Arrow and Debreu de-homogenized

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Abstract: The so-called Arrow-Debreu model, until today, 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 earns credit or blame? 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 its submission in May 1953. I show, first, that Arrow and Debreu did not share the same interest in their work, that they played different roles, and drew different lessons from it. Having argued that, neither Arrow nor Debreu could identify 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.

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(1) Introduction

The so-called Arrow-Debreu model, until today, 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. Regarding economists' historical consciousness, Arrow-Debreu marks a lower 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 seems appropriate that Arrow 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 earns credit? Arrow, Debreu, both or neither of them? The purpose of this essay is straightforward: the pervasive character of 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 work, and drew different lessons from it. For both, the article of 1954 was a compromise. Having argued that, neither Arrow nor Debreu could identify 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 actual historical work on the original article. There is but one historical account describing the refereeing process (Weintraub and Gayer 2001). The following account of the making of the article owes debt to the fortunate fact that Arrow was travelling in Europe during most of the time of their joint efforts, so that the evolution of the article is almost completely documented in their extensive letter exchange between their first contact in February 1952 and its submission in May 1953. Having never previously met in person, Arrow and Debreu only met once during this period in December 1952. More fortunately, Debreu, meticulous as he was, kept double copies of this letter exchange, including notes of their only meeting.[1] In addition, Debreu 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 (2). 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 (3). 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 (4).

(2) Simultaneous Discovery?

Arrow and Debreu, both born in 1921, were in their early 30s when they joined forces. As people, this was the only fact they had in common. If one reverses their intelligence, one could compare them with Laurel and Hardy. 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 communicating:

> 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)

[1] All quotes, if not otherwise indicated are taken from the Debreu papers at the Bancroft library, 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.

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 developments." (DPC 14) In both respects, Debreu could not be more different than Arrow. This difference would not be noteworthy if it had not reflected their role in the process of writing. To begin with, what were the different ways that led Arrow and Debreu to their "discovery" of a topological existence proof in general equilibrium theory (GET)?

When considering the origin of Arrow's and Debreu's interest in an existence proof, we need some sense of its time. During the early post-war years, the interaction between mathematics and economic theory was continuously re-negotiated on various and unstable political grounds. It is most intricate to disentangle interests in economic theory, excitement about new mathematical tools, obligations towards political positions, and the influence of the military funding scene. As wide as these ramifications of innovations in mathematical economics are, however, so tight was the community in which it took place. Those who pushed mathematical economics beyond calculus (which was the mathematics previously known among economists) were almost exclusively centered at the Cowles Commission in Chicago. There, several research programs (Keynesian and Walrasian, statistical and theoretical, applied and pure) were pursued next to each other with great enthusiasm about techniques and little pondering about their impact on the profession at large. Hardly anyone at Cowles in the post-war years had expected that their group would once move from the periphery into the centre 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." (DPC 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* at the end of June 1949. The conference initiated a shift towards theory at Cowles as represented by the new Director of Research and editor of the proceedings, Tjalling Koopmans (1951). In this conference, all of the ingredients of Arrow and Debreu's article were on the table, though premature to be put together. Koopmans mentioned the existence problem as discussed in Menger's colloquium right from the onset, but no contributor picked it up. The focus 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). If Arrow and Debreu had a simultaneous discovery, it was the discovery of this article of von Neumann, which had been the talk of the Cowles community since its recovery at this conference in 1949.

The conference established two trends as a prelude to Arrow and Debreu: first, 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 techniques. Koopmans introduced activity analysis openly as a contribution to the calculation debate by referring to Mises and Lange. Yet, he considered centralized allocation an organizational necessity rather than a political option. As a result, the Hicks-Samuelson, and the Lange-Lerner type of welfare economics merged. Another decisive precondition of Arrow and Debreu was the preference for convexity analysis in combinatorial topology over calculus, explicit in von Neumann's article when using 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 [1936]: 1) Kuhn and Tucker, for example, supported this preference by elaborating von Neumann's minimax theorem – that is, using topology in the context of strategic rather than competitive behavior. As a result, the different intuitions between game theory and perfect competition analysis merged for being amenable to the same topological methods. Both trends were vital for the take-off of axiomatic GET at Cowles.

