Journal of the History of Economic Thought,
Volume 34, Number 4, December 2012

ARROW AND DEBREU DE-HOMOGENIZED

BY

TILL DÜPPE

To this day, the so-called Arrow–Debreu model represents a trademark of rigorous economic research—be it as a benchmark for extending the model, for weakening its assumptions, for structuring data sets, or for providing alternative models. But who should earn the credit? Arrow or Debreu? This essay presents "the making of" Arrow's and Debreu's joint article of 1954 as documented in their extensive letter exchange between their first contact in February 1952 and submission in May 1953. I show, pivotally, that Arrow and Debreu did not share the same interest in their work, that they played different roles, and drew different lessons from it. Moreover, neither Arrow nor Debreu can be identified with the way the profession would later refer to the Arrow–Debreu model. To the contrary, both, in their own ways, sought to counter what others perceived as limitations when placing their hopes in the model.

I. INTRODUCTION

To this day, the so-called Arrow–Debreu model represents a trademark of rigorous economic research—be it as a benchmark for extending the model, for weakening its assumptions, for structuring data sets, or for providing alternative models. Moreover, it is part of the initiation of economists at graduate school, as well as a cornerstone of economists' policy-related activities. Also regarding economists' historical consciousness, the Arrow–Debreu marks a limit: referring to work before Arrow–Debreu tends to be seen as historical rumination, while reference to work after Arrow–Debreu potentially counts as a contribution to economic theory. And so it

Département des sciences économiques, Université du Québec à Montréal (UQAM), 315 St. Catherine St. East, Montréal H3C 3P8, Canada. I gratefully acknowledge the comments I received for previous versions at several conferences and workshops from Roger E. Backhouse, Vivienne Brown, Rodolphe Dos Santos Ferreira, M. Ali Khan, Steven G. Medema, and Joseph M. Ostroy. I thank the Bancroft Library, Berkeley, for their help in guiding me through the Debreu papers. I am particularly grateful to the most valuable correspondence and conversations with E. Roy Weintraub, on whose works the present account is built.

ISSN 1053-8372 print; ISSN 1469-9656 online/12/04000491-514
doi:10.1017/S1053837212000491

seems appropriate that Arrow's and Debreu's article of 1954 is the only piece of work in economics that was worth two Nobel Prizes, one for Arrow in 1972, and another for Debreu in 1983.

But who should earn the credit? Arrow, Debreu, both or neither of them? The purpose of this essay is straightforward: the Arrow–Debreu model cannot be traced to either Arrow or Debreu. I show, pivotally, that they did not share the same interest in proving the existence of a general equilibrium, that they played different roles during the process of their work, and drew different lessons from it. For both, the article of 1954 was a compromise. Moreover neither Arrow nor Debreu can be identified with the way the profession would later refer to the Arrow–Debreu model. To the contrary, both, in their own ways, sought to counter what others perceived as limitations when placing their hopes in the Arrow–Debreu model.

As in other cases of canonical work, there is, compared to the amount of references to the Arrow–Debreu model, little historical work on the original article. There is but one account describing the refereeing process (Weintraub and Gayer 2001). The following account describing the actual writing of the article owes debt to the fortunate fact that Arrow was traveling in Europe during most of the time of their joint efforts, so that the evolution of the article is almost fully documented in their extensive letter exchange between their first contact in February 1952 and submission in May 1953. Having never previously met in person, Arrow and Debreu met only once during this period, in December 1952. Debreu, meticulous as he was, kept double copies of this letter exchange, including notes of their only meeting.[1] In addition, he kept a personal chronology of his scientific career, documenting the order of events of conceiving the proof (DP additional carton 3).

In order to set the stage, I describe the different paths by which Arrow and Debreu arrived at the point of recognizing the opportunity of a joint article (II). In a largely informal fashion, I go through the issues they had to resolve, their negotiations of what to include and exclude, and the compromises they made before agreeing on a final version (III). I then contrast the research their joint work triggered vis-à-vis the increasing popularity of the Arrow–Debreu model in the profession at large (IV).

II. SIMULTANEOUS DISCOVERY?

Arrow and Debreu, both born in 1921, were in their early thirties when they joined forces. As people, this was the only fact they had in common. While Arrow was extroverted, outspoken, quick, highly eclectic, and unafraid of dropping a brick, Debreu was introverted, silent, shy, dogmatic, and afraid of making a mistake. While Arrow thought on his feet and immediately said what came to his mind, Debreu could not walk without a safety net (as Hildenbrand used to tell him), and never spoke up before knowing the definite answer. As a PhD student of Arrow and long-time colleague of Debreu at Berkeley said about them:

[1] All quotes, if not otherwise indicated, are taken from the Debreu Papers at the Bancroft library (DP), Carton 10, Folder "Existence of an Equilibrium in an competitive economy," and Folder "Competitive Equilibrium." Debreu did not keep copies of every single draft. Comments that he or Arrow may have written there are not documented.

> I have visions of conversations consisting of Arrow going on at an extremely rapid pace, and Gerard every once in a while saying something, or coming back with a proof. I can't imagine a dialogue between the two of them. Their styles were just so different. Arrow was going to conjecture about anything. . . . He was unbelievably fast, but open in what his thinking was. Gerard's thinking was never open (Steve Goldman, personal communication).

In 1972, at the AEA lunch in honor of Arrow's Nobel Prize, Debreu gave a toast. He noted "the breadth of your interests and your extraordinary willingness to discuss economic ideas at any stage of their development" (DP 14). In both respects, Debreu could not be more different from Arrow. Their different personalities would not be noteworthy, though, if they had not reflected their role in the process of writing. To begin with, what were the ways that led Arrow and Debreu to their "discovery" of a topological existence proof in general equilibrium theory?

When considering the origin of Arrow's and Debreu's interest in the existence proof, we need some sense of its time. During the early post-war years, the interaction between mathematics and economic theory was continuously renegotiated on various and unstable political grounds. As readers of Mirowski (2002) know, it is rather intricate to disentangle interests in economic theory, excitement about new mathematical tools, obligations towards political positions, and the influence of military funding. As wide as the ramifications of innovations in mathematical economics are, however, so narrow was the community in which they took place. Those who pushed mathematical economics beyond the previously used differential calculus were almost exclusively centered at the Cowles Commission for Research in Economics in Chicago. There, several research programs—Keynesian and Walrasian, statistical and theoretical, applied and pure—were pursued simultaneously with great enthusiasm about techniques and little thought about their impact on the profession at large. Hardly anyone at Cowles in the post-war years had expected that their work would one day move from the periphery into the center of economic research. "How small that group looks in retrospect," Debreu later exclaimed when praising Arrow, "and how difficult it would have been to anticipate that several of your contributions would become standard parts of the graduate economic theory program" (DP 14).

The starting point for the new era of mathematical economics onto which Arrow and Debreu would jump was the Cowles conference on Activity Analysis of Production and Allocation in June 1949 (see Düppe and Weintraub forthcoming). In this conference, all the ingredients of Arrow's and Debreu's article were on the table, though premature to be put together. On the very first page of the introduction to the proceedings, Koopmans (1951, p. 1) mentioned the existence problem as it was discussed, among others, by Wald ([1936] 1951) in Menger's mathematical colloquium, but hardly any contributor picked it up.[2] The focus of the conference was rather on linear programming framed by two theories: Leontief's input–output model as restated by Samuelson, on the one hand; and von Neumann's growth model, on the other ([1936] 1945). If Arrow and Debreu had a simultaneous discovery, it was the

[2]Although Arrow would mention the work done in Menger's colloquium in the final article, and although this work figures prominently in many later historical accounts (as, for example, in Punzo 1991), it was, as we will see, immaterial for the actual writing of the article.

discovery of this article by von Neumann, which had been the talk of the Cowles community since the Activity Analysis conference in June 1949. It was the first existence proof in an economic context using the fixed-point theorem.[3]

The conference established two trends as a prelude to Arrow and Debreu. First and foremost, although much of the research that was referred to had been developed in the context of either actual military or possible socialist planning, the conference initiated the mitigation of the political connotations of mathematical economics. Koopmans introduced activity analysis openly as a contribution to the calculation debate by referring to Mises and Lange (1951, p. 3). Yet, he considered centralized allocation an organizational necessity, rather than a political option. As a result of this de-politicization, the Hicks–Samuelson and the Lange–Lerner type of welfare economics merged. The second historical prerequisite for an existence proof to become important was the preference for convexity analysis in combinatorial topology over differential calculus, which effectively de-crowned Hicks' oeuvre as the standard reference in economic theory. Von Neumann's article, rather than Samuelson's restatement of Leontief, was the benchmark in this respect, particularly for its seminal use of Brouwer's fixed-point theorem. "The connection with topology," von Neumann wrote, "may be surprising at first, but the author thinks that it is natural in problems of this kind" (1945, p. 1). It was this shared commitment to advance rigor in economic theory that later made the participants speak of the beginning of a new era.

