgescu-Roegen ended the correspondence after two letters.

11 Of course the relationships between Von Neumann and Morgenstern, as well as Savage and Friedman, offer other cases (with possibly different implications) of communication between mathematicians and economists.

12 Or is said to have claimed. See the discussion of the Thom "claim" in Woodcock and Davis 1978, 70.

6 Equilibrium Proofmaking (with Ted Gayer)

1 The first such history was provided in Weintraub (1983). Related material was developed by Ingrao and Israel (1990 [1987]).

2 Indeed, the philosopher Imre Lakatos (1970) made such "incontrovertibles" the cor-

nerstone of his methodology of scientific research programs, associating them with the "hard core" of the scientific research program; earlier Thomas Kuhn (1962) had of course used a related idea in developing the paradigms of normal science.

3 The image of the "black box" is taken from Bruno Latour, particularly his use of it in Latour 1987. He describes how "black boxes are used by cyberneticians whenever a piece of machinery or a set of commands is too complex. In its place they draw a little box about which they need to know nothing but its input and output" (2–3).

4 Among students taking an early course in econometrics matters were a bit clearer: "What Hercules will attempt to solve the system of equations which we have established above for the determination of general equilibrium! It will be observed that the system is not linear in character . . . [and] it is difficult to believe that the system of demand functions is essentially linear. . . . A partial existence theorem has . . . been given for the mathematical problem by A. Wald, who considered a system of the Walrasian form. He enumerated a set of conditions both on the demand functions and the technical coefficients, which would assure the existence of a mathematical solution of the equations. The complex character of the problem makes it impossible to summarize the analysis here" (Davis 1941, 186–87).

5 "Dear Professor Weintraub: Mr. Horsch has asked me to send you the list of fall adoptions on *Price Theory*. They are as follows: Alabama Polytechnical Institute, 7; University of California at Los Angeles, 55; University of Chicago, 131; Northwestern University, 9; Roosevelt College, 8; University of Delaware, 15; University of Michigan, 6; University of Detroit, 16; Lincoln University, 6; New York University, 25; University of Pittsburgh, 34; Pennsylvania State College, 15; University of Pennsylvania, 13; University of Texas, 19" (letter from Pitman Publishing Corporation, 17 October 1950).

6 As noted in Weintraub (1983), the general economics journal *Zeitschrift für Nationalökonomie* contained a survey piece by Wald in 1936, translated into English and published in *Econometrica* in 1951. Von Neumann's 1936 paper appeared in an English translation in the *Review of Economic Studies* in the 1945–1946 volume. Moreover, Wald's and von Neumann's work was certainly discussed in Schumpeter's graduate economic theory class at Harvard in the late 1930s (Weintraub 1997).

7 This "Chicago" view was in print earlier with Milton Friedman's (1946) hostile review of Oscar Lange's *Price Flexibility and Unemployment*, with Lange of course representing the "other Chicago" of the Cowles Commission. Chicago was the Marshallian antagonist to Cowles's Walrasian predilection.

8 More than a decade earlier in 1941, Oskar Morgenstern had published a hostile review of Hicks's (1939) *Value and Capital* in Chicago's own *Journal of Political Economy*, a piece that effectively sneered at equation-counting to establish equilibrium.

9 The third edition of Stigler's *The Theory of Price* appeared in 1966, well after the publication of Arrow and Debreu's equilibrium proof. Nonetheless, this edition made no mention of Arrow and Debreu, nor did it mention general equilibrium (the chapter on

general equilibrium from the previous edition was dropped). Instead Stigler, continuing to reflect the long-standing Chicago pro-Marshall, anti-Walras position, used partial equilibrium exclusively. A fourth edition of *The Theory of Price* appeared in 1987, again with no mention of general equilibrium. This edition does include a photograph of Arrow, however, and part of the caption mentions Arrow's "fundamental work on the existence of competitive equilibria" (251).

10 In their 1980 third edition textbook, Henderson and Quandt split the "Multimarket Equilibrium" chapter into two chapters: "Multimarket Equilibrium," and "Topics in Multimarket Equilibrium." Nonetheless, the exposition on the existence of an equilibrium is the same as in the previous edition.