Arrow contributed to the 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 has been Research Associate then Assistant Professor at Cowles while at the same time doing his PhD in the economics department at Columbia University with Harold Hotelling and Abraham Wald. With a master's degree in mathematics, Arrow had moved to economics because of his interest in mathematical statistics. Since mid 1948, Arrow was also working for RAND, fostering game theory and operations research. In combination with his interest in uncertainty, 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 economics. The paper he gave at the activity analysis conference in summer of 1949 was his farewell to Cowles before he left for Stanford.

Having learned his economics from Hicks, and having acquired the taste for convexity analysis from Cowles, Arrow then began working on an optimality proof using the separation theorem – "An Extension of the Basic Theorems of Classical Welfare Economics" (1951c). This largely literary paper was informed by a wide range of problems including saturation, Hicks-Kuznets distribution, and Lange's notion of welfare. Understanding the welfare implications of the Hicks-Samuelson type of GET, with an interest inflamed by the Lange-Lerner type of market socialism, was and would be Arrow's primary interest in GET. 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 research paper, Debreu had made his debut at Cowles in the summer of 1950 shortly after Arrow left. One of his first tasks was to review Arrow's paper on the same topic. Debreu had arrived on U.S ground in the summer of 1949 when taking on a Rockefeller year that he spent mostly at Harvard. At a short visit at Cowles in October 1949, he must have made a considerable impression since he was offered a position as research associate. Though Arrow and Debreu moved in the same circles, they had not met each other.

Debreu had a rather different background than Arrow. 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: 88)

Henri Cartan, one of the founding members of the Bourbaki group and Debreu's teacher, stamped Debreu's intellectual values for all years to come. In 1945, without clear reasons, Debreu gave up pursuing a career as a mathematician and joined the group around Maurice Allais and Francoise Divisia. There, he learned of the Walrasian world. But he could neither live out his mathematical taste that Allais, the engineer, did not support, nor could he hope for an academic career, since Allais was not supported by the French. Both would change at Cowles. Arriving in the U.S. was less of a step towards economics, but a return to his mathematical training.

> 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: 153).

Debreu's first contact with mathematical economics of "uncompromising rigor" was von Neumann's and Morgenstern's *Theory of Games* (1944), which he was reading eagerly during his Rockefeller year. Overlooking the anti-Walrasian impulse of this lustrous book, it led him to work on the optimality proof via convexity mentioned above (1951). In this largely formal paper, Debreu referred to Koopmans' activity analysis on equal footing with von Neumann's minimax theorem. In a "historical note" regarding calculus, 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," (282) though none of them could doff the corset of calculus – half a century of economic theory in vain attempts to be rigorous.

Debreu acknowledged Arrow's article (1951: 282, footnote 14), and Arrow in his article thanked Debreu for comments (1951c: 507).

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That was their first contact. An existence proof was not yet on their minds. For this, they were missing, first, a conception of the existence problem, and, second, the key insight that an equilibrium could be formalized as a fixed-point. A handful of articles were crucial for both elements: von Neumann (1936), Kakutani (1941), Wald (1951 [1936]), and Nash (1951). Regarding the existence problem, in hindsight, mathematical economists and their historians habitually refer to Menger's mathematical colloquium in Vienna during the early 1930s. There, the mathematician Schlesinger in a talk in 1934 and Wald in a series of articles touched on the problem while discussing Cassel's adaptation of Walras, in particular issues regarding commodities of zero price, and the critique of Stackelberg and Neisser that there may be negative prices. Although Arrow would mention Wald, Walras, and Cassel in the introduction, and the other names in the historical note, their work was immaterial for the actual making of their article.