Yet, there was one impediment for the takeoff of axiomatic general equilibrium theory at Cowles: von Neumann's aversion against Walrasian competitive analysis, as obvious for all readers of the *Theory of Games* ([1944] 1953, p. 15). In order to freely use the tools developed in the context of this illustrious book, the opposition between strategic and competitive analysis first had to be downplayed. Von Neumann himself made the start: his 1928 proof of the minimax theorem in the context of games and his 1936 growth model in the context of competition were "oddly connected," von Neumann wrote, for being reducible to one another via a saddle point (1945, p. 1). Von Neumann and Morgenstern wondered whether "there may be some deeper formal connections here.... The subject should be clarified further" ([1944] 1953, p. 154). Without anyone ever clarifying further, the equivalence made possible the steady crowding out of game theory as an alternative "paradigm" to competition.

Arrow contributed to the Activity Analysis conference with an article on Leontief's model. He generalized a result of Koopmans regarding substitutability and thereby confirmed the superiority of convexity analysis over calculus (1951a). Before that, between 1946 and 1949, Arrow had been a research associate and then assistant professor at Cowles while pursuing his PhD at Columbia University under Harold Hotelling and his assistant Abraham Wald. With a master's degree in mathematics, Arrow, in 1941, had moved to economics because of his interest in mathematical statistics, all the while still planning to become an actuary. In Hotelling's economics class, he learned of Hicks' *Theory and Value*, and, in retrospect, proclaimed having then conceived a notion of the existence problem: "Somehow, when reading

[3]Though von Neumann was not present at the conference, his spirit was represented by both Princeton mathematicians, such as Ansley Coale, Harold Kuhn, Albert Tucker, and his student David Gale, as well as RAND scholars, such as Armen Alchian and Marshall Wood. For the background of von Neumann's article, see Leonard (2010, p. 65 ff).

Hicks, I got the idea that there was a question whether these solutions exist. I guess I had been exposed to enough mathematics to know that when one has a system of equations one worries about existence (in Feiwel 1987, p. 194). Soon after the war, he learned of Abraham Wald's work on existence, read it in German, but was discouraged when talking to Wald:

> I do remember asking him about them and about possible generalizations (particularly with regard to the production assumptions). He felt the field was very difficult and did not encourage further work ... I did not believe I was the one capable of really improving on the results (Arrow, in Weintraub 1985, p. 96).

Acknowledging the authority of Wald, Arrow went on pursuing other projects offered to him during the immediate post-war years. With Hotelling's support, he became the protégé of the new era. In 1948, he began working for RAND, fostering game theory and operations research. In combination with his interest in uncertainty as well as logic, his work on social choice emerged, which resulted in his PhD thesis (1951b). Arrow's research interests thus covered large parts of the rising scene in mathematical economics, and he had already, before meeting Debreu, secured a major publication and a name in the community. The paper he gave at the Activity Analysis conference in 1949 was his farewell to Cowles before he left for Stanford (see Düppe and Weintraub forthcoming).

After his arrival in Stanford, Arrow returned to welfare economics and began working on an optimality proof using the separation theorem— "An Extension of the Basic Theorems of Classical Welfare Economics" (1951c). This largely literary article was informed by a wide range of problems such as saturation and distribution, and informed by canonical contributions not only of Hicks and Samuelson but also Lange and Lerner. He presented his paper in August 1950 at the Second Berkeley Symposium on Mathematical Statistics and Probability. At the same time in August 1950, Debreu presented a similar article proving optimality by convexity at the Harvard meeting of the Econometric Society— "The Coefficient of Resource Utilization" (1951). With this proof, Debreu made his debut at Cowles shortly after Arrow left. In this largely formal paper, Debreu referred to Koopmans' activity analysis on equal footing with von Neumann's minimax theorem, thus ignoring the different contexts of competitive and strategic analysis. And in an "historical note," he dismissed Pareto for not having established the conditions of an optimum "in spite of lengthy developments," and listed that "gradual improvements [were] brought by Barone, Bergson, Hotelling, Hicks, Lange, Lerner, Allais, Samuelson, and Tintner" (p. 282), though none of them could doff the corset of calculus—half a century of economic theory in vain attempts to be rigorous. Such a remark is telling for Debreu's background. In his Nobel lecture, he would say:

> Kenneth Arrow has told in his Nobel lecture about the path that he followed to the point where it joined mine. The route that led me to our collaboration was somewhat different. After having been influenced at the École Normale Supérieur in the early forties by the axiomatic approach of N. Bourbaki... (Debreu 1984, p. 88).

Henri Cartan, one of the founding members of the Bourbaki group, and Debreu's teacher, engendered Debreu's taste for mathematical purity for all the years to come (see Düppe 2012). In 1945, without clear reasons, Debreu gave up pursuing a career as a mathematician and joined the group around Maurice Allais and François Divisia.

There, he learned of the Walrasian world. But he could neither live out his mathematical taste that Allais, the engineer, could not share, nor could he hope for an academic career, since Allais was not well-regarded by the French. Both predicaments would be resolved at Cowles. For Debreu, arriving in the US was a return to his mathematical training rather than a step towards economics: ''Whereas before I was in a group which felt mathematics went too far and points of rigor were not terribly important, at Cowles I came to think, very quickly, that full understanding of a problem required no compromise whatsoever with rigor'' (in Weintraub 2002, p. 153).

Debreu's first contact with mathematical economics of uncompromising rigor was von Neumann's and Morgenstern's *Theory of Games* (1944), which he read eagerly before arriving at Cowles. Overlooking the anti-Walrasian impulse, it led him to work on the optimality proof via convexity mentioned above (1951). Having arrived at Cowles shortly after Arrow left, with this optimality proof that was similar to Arrow's proof, Debreu's first task was to review Arrow's paper. Debreu acknowledged Arrow's article (1951, p. 282, fn 14), and Arrow in his article thanked Debreu for his comments (1951c, p. 507).

That was their first contact. An existence proof was not yet on their minds. For this, they were missing the key insight that a general equilibrium could be formalized as a fixed point. For Arrow, apart from von Neumann's seminal article, the decisive ingredient missing for this insight was the encounter of Nash's existence proof in n-person games (1951). Nash was using Kakutani's 1941 generalization of von Neumann's theorem, not presented in an economic context. Nash thus did not refer to von Neumann as the pioneer of using the fixed-point theorem—a key catalyst for leaving behind von Neumann's aversion against competitive analysis. For it was then that it occurred to Arrow, as he recalled, that ''after reading first von Neumann, but especially Nash's 1950 paper'' (in Feiwel 1987, p. 194), that he had the following idea: *if* he could describe a competitive equilibrium as a game, he could use Nash for an existence proof. Arrow thus remained true to the game theoretical context of Nash. He wanted to provide a synthesis of the notion of a game and that of competition by adding a so-called ''fictitious player'' choosing the price system. By November 1951, he completed a technical report for Cowles called ''On the Existence of Solutions to the Equations of General Equilibrium under Conditions of Perfect Competition.''[4] In December 1951, Arrow left for Europe on a Social Science Research Council Fellowship.

Debreu, on the other hand, was not struck by Nash. Being trained in the axiomatic method of Bourbaki, he was less interested in the contexts of utilizing fixed-point theorems, but rather in the fact that it was a topological theorem—topology being one of the ''mother-structures'' of Bourbakian mathematics.

> [Nash's article] was important, in a way, via Kakutani's Theorem, the fixed point theorem, which has always remained one of the major mathematical tools, in my opinion. But [the] Nash equilibrium has never played an important role for me professionally (Debreu, in Leonard 1992, p. 9).