11 The following were residents of the Cowles Commission at some point between 1950 and 1953 (see Hildreth 1986): Stephen G. Allen, Kenneth J. Arrow, Pierre F. J. Baichere, Earl F. Beach, Gary S. Becker, Martin J. Beckmann, Francis Bobkoski, Karl Borch, George H. Borts, Karl Brunner, Rosson L. Cardwell, Herman Chernoff, John Chipman, Carl F. Christ, Gerard Debreu, William L. Dunaway, Atle Harald Elsas, Karl Fox, Jose Gil-Pelaez, Thomas A. Goldman, William Hamburger, I. N. Herstein, Clifford Hildreth, William C. Hood, Henry S. Houthakker, Leonid Hurwicz, Herman F. Karreman, Tjalling C. Koopmans, Jules Levengle, Siro Lombardini, C. B. McGuire, Pierre Maillet, Edmond Malinvaud, Sven Malmquist, Harry Markowitz, Jacob Marschak, Rene Montjoie, Marc Nerlove, William Parrish, Sigbert J. Prais, Roy Radner, Stanley Reiter, Bertram E. Rifas, Herman Rubin, Sam H. Schurr, William B. Simpson, Morton L. Slater, Gerhard Stoltz, Erling Sverdrup, James G. Templeton, Ciro Tognetti, Leo Tornqvist, Jaroslav Tuzar, Daniel Waterman, Isamu Yamada, and Jagna Zahl.

12 The correspondence on which this material is based is preserved in the Nicholas Georgescu-Roegen Papers, located in the Special Collections Library at Duke University. Nicholas Georgescu-Roegen, a Romanian-born economist, was an associate editor at *Econometrica* during the period we are considering. Georgescu-Roegen studied mathematics at the University of Bucharest and earned his Ph.D. in statistics at the Sorbonne in 1932, and studied for a period under the statistician Karl Pearson at the University College in London. He visited the United States in 1934, where he became interested in economics due to the influence of Joseph Schumpeter. During this time he published his influential paper "The Pure Theory of Consumer Behavior" in the *Quarterly Journal of Economics.* He returned to Romania in 1936, but came to America for good in 1948, and spent most of his career at Vanderbilt University. In 1971 he published his book *The Entropy Law and the Economic Process,* in which he claimed that the second law of thermodynamics implies that economic processes lead the world toward disorder, and thus the steady-state equilibrium commonly ascribed to by neoclassical economics is impossible.

13 This of course is part of the larger story of the creation of a community of mathematically adept social scientists in the wartime and immediate postwar period. This story is well told in Mirowski (2001) and in Leonard (forthcoming).

14 William J. Baumol, born in New York City, received his BSS from College of the City of New York in 1942. He received his doctorate from the University of London in 1949, where he wrote a dissertation on *Welfare Economics and the Theory of the State.* He taught at the London School of Economics from 1947 to 1949, then left to join the faculty at Princeton. His book *Economic Dynamics* established him as a mathematically able economic theorist. He became a full Professor at Princeton in 1954, and since 1971 he has held a joint appointment there and at New York University where he pioneered a new area of study, the economics of the arts.

15 The paper states, immediately following the statement of the lemma, that it "generalizes Nash's theorem on the existence of equilibrium points for games."

16 A recent study by Hamermesh (1994) examines what characteristics editors look for in referees. He suggests that the current practice of top journals is to use heavily cited people to serve as referees, especially when the author is well known. In this context, the choice of Phipps as a referee is odd. However, Hamermesh also finds that journal editors frequently choose as referees people who have recently published an article in the journal. Phipps had two *Notes* that appeared in *Econometrica* in 1950.

17 We note that some of the exact language—wording and phrasing—in the Miller thesis appears as Phipps's words in letters we have from Phipps to Don Patinkin (chapter 5).