More important was the use of combinatorial topology in economics, in particular the fix-point theorem of Brouwer going back to 1912. Two of von Neumann's papers were pioneering in this respect: his 1928 proof of the minimax theorem and his growth model in general equilibrium in 1936 mentioned above. While the former gave no reference to an economic tradition, the latter merely referred to a "typical economic equation system" (1). While the former modeled strategic behavior, the latter modeled perfect competition – which, in the *Theory of Games* was expressively opposed to each other (1953 [1944]: 15). And yet the two theorems were "oddly connected," von Neumann wrote, for being reducible to one another via a saddle point (1945 [1936]: 1). Von Neumann and Morgenstern wondered whether "there may be some deeper formal connections here (…). The subject should be clarified further." (1953 [1944]: 154) Without anyone ever clarifying further, the equivalence made possible the steady crowding out of game theory as an alternative "paradigm" to competition.

Kakutani (1941) was a key catalyst since he generalized von Neumann's fixed point theorem without reference to any economic context. Morgenstern contributed his share when mentioning von Neumann's article together with Wald when reviewing Hicks's *Value and Capital* (1941). Subsequently, when Nash later proved the equilibrium in n-person games by using the fixed point theorem, he only mentioned Kakutani and not von Neumann and thanked David Gale for suggesting Kakutani – who in turn would contribute to competitive analysis. Von Neumann himself neither pursued further competitive analysis, nor (indirect) topological fixed point proofs. In the *Theory of Games*, von Neumann and Morgenstern were referring to Kakutani, however pejoratively, as "non-elementary" (154) since a fixed point would not amount to a constructive proof and was thus limited for explanatory purposes.

How, then, did Arrow conceive the idea of proving existence by a fixed point? During his graduate studies in 1942 at Columbia, Arrow was reading Hicks:

> Somehow, when reading 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: 194)

After the war, he learned of Abraham Wald's work on existence and read it in German before being translated in 1951. He talked to Wald, but was discouraged:

> 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. (in Weintraub 1985: 96)

Arrow accepted Wald as a mathematical authority. But then, "after reading first von Neumann", he says, "but especially Nash's 1950 paper," (in Feiwel 1987: 194) 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 "artificial player" who chooses prices and marginal utilities. 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." This paper would not be a Cowles discussion paper, and, unfortunately, is no longer available at the Cowles library. In December 1951, Arrow left for Europe on a *Social Science Research Council Fellowship*.

Debreu, instead, was not struck by Nash, let alone Wald. Being trained in the axiomatic method of Bourbaki, Debreu 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. His first encounter of the fixed-point theorem could have been von Neumann and Morgenstern referring to Kakutani (1944: 154). But more encouraging must have been his first office mate at

Cowles, Morton Slater, who used Kakutani's theorem, too (1950). And von Neumann's article?

> 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: 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 any economic context. Besides approval, Debreu was proud having found, if not an error, but a mathematical slip: von Neumann's theorem was unnecessarily restrictive for his purpose. "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: 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: 3)

Debreu thus never conceived of von Neumann's aversion against standard price theory. For the same reason he was not taken aback by reading Nash:

> It 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: 9)

And so Debreu expected 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 Andre Weil. They helped him with a version of the theorem by Deane Montgomery and Samuel Eilenberg; Montgomery told Debreu about a yet different version of 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 it is equally valid for both von Neumann's 1928 and 1936 paper, and is also more general than Kakutani since it replaced convexity by contractibility assumptions. This discussion paper would be the basis for his comments he would later give to Arrow's technical report. 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: 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 GET independently, the making of Arrow and Debreu would only require negotiations on the surface without confronting their notions of its deeper meaning.

(3) 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. In December 1952, they met in person before Debreu presented the draft at the meeting of the Econometric Society. 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.

First phase (February to October 1952)

Between February and October of 1952, 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.[2]

[2] 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.