[4]This report would not be a Cowles discussion paper, and, unfortunately, is no longer available at the Cowles library.

https://doi.org/10.1017/S1053837212000491 Published online by Cambridge University Press

Debreu's first encounter of the fixed-point theorem could have been von Neumann's and Morgenstern's referring to Kakutani (1944, p. 154). But more encouraging must have been his first office mate at Cowles, Morton Slater, who used Kakutani's theorem, too (1950).[5] And von Neumann's growth model? "The paper by Wald that gave the first proof of existence in the early 1930s did not happen to be important for me. The work of von Neumann on growth turned out to be much more significant since, in particular, it led to Kakutani's theorem" (in Feiwel 1987, p. 249). Von Neumann's 1936 article, as mentioned above, was the only time von Neumann used a general equilibrium framework. But it was not even that which caught Debreu's interest. He approved of it, since it led to Kakutani's generalization without an economic context.[6] Debreu thus never conceived of von Neumann's aversion against standard price theory.

And so Debreu expected that his contribution to economics would be to work with a yet more general version of the fixed-point theorem as advanced by topologists of the day. Next to Israel Herstein and John Milnor, he consulted the most prominent Bourbakists at Chicago: Saunders Mac Lane and André Weil. They helped him with a version of the theorem by Deane Montgomery and Samuel Eilenberg (1946). From Montgomery as well as from Jean-Louis Koszul, another Cartan student and second-generation Bourbakist like Eilenberg, Debreu learned of a yet different version of the theorem by Edward Begle (1950). As early as January 1951, Debreu had his own article on a saddle-point existence theorem without much reference to an economic context (1951b). He notes that his theorem is equally valid for both von Neumann's 1928 and 1936 papers, and is also more general than Kakutani's, since it replaced convexity by contractibility assumptions (*ibid.*, fn 3). This discussion paper would be the basis for his comments he would later give to Arrow's technical report. Of course, Arrow would never reach that height of abstraction.

All in all, though Arrow and Debreu arrived at the proof by different paths, much ground had already been covered before they began their joint work. Arrow suggested speaking of a "simultaneous discovery": "This was essentially an example of two people arriving totally independently at the same solution" (in Feiwel 1987, p. 195). This "discovery" was certainly in the air since von Neumann's pioneering article, but what is important for the present purpose is that, after they had conceived the idea of a fixed-point proof in general equilibrium theory independently, the joint writing of the article would require negotiations only on the surface without confronting their notions of its deeper meaning.

[5]Morton Slater played a decisive role for McKenzie's conceiving the idea of proving existence with a fixed-point theorem. Though both, Debreu and McKenzie, were inspired by the same work of Slater (1950), when McKenzie, during his stay at Cowles, asked Debreu about his research, Debreu, missing the chance for collaboration, refused to share his interests (see Weintraub 2011).

[6]Besides approval, Debreu was proud of having found a mathematical slip in von Neumann's theorem: it was unnecessarily restrictive. "He did not need that powerful tool to prove the theorem that he was after. The separation theorem for convex sets was quite sufficient" (Debreu, in Leonard 1992, p. 3). Though this discovery must have been very satisfying for Debreu, he later would comment reverently: "Thus the main mathematical tool for the proof of existence of a GE owes its origin to an accident" (1998, p. 3).

III. THE MAKING OF ARROW AND DEBREU 1954

How, then, did the article evolve? In a nutshell, in February 1952, Debreu contacted Arrow with comments on his technical report. Arrow suggested collaboration and Debreu wrote the first draft. During this first phase, Arrow was on a research trip in Europe—January and February in Rome, March in Montreaux, April in London, then in Paris, and September in Bergen. In December 1952, they met in Stanford in person before Debreu presented the draft at the meeting of the Econometric Society in Chicago. After final revisions in spring of 1953, Arrow wrote the introduction and the historical note, and the paper was completed by the end of May 1953.[7]

First Phase: February to October 1952

By the end of January 1952, Koopmans called Debreu to his office in order to find out about his current research. Debreu told him of his interest in existence, and Koopmans gave him Arrow's technical report mentioned above. Debreu was pleased. Some days later, on February 5, 1952, he sent Arrow, then in Rome, detailed comments including an outline of his own approach—neatly typed.

> Dear Dr. Arrow: Koopmans last week handed me your remarkable paper "On the existence of Solutions … perfect competition". I have read it thoroughly with great delight and now I take the liberty of sending you a long letter of comments. I hope that the criticism I will occasionally make will not mean, in your eyes, that my admiration for the way in which you overcame the difficulties of this subject is lessened.… I had been working myself intensively on this problem for some time when your paper reached me but I had not yet obtained a complete proof of the existence of equilibrium. After having read your article I easily bridged the last gaps in my work. I will give you, below, a concise account of my line of approach, a little different from yours.

In his comments, Debreu provided meticulous notes for simplifying the proof (for example, by describing technological possibilities not by convex sets, but by convex cones) or how to renounce assumptions (most important, convexity by contractibility). More critically, he noted an actual error regarding the discontinuity of the minimum worth condition when prices are zero. But the major criticism Debreu addressed in this first letter was that he did not approve of Arrow's use of the "fictitious player": "The introduction of the fictitious players $\mathrm{I} + 1 \leqq j \leqq 2\mathrm{I}$ with the use of Kuhn and Tucker's theorem seems artificial to me (and this is probably my most important criticism). The approach I have taken below gets around this."

By using the fictitious player, as noted above, Arrow remained true to Nash's game theoretical context from which he conceived the existence proof. Debreu, to anticipate

[7]During this period, both Arrow and Debreu had parallel projects. Notably in Paris, at the colloquium on econometrics, Arrow presented an article on uncertainty, formulating what came to be known as his contingent commodity approach (1952). He received comments by Debreu's teacher Maurice Allais. Debreu, instead, completed a joint paper with the mathematician Israel Herstein on "Nonnegative Square Matrices" in February 1952 (1953). Inspired by Herstein's and Montgomery's only sidestep in economics (1953), he worked on preference ordering, completed in April 1952 (1954), as well as on an optimum proof, completed in January 1953 (1954). These latter contributions would become important for his *Theory of Value* (1959), but remained immaterial for his work with Arrow.

the result, would not succeed in convincing Arrow. The fictitious player would be mentioned in the article, called "the market participant." It would remain a sore point regarding the interpretation of the article. The fictitious player would be reason enough, notably for game theorists, to interpret Arrow–Debreu as most suitable for a socialist economy (Shubik [1972] 1977). Clearly, Debreu never felt the urge to respond to interpretations such as Shubik's.

A week later, on February 12, Debreu sent a short note, clarifying Arrow's error, and added: "I will finally write a discussion paper about this question and send it to you as soon as it is ready." This paper was the discussion paper 2032: "An Economic Equilibrium Existence Theorem" (1952a). It put his saddle-point proof, including Begle's fixed-point theorem, in an economic context—though he could hardly present an economic motivation for the article. He opens the text by stating:

> Economic theory no longer accepts the once standard implication that if the equilibrium of an economic system can be described by a set of equations whose number matches the number of unknowns, an equilibrium point actually exists. A proof of exacting rigor is now required (1952a, p. 1).

This would be the only motivation for Debreu. Accordingly, he closes the text by showing that his theorem includes that of Arrow, of Nash, of Kakutani, of von Neumann, and of von Neumann and Morgenstern (*ibid*., p. 13). Utilizing Begle's version of the fixed-point theorem, he covered all previous uses of the theorem in economics. This is what Debreu believed to be his contribution.

Debreu's comments and discussion paper must have been a challenge for Arrow. Arrow was not acquainted with the mathematics of fixed points beyond Kakutani, and did not have the privilege of getting extra lessons from André Weil or Saunders Mac Lane. He replied on March 8, 1952, from Montreaux—in barely legible handwriting.

> Dear Debreu. I wish to thank you for your series of letters and the manuscript of your Cowles Commission Discussion paper. I am sorry not to have answered earlier, but you can readily understand that work and travel, with the many experiences of a world new to us, have taken my time. Your major point, that my handling of the function $V_i\ (p)$ is an error, is entirely correct.

Having admitted his error, Arrow found a very similar mistake in Debreu's paper regarding corner solutions: initial endowments (in their notation, $z^o{}_{hi}$) need to be strictly positive (1952a, p. 7, line 10). Debreu immediately added an erratum on March 14. Arrow, however, played down Debreu's mistake for its trivial economic meaning: "The defect is very trivial from an economic point of view, since assuming the existence of labor variables amounts to saying no more than that an individual will work if he has no other source of income." Arrow continued by suggesting a new version of his theorem (his "Lemma 2") without accepting Debreu's more general version of the proof, and added:

> The Theorem just stated is provable in exactly the same way as my Lemma 2. It is, of course, a special case of your theorem, though it has the advantage of avoiding [the] direct hypothesis [of] the continuity of $A_i\ (\bar{a}_i)$, which may be difficult to verify in given situations.

https://doi.org/10.1017/S1053837212000491 Published online by Cambridge University Press

This remark is telling for Arrow's attitude. He insisted on his own less general lemma for the sake of its economic meaning, and, at this point, even its greater verifiability! Arrow aimed at making the model "work." Assumptions too strong would amount to a failure. Debreu hardly thought that way: which assumptions are necessary is mathematically determined, while their meaning is to be assessed ex-post. If they are weak—good for the economist! If they are strong, the proof is valuable for showing the restrictiveness of the model. Proving existence does not make a model work, but assesses the model. "In proving existence," he would say later, "one is not trying to make a statement about the real world, one is trying to evaluate the model" (in Feiwel 1987, p. 243).