18 We have been unable to locate any corroborating biographical material on Miller. He does not appear to have left traces in any literature, or to have been noted in any material uncovered by Paul Ehrlich in his history of mathematics at the University of Florida, or anyone's memories at Clemson. Our only knowledge of Miller thus comes from his unpublished doctoral dissertation, where the "Vitae" on the last page tells us that Miller was born in 1911 in Birmingham, Alabama, and graduated from Birmingham-Southern College in 1931. He received his mathematics M.A. from Florida in 1933, and then taught high school and coached sports in Alabama. He got a job as an engineer in Pittsburgh in 1936, and in 1938 he became an Instructor in Mathematics at Clemson College. Following military service, he returned to Clemson, and eventually got a leave of absence to finish his Ph.D. in the period 1949–1951. He was thus forty when he received his degree, and presumably returned to Clemson. To be fair, Phipps was not always so off base. He did publish a couple of correction notes on papers by Milton Friedman (which appeared in the *Journal of Political Economy*) and Gerhard Tintner (which appeared in *Econometrica*), and he published a lengthy paper criticizing Patinkin's monetary theory in *Metroeconomica* (chapter 5).

19 Nonetheless, it is worthwhile to point out that although Georgescu-Roegen was in the habit of writing comments throughout the margins of submitted manuscripts, his copy of Arrow and Debreu's manuscript contains written comments only in the margins of the preliminary sections, with no comments in the margins of the sections containing the proofs.

20 The American Summer Meeting of the Econometric Society was held in Kingston, Ontario, Canada from 31 August–4 September 1953. Among others, attending mem-

bers of the program committee were Debreu and McKenzie. Baumol, Koopmans, Strotz, and Georgescu-Roegen were also present, and McKenzie gave a paper, discussed by Koopmans, on "Competitive Equilibrium with External Economies" in a session chaired by Strotz. Small world indeed.

21 In my earlier history of the theory (Weintraub 1983), I insisted on the term "Arrow-Debreu-McKenzie Model." The usage has not generally taken hold. The connection of Lionel McKenzie's work to that of Arrow and Debreu is a tricky subject to broach as two of the three men have won the Nobel Prize with citations noting the existence proof. McKenzie's work was independently done, and his use of the Kakutani fixed point theorem to prove the existence of the general equilibrium is still the favored expository route, and is the one used later by Debreu in his *Theory of Value*, and by Arrow in his textbook with Frank Hahn, *General Competitive Analysis*. A simple-minded chant of "Mertonian simultaneous discovery" seems not to suffice. After all, in the process we are describing, *Econometrica* referee/editor Georgescu-Roegen asks that the Arrow-Debreu proof be modified to resemble McKenzie's proof!

22 The eighteenth and final item is the set of fourteen minor corrections, like typographical errors, notation confusions, and suggested wording changes.

23 He does state that it "is difficult to see how this solution partakes of the nature of a game." He also criticizes the paper for not considering the uniqueness of the solution.

24 However, other studies (such as Zuckerman and Merton 1971) find no evidence of institutional bias on the part of referees.

25 This kind of self-reflecting, self-aggrandizing referee comment by Frisch has been noted elsewhere. As Samuelson has recalled: "Ragnar Frisch was pretty much autonomous editor of the early issues of *Econometrica*. He was interested in everything. Also, he believed in the superiority of his interpretations of anything and everything (indeed, he was so great a mind that there was much merit in such a belief). When Wassily Leontief participated in the post-1933 revival of the economic theory of index numbers . . . Frisch held up publication of the Leontief 1936 contribution until he could publish in the same issue of *Econometrica* his own survey article on the subject. Foul play, I say" (as quoted in Shepherd 1995, 23).

7 Sidney and Hal

1 Quotations from that autobiography will be taken from the Kregel collection's version, and will be noted as Weintraub 1988 (1983).

2 That typescript, titled "I remember Hal," is written in verse form.

3 Penned literally. Sidney's stationery, and pens, were rough especially during the war, and his handwriting was never very legible. This got worse during the war when he began a private war with U.S. Army mail censors, testing the limits of their vision and patience as he developed a cursive script best characterized as a wavy line with bumps.