By the end of January 1952, Koopmans asked Debreu about his work. 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 hints of how to simplify the proof (as e.g. by describing technological possibilities not by a convex set, 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 $I + 1 \leq j \leq 2I$ 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 wanted to remain true to Nash's game theoretical context from which he conceived the existence proof. Debreu, to anticipate the result, would not succeed in convincing Arrow. The fictitious player would be mentioned in the article, but omitted in Debreu's *Theory of Value*. It would

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Debreu, instead, completed , a joint paper with the mathematician Israel Herstein on "Nonnegative Square Matrices" in February 1952 (1953); he also worked on the "real representation of a preference ordering", completed as a working paper in April 1952 (1954).

remain a sore point regarding the interpretation of the article. The fictitious player not only could be associated with the Walrasian auctioneer (a notion that has put forth the question how one reaches an equilibrium), but would be reason enough notably for game theorists to interpret Arrow-Debreu as most suitable for a socialist economy (Shubik 1977 [1972]). Clearly, Debreu had 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 points 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: 1)

This would be the only motivation for Debreu. Accordingly, he closes his article by showing that his theorem includes that of Arrow, of Nash, of Kakutani, of von Neumann, and of von Neumann and Morgenstern. Utilizing Begle's version of the fixed point theorem, he covered all previous uses in any economic context. This is what Debreu believed to be his contribution.

Debreu's comments and discussion paper were a challenge for Arrow, to say the least. Arrow was not acquainted with the mathematics of fixed points beyond Kakutani, and had not the privilege of getting extra lessons from Andre Weil or Saunders Mac Lane. He replied at March 8, 1952, from Montreaux – in poor 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 the error, Arrow also noted a similar mistake in Debreu's paper regarding corner solutions: initial endowments (in their notation, z^{o}_{hi}) need to be strictly positive (1952a: 7, line 10). Debreu immediately added an Erratum on March 14. Arrow, however, vilified 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 direct hypothesis [?] the continuity of A_i ($\bar{a}_i$), which may be difficult to verify in given situations.

This remark is telling for Arrow's attitude. He insisted on his own less general Lemma for the sake of it 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: 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.

Debreu must have felt little surprised, since the difference between his approach (via Begle) and Arrows (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 in his surrounding 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 expectably contribute to Arrow in the league of economists – serving as the mathematical engineer of the article. 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 a separate publication by arguing for a *separation* of the mathematics from their joint paper on "economics proper" (Ibid). From Debreu's point of view, a separate publication needed justification since his contribution to their joint work would not exceed the value of 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, again badly 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.

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 epistemic reason of greater verifiability but also for social and rhetorical reasons, which had been even more extraneous to Debreu's intellectual values.

Arrow showed to worry most about a problem regarding the meaning of axioms: *saturation* and *public goods*:

> 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? 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." Then, Debreu made a surprising, if not spiteful suggestion: he went further than Arrow's proposal to keep the mathematics low, 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 proof-reader – it was consequential from his point of view: if generality does not count at all and if economic meaning is everything that counts, why bother with a proof at all? But Debreu would not convince Arrow. In the end, the proof would be included. One may wonder if literary economics would still be an option today if Arrow had accepted Debreu's proposal.

So far for the first round of negotiations. Debreu planned to write a first draft by May, 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 conclude the synthesis of his two discussion papers (1951b, 1952a) as it would appear by 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: 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's return 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. Indeed, they had not much to debate, 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." (in Gallagher 2005) No discussion, sure, but immediate understanding? Arrow and Debreu immediately understood one another because it was difficult to object to Debreu, who did not have a strong position in economics. And so, though Debreu planned to stay for 10 days, he left days before.

Debreu presented the paper in Chicago on December, 27, 1952. It turned out that someone else was working on the same problem. McKenzie presented a fixed point proof in a general equilibrium model in the context of trade the day after (1954). Debreu noted that their paper implied McKenzies result, and, not without consulting Koopmans before, spoke up about this implication in McKenzies session. Debreu must have been impressed, since McKenzie even referred to the fixed-point theorem of Eilenberg and Montgomery – a generality that Debreu could not act out with Arrow. But it was not until late after publication that Debreu, or Arrow, acknowledged McKenzie for his equivalent proof.