Having found an error, and demanding his own version of the theorem, Arrow suggested joining forces:

> In view of the essential overlapping of results between the two of us, I would propose that we prepare a joint publication. The relation between the two approaches needs some clarification.… There should be a still more general function covering both cases, but it may not be worthwhile to investigate. Of course, if you prefer separate publication, it will be perfectly acceptable to me.

How must Debreu have felt? At first, certainly little surprised, since the difference between his approach (via Begle) and Arrow's (via Nash) apparently seemed rather insignificant to Arrow. He also must have felt honored, for Arrow had already established a name in the Cowles community. Since everyone around him had greater experience in economic reasoning than himself, he was advised to seize the opportunity to upgrade his economic profile. But his excitement must have been marred by the fact that his more general proof would not be supported by Arrow. Arrow made clear that he would not share what Debreu considered his genuine achievement—generality.

And so, Debreu accepted the offer on March 14, 1952; however, not without drawing a clear line between what he considered his mathematical achievement and what he would expectedly contribute to Arrow in the league of economists—serving as the article's mathematical engineer. Hence, he asked Arrow to agree on a separate publication of his theorem in a mathematical journal:

> The prospect of working in close collaboration with you on this question is very attractive to me and I thank you for your spontaneous offer to write a joint paper. The first point to settle then is the proposal that Koopmans made in his letter of March 11 (of which I am enclosing a copy) of my publishing a synthesis of my saddle point paper and of Section 1 of Economics 2032 in a mathematical journal. There seems to be a definite advantage in excluding the heavier than usual mathematical content from an article written for economists in an economic journal. Secondly, Tucker wrote to us that the replacement of convexity by contractibility in this kind of question was enough of a straight contribution to mathematics to justify publication in a mathematical journal.

Debreu justified his own publication by arguing for a separation of the mathematics from their joint paper on "economics proper" (*ibid.*). From Debreu's point of view, this separate publication needed an explanation, since he expected that his contribution to the joint article with Arrow would be the same as what he would accomplish in his own publication. From Arrow's point of view, however, there was no conflict whatsoever, since Debreu would hardly use the same economic reference. On March 21, by then in London, Arrow replied with a lengthy handwritten letter:

> my own efforts in this direction, as given in lemma 2, and the theorem I gave in my last letter, are so much more restricted in scope than your very important contribution, that there can be no hesitation on your part in publishing the results in a mathematical journal.

Instead of pondering about the generality of Debreu's axioms, Arrow worried most about a problem regarding the meaning of the axioms: *saturation* and *public goods* as discussed by his promoter, Harold Hotelling:

> I would prefer, if possible, not to assume the impossibility of saturation in any one commodity. Hotelling's argument that bridges or museums should be free rests on the hypothesis that individuals will become saturated with those commodities and will not demand infinitely large quantities at zero price.

Bridges? Museums? Things got even worse for Debreu. In the same letter, Arrow once more suggested another theorem and added:

> This theorem can be proved exactly as my Lemma 2. From the mathematical point of view, of course, there is no reason not to make use of your theorem, but from that of exposition and appeal to what is at best a very limited audience, there may be some advantage in this course. Convex sets and Kakutani's theorem are beginning to be familiar, and a paper such as ours may accelerate the process, but to appeal to still another fixed-point theorem of still greater generality may not serve a useful pedagogical purpose. We should, of course, refer to your more general theorem.

Arrow thus defended his less general version not only for the reason of greater verifiability, but also for social and rhetorical reasons, which had been even more extraneous to Debreu's intellectual values. Debreu immediately replied to Arrow, meanwhile in Paris, on April 2, 1952. He did not engage in Arrow's worries. Regarding the unsatisfying assumption of strictly positive endowments, he simply referred to authority: "I have naturally taken consolation . . . in the fact that von Neumann has to make also a rather restrictive assumption." Instead, Debreu made a spiteful, maybe strategic, suggestion: he went further than Arrow's proposal to keep the mathematics to a minimum, and even suggested skipping the mathematical proof *altogether*:

> I suggest that in our economic paper we state the preliminary mathematical results with convexity only, that we define naturally all the necessary concepts, but that we give no proof. There will be so much to prove anyhow, and it is certainly highly advisable to keep the mathematical details at their minimum. Are you in general favorable to this?

Although this suggestion would set Debreu in the most passive role in the further process of writing—serving as no more than the mathematical proofreader—it was consequential from his point of view: if generality does not count and if economic meaning is everything that counts, why bother with a proof at all? As we know, Debreu would not convince Arrow. The proof would be included. Some readers might speculate if literary economics would still be viable today if Arrow had accepted Debreu's proposal.

Though Arrow did not accept Debreu's proposal, he gave in to using Debreu's more general version of the proof, compared to Kakutani's. Georgescu-Roegen, who later would be in charge of handling the refereeing process, suggested, just like Arrow, "to make the proof more elementary and simpler or to present it as elaborated consequences of other well-known theorems" (in Weintraub 2011, p. 211). Also he referred to Kakutani. Though Debreu would later point to the generality of the proof as *the* distinguishing feature of their paper as compared to other proofs produced at the time (most notably Lionel McKenzie's), in his *Theory of Value* he would introduce the fixed-point theorem by referring to Kakutani.

So far for the first round of negotiations. Debreu planned to write a first draft by May 1952, and, at that point, expected that it could be presented at the September meeting of the Econometric Society in East Lansing. But their work was interrupted. Arrow was busy in Europe, and Debreu suffered from a kidney infection that lasted over a month, before further suffering from the summer heat, as he apologized for the lack of work on July 14. After he received the green light from Arrow, Debreu preferred to first finish the synthesis of his two discussion papers (1951b, 1952a) as it would appear in August: "A Social Equilibrium Existence Theorem" (1952b). This paper is notable in that it presents the equilibrium without referring to competitive or strategic behavior at all. Debreu simply spoke, more generally, of interdependence of behavior. In his conclusion, he made clear what his target discourse was when dealing with economics: he praised Begle's, Montgomery's, and Eilenberg's generalizations of the fixed-point theorem "as valuable contributions to topology whose origin can be traced directly to economics" (1952b, p. 892). While for Arrow, mathematics was useful for economics, for Debreu, economics was useful for mathematics.

Second Phase: December 1952 to May 1953

Robert Strotz, the editor of *Econometrica*, had heard of Arrow's and Debreu's project from Koopmans, and invited them to present their work at the December meeting of the Econometric Society in Chicago. They accepted, and had to agree on a presentable draft. Shortly after Arrow returned from Europe, on Wednesday, December 10, 1952, they finally met at Stanford for their first time in person. After understanding with whom they had gotten involved, they must have grasped that they were working on different intellectual projects. Yet, their work was already so far advanced that it might have been too late to debate the deeper meaning of their proof. Indeed, they did not have much to discuss, as Arrow suggested: "It was a wonderful experience, he was just so brilliant to work with. One of us would say a single word, and the other would just understand immediately" (quoted in Gallagher 2005). No discussion, sure, but immediate understanding? Perhaps Arrow and Debreu immediately understood one another because it was difficult to object to Debreu, who did not have a strong position in economics. Whatever happened, the fact is that Debreu planned to stay for ten days, but left several days earlier.

Debreu presented the paper in Chicago on December 27, 1952. It turned out that someone else was working on the same problem. Lionel McKenzie presented a fixed-point proof in a general equilibrium model in the context of trade on the following conference day (1954). Debreu noted that their paper implied McKenzie's result, and, not without consulting Koopmans before, spoke up about this implication in

McKenzie's session. Debreu must have been impressed, since McKenzie deliberately decided to use Kakutani's theorem while referring to that of Eilenberg and Montgomery (*ibid*., p. 158)—a generality that Debreu could not act out with Arrow. Later, as reported by Weintraub (2011), Debreu would not remember his oral intervention at the conference, and, moreover, he did not inform Arrow about McKenzie's proof. Aware of the similarity, Debreu even accepted to function as a referee for McKenzie's article, submitted prior to his joint paper with Arrow, but refereed later due to the sluggish work of the first referees, John Nash and Leonid Hurwicz. Strategic or not, it was not until late after publication, after Arrow's Nobel Prize, that Debreu, or Arrow, acknowledged McKenzie for his equivalent proof (see Düppe and Weintraub forthcoming).

In the first months of 1953, the making of Arrow's and Debreu's paper went into its final round. Debreu sent Arrow thorough comments on the basis of their meeting (undated, see Arrow's letter on January 1, 1953). The first and most important comment was to remind Arrow of the separation of the mathematics and the economics involved: "We should make a great effort to make clear the logical structure of the theorems and carefully distinguish assumptions [underlined] and conditions [underlined]. It is probably impossible to succeed completely without excessive pedantry." In later accounts, Debreu would present this "careful distinction" as the main contribution of their paper (1984). The fact that he, still at this late stage of the process, had to caution Arrow in separating these two contexts shows how little Arrow obeyed this imperative. An outside referee, Cecil Phipps, would later emphatically criticize the lacking separation of the mathematics and the economics. Arrow's response to this criticism would be (January 13, 1954): "I don't agree at all. I think, on the contrary, the important thing is to display the interdependence of the mathematics and the economics."

Debreu also called for caution when addressing interpretations, in particular if they are contested. At several points he noted: "Deletion: Controversy about the interpretation of a text." When Arrow, for example, wanted to refer to David Wright, Debreu noted:

> Deletions: The main reason is that the fact and the reasonings [sic] do not have the character of certainty and sharpness of the rest of the paper. Moreover I think we should keep away from controversy with Wright about Keynes and forced interpretations of ancient texts.

Why refer to quarrels regarding interpretations, if the axiomatic structure stands without them? Debreu would succeed. Ancient texts by Keynes and Wright would not be mentioned. In the same spirit, Debreu also suggested not to use game-theoretic notions: "I have changed player, strategy to agent, action. It seems desirable to have a terminology different from that of games. Moreover are the words player and even strategy so good?" Metaphorical speech, inviting wrong-headed connotations, ought to be avoided.

Arrow used Debreu's comments in preparing another draft, which was completed by February 5, 1953. The introduction was not yet written. Having gotten to know his co-author, Arrow began the letter by apologizing that he was unnecessarily copious due to relating "abstract ideas to the raw material of economic reality":

> Some of my comments on the assumptions were fairly detailed, but I think they are useful in relating the abstract ideas to the raw material of economic reality. I have probably been pedantic in spelling out details of the proof, instead of leaving them to the reader, please make any changes along those lines that you care to. My work was tremendously simplified by the excellent set of notes that you supplied me with, and I want to thank you for them. . . . I have generalized the formulation of dividends to permit non-proportional payments. This in no way complicates the proof, and it adds to the realism, since we can treat of preferred stock, bonds, and other forms of corporate financing.

Another change for 'adding to the realism' was to use 'excess demand' instead of 'net demand' for the sake of relating their work to the ''law of supply and demand'':

> The most important deviation is my unrepentant feeling that 'excess demand' is a better concept than 'net demand'. It simplifies expressions any number of times and is basic when dealing with the interpretation of market equilibrium in terms of the law of supply and demand. Why don't you take a little poll among the Cowles Commission people?

Arrow clearly considered their work to be a formalization of ''the law of supply and demand.'' There is no reason to believe that Debreu had ever considered this interpretation at all. Arrow's sense for realism was further expressed in his continued concern for the assumption of strictly positive initial endowments: he noted an objection by his colleague from Columbia University, William Vickrey, who argued that the initial endowment might not be enough to survive: ''I suppose one could have equilibrium through non-survival of some consumption unit, but this seems a gruesome solution,'' Arrow wrote with a sense of disappointment. On March 5, 1953, he informed Debreu of his reading of Joan Robinson in this regard.

> In regard to Vickrey's objection that the initial holding may not be sufficient for survival, I have run across an interesting passage in Joan Robinson's ''pure theory of international trade'' in her collected Economic Papers. In reference to the theory of equilibrium in that field she holds that it may very well not be possible at the existing levels of population. That is, the equilibrating process may operate through the death of part of the population. 'The invisible hand works, but it may work by strangulation'.

It was such remarks that Debreu had referred to as ''forced interpretations.''

As it is with negotiations, actual differences are brought forward only at the end. Note that until this point, the paper was not embedded in a discursive context. At no point of their work so far had they discussed their views on the context of the proof. On April 13, 1953, Arrow sent Debreu his version of the introduction including the ''historical note'' (1954, section 6). ''If you want to, expand the introduction in any way. I am not too satisfied with it as it stands, but I just ran out of ideas.'' The article came close to conclusion without Arrow and Debreu ever discussing any of the issues Arrow raised in this introduction and historical remark—that is, the proof's usefulness for ''both descriptive and normative economics'' (p. 265), the tradition going back to Walras, Cassel, Neisser, Stackelberg, Zeuthen, etc., and the relationship between existence and uniqueness of an equilibrium (p. 287 ff.). In his reply (April 23), Debreu showed that he had not considered the necessity of an introduction before, and, expectedly, proposed a deletion: "I think that a short introduction was quite in order. I have even deleted three lines at top of page 2." These three lines must have included the following: "from the point of view of normative economics the problem of existence of an equilibrium for a competitive system is therefore also basic" (1954, p. 265 f.). But what for Debreu was a mere matter of conciseness was essential for Arrow. Arrow insisted vehemently:

> The deletion of the three lines on top of page 2 of the introduction removes the point of the whole paragraph. It was precisely the fact that the necessity and sufficiency of competitive equilibrium of Pareto optimality still left open a loophole in the argument for a price system that led me to study the existence question. I consider the retention of those lines, or at least their meaning, important.

Similarly, concerning welfare economics, Arrow insisted twice on references to standard articles like those of Lange (1942) or Hotelling (1938), but Debreu insisted both times on leaving them out and won out over Arrow (see letters April 17, May 4, May 13). Thus, only at their very end did disagreement regarding the economic meaning of their proof become apparent, particularly as it concerns welfare economics.

In the last revisions that Debreu sent some days before submission, on May 13, 1953, he again tried to minimize the mathematics that he would want to claim for himself. Debreu managed to avoid referencing any other prior use of the fixed-point theorem apart from Nash, von Neumann, and his own. Regarding the reference to Eilenberg, he argues: "Delete reference to Eilenberg. [H]is paper is too sophisticated for economists in general." Clearly, the more mathematics he would manage to exclude from the article with Arrow, the more he could later claim for himself.

The paper was completed on May 20, 1953. They were uncertain whether to send it to Koopmans or to Robert Strotz of *Econometrica* since they knew that Koopmans would not have an internal referee—the Cowles community already agreed upon its worth. They sent it to Koopmans, who forwarded it to Strotz on June 8, 1953. Koopmans explained the regret of not having provided internal referees with the following words: "Needless to say that this does not imply any feeling that we should regard this as an over-specialized study. It is addressed to a classical problem in economic theory and brings to it new mathematical tools." Referring to "a classical problem" and "new mathematical tools" in one breath, this was the first time that Arrow–Debreu arose from Arrow and Debreu.

IV. AFTERMATH

For Cowlesmen, there could not have been any doubts about the worth of Arrow's and Debreu's joint article. But outside Cowles, there were not many economists able to appreciate the mathematics, and not many mathematicians able to appreciate the economics. This predicament led to an actual conflict during the refereeing process, as has been thoroughly described by Weintraub and Gayer (2001). Nicholas Georgescu-Roegen, the associate editor of *Econometrica*, sent the paper to William Baumol and an outside mathematician, Cecil Phipps, who emphatically argued against publication. The negotiations between the Cowles community and *Econometrica* undermined Phipps' voice and thus an actual alternative approach in mathematical economics. But these quarrels were conducted without the authors. Arrow and Debreu did not have to defend their article against this skeptical referee, and no major changes had to be made after the refereeing process.[8] Regarding our present study of the differences between Arrow and Debreu, more revealing are the different impulses their joint work gave to their further work. According to the preceding account, it should come as no surprise that they headed in different directions.