In the first months of 1953, the making of Arrow and Debreu's paper went into its final round. Already before the presentation, Debreu sent Arrow thorough comments on their meeting (undated, see Arrow's letter on January 1, 1953). The first and most important comment of Debreu was to remind Arrow of the rigorous separation of the mathematics and the economics:

> 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.

Accordingly, Debreu called for caution when addressing interpretations, in particular if they are contested. At several points he required: “Deletion: Controversy about the interpretation of a text.” When Arrow, for example, wanted to refer to 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 of 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 for preparing another draft completed by February 5, 1953. The introduction was not yet written. Being cautioned for simplicity, Arrow began the letter by apologizing that he was unnecessarily copious due to relating their “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 seemingly assumed that their work was 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 still struggled with the assumption of strictly positive initial endowments: he noted an objection by 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 unity, 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 discursively embedded by any means. 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, above all, the proof's usefulness for "both descriptive and normative economics" (265), the tradition going back to Walras, Cassel, Neisser, Stackelberg, Zeuthen, etc., and the relationship between existence and uniqueness of an equilibrium (287 ff.).

In his reply (April 23), Debreu showed that he had not considered the necessity of an introduction before: "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: 265 f.) But what for Debreu was a mere matter of conciseness was essential to Arrow. Arrow replied:

> 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 reference to standard articles like those of Lange (1942) or Hotelling (1938), but Debreu insisted twice on leaving them out and won out over Arrow (see letters April 17, May 4, May 13). Thus, only at their very end, there were some signs of disagreement regarding the economic meaning of their proof, particularly as concerns welfare economics.

In the last revisions that Debreu sent some days before submission, on May 13, 1953, he again pushed off the mathematics that he would want to claim for himself. Debreu managed to avoid even referencing Kakutani in the final version. Regarding reference to Eilenberg he argues:

> Delete reference to Eilenberg. 1) his paper is too sophisticated for economists in general 2) even worse the result I mentioned to you in December is never stated explicitly in his article. 3) Still more important, none of his results would cover the case you quote (x_i is

> not supposed to be connected) whereas a new result I have made (better than his in this respect) covers it. As for credit, my paper shall normally give full reference.

This "new result" was his working paper "The Continuity of Multivalued Functions in Economics" completed in May 1953. Debreu thus had already begun working on an alternative proof during the last phase of their joint work. 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, regretful of not having provided internal referees:

> 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 bring to it new mathematical tools.

Referring to "a classical problem" and "new mathematical tools" in one breath, this was the first time that out of Arrow and Debreu arose Arrow-Debreu.

(4) The Becoming of Arrow-Debreu

For Cowlesmen there could not have been any doubts about the worth of Arrow 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 outsider 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 not taken to the authors themselves. Arrow and Debreu had not to defend their paper against outsiders, and they hardly accepted any changes from the refereeing process.[3] Regarding our present purpose of showing their difference, more revealing are the different impulses their joint work set for each of them. According to our account, it comes as no surprise that they headed in different directions.[4]

For Debreu, clearly, the work with Arrow delayed what he himself considered his contribution to mathematical economics. Since his first contact with Arrow, he was waiting to claim his own more general proof of existence. After he managed to keep the publication with Arrow free from allusions to it (there was not even a direct reference to Kakutani) all doors were now open for his own proof. After a six-month leave in France, he worked on putting his version of the fixed-point theorem (1951b) in the equilibrium framework designed with Arrow. By spring 1954, he completed a full proof as it would later appear in his *Theory of Value* (1959). But he was not satisfied since he considered it still too close to what he did 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 and Andre 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

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[3] One of the few remarks that Arrow and Debreu discussed but rejected was Georgescu-Roegen's suggestion of mentioning Leontief. On January, 27, 1954, Arrow wrote to Debreu arguing that Leontief does not fit in since he worked "with a peculiarly simplified consumption structure so that prices become essentially irrelevant; further, Leontief himself did not introduce the non-negativity conditions which are essential. This is really getting into the area of your competence."