For Debreu, clearly, the work with Arrow delayed what he considered his contribution to mathematical economics. Ever since his first contact with Arrow, he was aiming at a more general existence proof. After trying to keep the publication with Arrow free from allusions to greater generality, all doors were now open for his very own. After a six-month leave in France, he continued searching for a more general proof and, by spring 1954, completed the version that would later appear in his *Theory of Value* (1959). But he was not satisfied, since he considered it still too close to what he had done with Arrow. What he was really looking for was an existence proof renouncing the fixed-point theorem altogether. For this purpose, he again consulted the most prominent Bourbakists on campus: Armand Borel, Pierre Samuel, and, of course, André Weil.

> I obtained the lemma in the form in which it appears in *Theory of Value*, pp. 82–83 in the late spring, or early summer, of 1954. The detailed plan of my monograph then became clear to me, and by the end of the summer 1954 the first four chapters were completed, and available in [typed?] form. At that time I did not seriously consider publishing my result in the form of an article because (1) I believed that my monograph would be finished in a few months, and would presumably appear in 1955, (2) given the papers that had been written before the summer of 1954 on the problem of existence (in particular Arrow–Debreu), the result did not seem particularly deep or original. Be that as it may, I communicated my result to Armand Borel (who was spending the academic year 1954–55 at the University of Chicago in the fall of 1954 in his office in Eckhart Hall), and to André Weil and to P. Samuel (at a lunch at the Weils) in the spring 1955. In both cases, my purpose was to discuss the question whether one could dispense with a fixed point theorem in proving the lemma (DP, additional carton 4).[9]

Alas, that proof dispensing of the fixed-point theorem would never be written. After the Cowles Commission moved to Yale, in fall 1955, Debreu had completed the most general version he had to offer, and sent it to von Neumann for publication in the *National Academy of Science Proceedings*. But von Neumann was too ill to handle it, so Debreu sent it to Marston Morse. Publication was delayed until 1956.

In the intellectual chronology Debreu kept for himself, he was meticulous about the order of these events, since two other mathematicians produced a very similar

[8]It was only after publication, in October 1954, after Phipps sent a second letter to the editor, that Arrow and Debreu were asked to respond.

[9]This note was written later on the request of Robert Aumann, who must have asked Debreu to give an account of the evolution of his proof as presented in his monograph: "Some comments on the history of the Lemma of Debreu–Gale–Nikaido."

proof during the same period: David Gale (1955) and Hukukane Nikaido (1956). Since Debreu's interest in a more general proof dated back to 1950, it was of considerable importance for him to notice when exactly he conceived of what later came to be known as the Debreu–Gale–Nikaido lemma. For this was the second time, after the encounter with McKenzie, that Debreu faced the possibility of losing a claim to the priority of his results.

In Yale, Debreu was mostly devoted to working out his monograph. Again, he was in direct contact with the most prominent mathematicians, most notably Shizuo Kakutani. His colleagues at Cowles, however, hardly shared his passion for advancing purity. All of them showed greater hopes for the expressive future of mathematics in economics: Simon began computer simulations; Marschak moved to information issues with experimental designs; and Patinkin included monetary theory in general equilibrium theory. This difference between him and the rest of Cowles became most apparent in the reviews Debreu received for the *Theory of Value*. All of the seven reviews showed reservations about its purity and noted the regrettable exclusion of monopolies, externalities, and money. Cowles reviewers, in particular, did not hold back their skepticism. Leonid Hurwicz wrote that the book is "unique in its uncompromising devotion to maintain the clarity and rigor of the axiomatic structure even at the expense of other objectives" (1961, p. 416). Martin Shubik was yet more explicit, showing an "uncomfortable feeling that it represents a tidying up of old work and problems which will not necessarily provide a stepping-stone for new work" (1961, p. 133). And, although Debreu had rigorously separated the mathematical context from the economic, Shubik concluded in what would become the tenor of his work's criticism: "economics is not mathematics. Rigor is a necessary but not sufficient condition for a valuable contribution to economic theory" (*ibid.*).

Debreu must have felt misunderstood. The objections were not new to him, and he would not be spared from them for the rest of his career. During the 1960s, he nevertheless managed to build up his own Neo-Walrasian community, not without the support of prominent mathematicians such as Steve Smale. His community was built on two pillars: one was to introduce new mathematical techniques, such as measure theory, and later on non-standard analysis; and another to attract the interest of game theorists by reviving Edgeworth's question of core-convergence. Regarding this program, even Arrow distanced himself from Debreu's work. On February 24, 1971, when Arrow had just completed his rather skeptical review of equilibrium analysis with Frank Hahn (1971), he wrote to Debreu:

> Speaking for myself, I am less and less persuaded that the measure-theoretic approach to the core is the only satisfactory one. . . . The principal problem is something that has bothered me from the beginning, about the meaning of equilibrium or of the core when there is a continuum of traders. Speaking naively, if there are an infinity of traders, endowments are infinite, and it is not easy to know what is meant by equating supply and demand (DP 5).

But Debreu knew all along that the meaning of equilibrium is open for interpretation. Already right after publication, on November 8, 1954, in his response to Cecil Phipps' second letter, Debreu reacted to the objection that the existence proof does not lend itself to empirical analysis. He considered the fact that the proof is empirically underdetermined and, hence, naturally open to several interpretations, not as a weakness but as "its most interesting characteristics":

> I conceive of AD as exhibiting the general and abstract feature of a market economy. It is natural that the model has several possible interpretations and it is in fact one of its most interesting characteristics. For example, I have shown last year how a proper interpretation of the symbols gives a theory of uncertainty without any change in formalism.

Some twenty years later after his work with Arrow, Debreu (1974) indeed proved rigorously that the existence proof really had no strong implications, which came to be known as the Sonnenschein–Mantel–Debreu results. Debreu was hardly surprised, but others continued praising and blaming him for clinging to the opposite view. For these results, as critical as they were, never entered the consciousness of those economists who began referring to Arrow and Debreu as the benchmark of rigorous research. Particularly the increasing popularity of the so-called "microfoundations" in macroeconomics during the 1970s shows how little economists learned to appreciate what Debreu considered the "most interesting characteristics" of his work.

For Arrow, too, Arrow and Debreu 1954 compromised his actual interest. In the introduction, he already made clear that the existence proof is no more than a preliminary exercise "for descriptive and for normative economics" (1954, p. 265). The existence proof was a buttress for pursuing other questions, but had no value in and of itself. This attitude made Arrow the actual Walrasian. For if there was anything that made the 1954 article Walrasian, then, it was the problem it left unresolved: the *tâtonnement*; that is to say, the learning-process by which equilibrium is achieved. This process is essential regarding the intuition of equilibrium analysis, in that equilibrium refers not only to the consistency of a model, but also to equilibrating forces—market forces. Thus, under what conditions is the equilibrium stable?

And so, right after publication, Arrow joined forces with Alain Enthoven (1956), Leonid Hurwicz (1958), and Henry Block (Arrow, Hurwicz, Block 1959) in order to prove stability. This research circled around the difference between local and global stability, introduced the Lyapounov theorem, set in spot the so-called weak axiom of revealed preferences and the strong assumption of gross-substitutability, and ultimately ended in a dead end of systematic counter-examples. In a row of replies to Arrow's stabilizing attempts, counter-example after counter-example frustrated the belief in a rigorous proof of market forces (Scarf 1960; McKenzie 1960; Gale 1963). Debreu had never even posed that question of stability. About Hurwicz and Arrow's work he merely "shook his head. He knew at the outset that this leads to nowhere" (Werner Hildenbrand, personal communication). For Debreu, as mentioned, equilibrium was a matter of the consistency of a model, and not a state of the world: "when you are out of equilibrium," he later explains, "you cannot assume that every commodity has a unique price because that is already an equilibrium determination" (in Weintraub 2002, p. 146). Disequilibrium, for Debreu, is a contradiction in itself, since then prices have no conceivable identity whatsoever.

Aside from his work on stability in direct response to his work with Debreu, Arrow began working in various fields of economic research, applied as well as theoretical. He always remained open for methodological innovations with an increasing awareness of methodological limitations—much the opposite of Debreu's dogmatic commitment to purity in equilibrium analysis. Arrow became an eclectic supporter of mathematical economics as an actual ‘‘applied science.’’ His broad interest led him to work on learning in anticipation of endogenous growth theory, non-linear programming replacing Kuhn–Tucker, as well as work on risk taking and organizational theory. His research always happened to have some connection with RAND. Jointly with Mordecai Kurz, at Stanford, he launched the Institute for Mathematical Studies in Social Sciences as well as the department for Operations Research. In advanced age, Arrow co-organized the 1987 conference on complexity in Santa Fe—a conference that has been celebrated as the end of economists' favor for deduction, which Arrow–Debreu symbolized. Without attempting to review his manifold contributions, Arrow was clearly exceptional in the history of economics, in that he did not have any inhibitions in crossing theoretical and methodological boundaries.

Arrow and Debreu thus reacted rather differently to their joint work. But neither the arcane purity of Debreu nor the applied eclecticism of Arrow represented the channel by which Arrow–Debreu became a benchmark of rigorous research. Without reviewing the complex history of the influence of their paper, let me only identify three sources of increasing popularity. In research, the recognition that the conditions under which a general equilibrium holds is the discursive benchmark of economic theory needed at least until the 1970s to become commonplace in economists' consciousness. This late influence of Arrow and Debreu 1954 is well documented by the amount of references to their article.[10] During the first ten years, there are thirty-six references, most of them in *Econometrica* from colleagues of either Arrow or Debreu. Until Arrow's Nobel Prize in 1972, roughly the same amount referred to the article, though an increasing number with computational use.[11] From then until Debreu's Nobel Prize in 1983, references increased considerably to above eighty—many of them from applied macro fields, such as finance, monetary theory, international trade, and even regional studies. Until today, references have not ceased to increase, though, to be sure, references in *Econometrica* have decreased. Even if we consider the substantial growth of the profession during these decades, the article clearly became a benchmark of research no sooner than the mid-1970s, in areas that neither Arrow nor Debreu considered the target audience of their article.

Another indicator of the success of the article is its use in textbooks. The hypothesis that textbooks lag behind research, and take over results only after they have been established, does not apply to Arrow and Debreu 1954. Already by 1958, their proof had been mentioned by Henderson and Quandt, and was fully presented even before Arrow's Nobel Prize in 1971 (see Weintraub 2001, p. 188). Arrow–Debreu became a benchmark of rigor, more than in research, for its amenability to

[10]Based on data from the Social Science Citation Index and Google Scholar. For similar results, see Oehler 1990.

[11]Notably, during this decade, there already was a reference by an historian of economics (Jaffe 1967). While de-homogenizing Menger, Jevons, and Walras, he contributed to the homogenization of Arrow and Debreu. Historians were quick, quicker than the great share of the profession of economics, in stating the historical importance of the article. Considering the increasing popularity to reconstruct the history of general equilibrium theory among historians during the 1970s and 1980s, one might wonder how much they have contributed to the making of Arrow–Debreu as a trademark of rigorous economics.

raise the entrance barriers into the profession of economics—the highest of which had been built no sooner than 1996 by Mas-Colell et al. Arrow and Debreu did not enter the profession as a result of research, but as a means for reinforcing the borders between disciplines—which explains the persisting gap between teaching and research in economics.

The third, and perhaps most important, source of popularity of Arrow–Debreu was the increasing reference to their model among critics of mathematical economics. After the adoption of Arrow–Debreu in macroeconomics, an entire generation of economists of Keynesian and Austrian literacy found themselves marginalized. They began the, until today, popular complaints regarding the overuse of mathematics, for which Arrow–Debreu became *the* boogeyman. It was Arrow, however, who, due to his eclecticism, could hardly be charged with such criticism, and it was Debreu who knew of them all along.

V. CONCLUSION

The previous exercise has shown that there are at least three Arrows and Debreus: Arrow and Debreu, Arrow and Debreu 1954, and Arrow–Debreu. Regarding the first, I noted their different personalities, different backgrounds, different paths by which they arrived at joining forces, their different roles during the process of writing, and the different lessons they drew from the proof.

Summing up these observations, while Arrow represents the continuity within the Walrasian and Hicksian tradition, for Debreu, nothing what sensibly could be called Walrasian made him interested in an existence proof. While Arrow's interest was prompted by the fact that Pareto optimality "still left open a loophole in the argument for a price system," for Debreu, the existence of an equilibrium and its welfare implications are, even if important parts of his work, two loosely related questions. Accordingly, while Arrow, after completion, immediately turned to more pressing explanatory uses of equilibrium theory, Debreu pursued yet more purity and generality without feeling responsible for economic implications. While, for Arrow, the joint work prompted rather eclectic research of reformulating the grand themes of dynamics and uncertainty in light of theories of knowledge, information, and complexity, for Debreu, the same work prompted a rather dogmatic research program involving the axiomatization of general equilibrium theory. While Arrow opened a channel for a variety of scientists entering economics as an applied science, Debreu opened a channel for mathematicians entering economics without being trained in it.

Arrow and Debreu 1954, therefore, was not the result of an harmonious division of labor between a mathematically inclined economist and an economically inclined mathematician. The making of Arrow and Debreu 1954 was a negotiation between generality and simplicity, on the one hand, at the cost of explanatory and expository efficacy, on the other. Arrow and Debreu, instead of ever confronting their different interests, compromised. Neither of them could really identify with their joint work. Debreu accepted his role as a mathematical proofreader, and Arrow accepted postponing the more urgent issues to later research.

> [T]he final paper is much closer to his than to my version. . . . It is possible that my exposition was a little closer to what economists would understand than what Gerard might have done had I left him to his own devices. . . . I made more of an effort in writing to bring along the mainstream, to explain what the question is, and I was probably the one who suggested the intertemporal interpretation (Arrow, in Feiwel 1987, p. 195 f.).

Without identifying with the final result, Arrow and Debreu could identify even less with the way Arrow–Debreu would be referred to later. Having different notions of their work, none of them would prevail in their reception. When macroeconomists later used Arrow–Debreu for microfoundations, Arrow had already acquired a skeptical attitude towards a satisfying deductive treatment on these issues, and Debreu had already proven mathematically the structural indeterminacy of general equilibrium theory. Aside from their influence upon research in economics, other sources such as teaching and criticism of economic theory stigmatized "Arrow–Debreu." And so the model survives the current methodological innovations away from deductive reasoning. It persists as the bulwark between economics and other disciplines. Perhaps, without the Arrow–Debreu model, economists would soon lose their bearings when demarcating economic research from other disciplines such as psychology and political science, which they otherwise embrace free from fears of losing identity.

Arrow and Debreu can be credited or blamed for Arrow–Debreu only to the extent that they did not explicitly *prevent* the use of their work. This objection cannot be placed on Arrow, who was quite explicit regarding the limits of general equilibrium theory. But it weighed heavily on Debreu. He assumed fallaciously that by never explicitly supporting a specific interpretation or use of his work, economists would be equally careful in using his work as scientific authority. But is this fallacious belief not part of the very nature of formal expressions?

Arrow and Debreu remained friends for the rest of their days without ever joining forces in academic writing again. They helped each other's and their students' careers, and played against each other as coaches of the faculty's football team, of Berkeley and Stanford, respectively. As late as 1995, they once more embarked on a joint project. They planned to edit the Elgar general equilibrium theory compendium. It was a difficult task. Arrow wanted to include welfare economics and social choice, Debreu did not. They had difficulties agreeing on what articles count as "landmark papers in general equilibrium theory." Debreu did not want to decide by himself, but conducted a survey of what his academic colleagues considered "landmark." Years passed. On December 24, 1999, Arrow wrote to Debreu: "Collaborating with you again would be a great pleasure. But I feel I have spent as much time on this project as I care to, I hope we find another occasion" (DP 6). Yet, once more in spite of their disagreement, their joined work was published: "Landmark Papers In General Equilibrium Theory, Social Choice and Welfare" (2001).

REFERENCES

Arrow, Kenneth J. 1951a. "Alternative Proof of the Substitution Theorem of Leontief Models in the General Case." In T. Koopmans, ed., *Activity Analysis of Production and Allocation*. New York: John Wiley, pp. 155–164.

Arrow, Kenneth J. 1951b. *Social Choice and Individual Values*. New York: John Wiley.

Arrow, Kenneth J. 1951c. "An Extension of the Basic Theorems of Classical Welfare Economics." In J. Neyman, ed., *Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability*. Berkeley and Los Angeles: University of California Press, pp. 507–532.

Arrow, Kenneth J. 1952. "Le role des valeurs boursières pour la répartition la meilleure des risques." *International Colloquium on Econometrics*. Paris: CNRS, pp. 1–8.

Arrow, Kenneth, and Gerard Debreu. 1954. "Existence of an Equilibrium for a Competitive Economy." *Econometrica* 22 (3): 265–290.

Arrow, Kenneth, and Gerard Debreu, eds. 2001. *Companion to General Equilibrium. Landmark Papers in General Equilibrium Theory, Social Choice and Welfare*. Cheltenham: Edward Elgar.

Arrow, Kenneth, and Alain Enthoven. 1956. "A Theorem on Expectations and the Stability of Equilibrium." *Econometrica* 24 (3): 288–293.

Arrow, Kenneth, and Leonid Hurwicz. 1958. "On the Stability of Competitive Equilibrium I." *Econometrica* 26: 522–552.

Arrow, Kenneth, H. D. Block, and Leonid Hurwicz. 1959. "On the Stability of the Competitive Equilibrium, II." *Econometrica* 27 (1): 82–109.

Arrow, Kenneth, and Frank Hahn. 1971. *General Competitive Analysis*. Amsterdam: Elsevier.

Begle, Edward G. 1950. "A Fixed Point Theorem." *The Annals of Mathematics* 51 (3): 544–550.

Debreu, Gerard. Gerard Debreu Papers [DP], Carton 1–14, additional carton 1–4, BANC MSS 2006/218. The Bancroft Library, University of California, Berkeley.

Debreu, Gerard. 1951a. "The Coefficient of Resource Utilization." *Econometrica* 19 (3): 273–292.

Debreu, Gerard. 1951b. "Saddle Point Existence Theorems." CCDP Mathematics 412.

Debreu, Gerard. 1952a. "An Economic Equilibrium Existence Theorem." CCDP Economics 2032.

Debreu, Gerard. 1952b. "A Social Equilibrium Existence Theorem." *Proceedings of the National Academy of Sciences* 38 (8): 886–893.

Debreu, Gerard. 1953. "The Continuity of Multivalued Functions." CCDP Economics 2079.

Debreu, Gerard. 1956. "Market Equilibrium." *Proceedings of the National Academy of Sciences* 42 (11): 876–878.

Debreu, Gerard. 1959. *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. New York: Wiley.

Debreu, Gerard. 1983. "Autobiography." In Wilhelm Odelberg, ed., *Les Prix Nobel: The Nobel Prizes 1983*. Nobel Foundation: Stockholm.

Debreu, Gerard. 1984. "Economic Theory in the Mathematical Mode." *The American Economic Review* 74 (3): 267–278.

Debreu, Gerard. 1998. "Foreword: Economics in a Mathematics Colloquium." In E. Dierker, K. Siegmund, and K. Menger, eds., *Ergebnisse eines Mathematischen Kolloquiums*. Wien: Springer, pp. 1–4.

Debreu, Gerard, and Israel N. Herstein. 1953. "Nonnegative Square Matrices." *Econometrica* 21 (4): 597–607.

Düppe, Till. 2012. "Gerard Debreu's Secrecy: His Life in Order and Silence." *History of Political Economy* 44 (3): 413–449.

Düppe, Till, and E. Roy Weintraub. Forthcoming. *Finding Equilibrium*. Princeton: Princeton University Press.

Eilenberg, Samuel, and Deane Montgomery. 1946. "Fixed Point Theorems for Multi-Valued Transformations." *American Journal of Mathematics* 68 (2): 214–222.

Feiwel George, R., ed. 1987. *Arrow and the Ascent of Modern Economic Theory*. New York: New York University Press.

Gale, David. 1955. "The Law of Supply and Demand." *Mathematica Scandinavica* 3: 33–44.

Gale, David. 1963. "A Note on Global Instability of Competitive Equilibrium." *Naval Research Logistics Quarterly* 10: 81–87.

Gallagher, Noel. 2005. ‘‘Gerard Debreu Dies at 83: First of Four Berkeley Economists to Win Nobel Prize over 18-Year Span.’’ *UC Berkeley Public Affairs.*

Herstein, Israel N., and John Milnor. 1953. ‘‘An Axiomatic Approach to Measurable Utility.’’ *Econometrica* 21 (2): 291–297.

Hotelling, Harold. 1938. ‘‘The General Welfare in Relation to Problems of Taxation and of Railway and Utility Rates.’’ *Econometrica* 6 (3): 242–269.

Hurwicz, Leonid. 1961. ‘‘Review of *Theory of Value*.’’ *American Economic Review* 51 (3): 414–417.

Jaffe, William. 1967. ‘‘Walras Theory of *Tâtonnement*: Critique of Recent Interpretations.’’ *Journal of Political Economy* 75 (1): 1–19.

Kakutani, Shizuo. 1951. ‘‘A Generalization of Brouwer’s Fixed Point Theorem.’’ *Duke Mathematical Journal* 8 (3): 457–459.

Koopmans, Tjalling, ed. 1951. *Activity Analysis of Production and Allocation.* Cowles Commission Monograph 13. New York: John Wiley.

Lange, Oscar. 1942. ‘‘The Foundations of Welfare Economics.’’ *Econometrica* 10 (3/4): 215–228.

Leonard, R. 1992. Interview with Gerard Debreu, April 15, Evans Hall, Berkeley. Debreu Papers, Carton 4.

Leonard, Robert. 2010. *Von Neumann, Morgenstern, and the Creation of Game Theory: From Chess to Social Science, 1900–1960*. Cambridge: Cambridge University Press.

McKenzie, Lionel. 1954. ‘‘On Equilibrium in Graham’s Model of World Trade and Other Competitive Systems.’’ *Econometrica* 22: 147–161.

McKenzie, Lionel. 1960. ‘‘Stability of Equilibrium and the Value of Positive Excess Demand.’’ *Econometrica* 28 (3): 606–617.

Mirowski, Philip. 2002. *Machine Dreams: Economics Becomes a Cyborg Science*. Cambridge: Cambridge University Press.

Nash, John F. 1950. ‘‘Equilibrium Points in N-person Games.’’ *Proceedings of the National Academy of Sciences* 36: 48–49.

Neumann, John von. 1928. „Zur Theorie der Gesellschaftspiele.’’ *Mathematische Annalen* 100: 295–320. Translated 1959, ‘‘On the Theory of Games of Strategy.’’ In Luce and Tucker, eds., *Contributions to the Theory of Games*, IV. Princeton: Princeton University Press, pp. 13–42.

Neumann, John von. 1936.‘‘Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes.’’ In Karl Menger, ed., *Ergebnisse eines Mathematischen Kolloquiums* 8: 73–83. Translated 1945–46, ‘‘A Model of General Economic Equilibrium.’’ *Review of Economic Studies* 13: 1–9.

Neumann, John von, and Oskar Morgenstern. [1944] 1953. *Theory of Games and Economic Behavior.* Princeton: Princeton University Press.

Nikaido, Hukukane. 1956. ‘‘On the Classical Multilateral Exchange Problem.’’ *Metroeconomica* 8: 135–145.

Oehler, K. 1990. ‘‘Speaking Axiomatically: Citation Patterns to Early Articles in General Equilibrium Theory.’’ *History of Political Economy* 22 (1): 101–112.

Punzo, Lionello. 1991. ‘‘The School of Mathematical Formalism and the Viennese Circle of Mathematical Economics.’’ *Journal of the History of Economic Thought* 13: 1–18.

Scarf, Herbert. 1960. ‘‘Some Examples of Global Instability of Competitive Equilibrium.’’ *International Economic Review* 1 (3): 157–172.

Shubik, Martin. 1961. ‘‘Review of *Theory of Value*.’’ *The Canadian Journal of Economics and Political Science* 27 (1): 133.

Shubik, Martin. [1972] 1977. ‘‘Competitive and Controlled Price Economies: The Arrow–Debreu Model Revisited.’’ In G. Schwodiauer, ed., *Equilibrium and Disequilibrium in Economic Theory.* Dordrecht: D. Reidel, pp. 213–224.

Slater, Morton L. 1950. ‘‘Lagrange Multipliers Revisited.’’ CCDP Mathematics 403.

Wald, Abraham. [1936] 1951. ‘‘On Some Systems of Equations of Mathematical Economics.’’ *Econometrica* 19 (4): 368–403. Translated from ‘‘Über einige Gleichungssysteme der mathematischen Ökonomie.’’ *Zeitschrift für Nationalökonomie* 7 (5): 637–670.

Weintraub, E. Roy. 2002. *How Economics Became a Mathematical Science*. Durham and London: Duke University Press.

Weintraub, E. Roy, and Ted Gayer. 2001. ‘‘Equilibrium Proofmaking.’’ *Journal of the History of Economic Thought* 23 (4): 421–442.

Interviews

Goldman, Steve. Monday, September 14, 2009. UC Berkeley.

Hildenbrand, Werner. Tuesday, March 23, 2010. University of Bonn.