[4] Arrow and Debreu shared an interest in uncertainty. Debreu spent the summer and fall of 1953 at Électricité de France in Paris, and worked with his friends Pierre Masse and Edmond Malinvaud: "The theoretical article on contingent commodities that Arrow published in that year [1952] and the applied problems created for Électricité de France by the uncertain amounts of water in hydroelectric plant reservoirs led me to the study of economic uncertainty that was eventually published as the last chapter of my monograph, *Theory of Value*, 1959." (1983)

> 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 Andre 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).[5]

Alas, that proof dispensing fixed points would never be written. After the Cowles Commission moved to Yale, in Fall 1955, Debreu wrote down the most general version he had to offer, and sent it to von Neumann for publication in the NAS Proceedings. But von Neumann was too ill to handle it, so that Debreu sent it to M. Morse. Publication was delayed until 1956 – "Market Equilibrium". In the intellectual chronology Debreu kept for himself, he was meticulous about the order of these events since two other mathematicians conceived a very similar proof during the same period: David Gale in Copenhagen (1955) and Hukukane Nikaido in Tokyo (1956). Since Debreu's interest in a more general proof went back to 1950, it was of considerable importance for him to notice when exactly he conceived what later came to be known as the Gale-Nikaido-Debreu lemma.

Already by the end of 1954, Debreu had all the ingredients of his monograph that would appear in 1959. Most of the time with Cowles at Yale, he devoted to working out his oeuvre. Again, he was in direct contact with the most prominent mathematicians, notably Shizuo Kakutani. His colleagues at Cowles, however, hardly shared his interest. 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 GET.

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 monopoly, externalities, and money. Particularly, Cowles' reviewers did not hold back their skepticism. Leonid Hurwicz wrote the book is "unique in its uncompromising devotion

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[5] This text was written later on the request of Robert Aumann who asked Debreu to give a personal account of the evolution of his proof as presented in his monograph: "Some comments on the history of the Lemma of Debreu-Gale-Nikaido."

to maintain the clarity and rigor of the axiomatic structure even at the expense of other objectives". (1961: 416) He applied standards of explanation when writing "one's understanding of the problem would have been greatly deepened by examples lacking equilibrium due to the failure of one or another of the assumption" (Ibid). 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: 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 of 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 Stave Smale. This community had two anchors: one was to introduce new mathematical techniques such as measure space 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 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. (DPC 5)

But Debreu knew all along that the meaning of equilibrium is open for interpretation. Already right after publication of his article with Arrow, on November 8, 1954, Debreu reacted to Koopmans's objection that the existence proof does not help any kind of empirical analysis. The fact that it is empirically underdetermined, or, in other words, leaves open several interpretations, Debreu considered, not a weakness of the proof, but its actual achievement.

> 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. As for the time horizon, I made allusions in New Haven to several interpretations. The simplest one consists in thinking of an economy with no next period and where the length of life of a consumer is implicitly determined by the x_1 he chooses. None is fully satisfactory and I am afraid it has to be so. (DPC 10, Folder Existence…)

Some 20 years later after his work with Arrow, Debreu indeed proved rigorously that the existence proof really had no strong implications. Simplifying results from Hugo Sonnenschein and Rolf Mantel, he proved the structural indeterminacy of excess demand functions. Debreu was hardly surprised, but others continued praising and blaming him for holding to the opposite. 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.

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: 265). The existence proof was a buttress of 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 actual *tatonnement*, that is to say, 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. Arrow’s “artificial player” that Debreu attempted to exclude, alluded to this process. 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 on stability 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, as mentioned, 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." (WH) 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: 146). Disequilibrium, for Debreu, is a contradiction in itself, since then prices have no conceivable identity whatsoever.

Next to his work on stability in direct response to his work with Debreu, Arrow began working in various field of economic research, applied as well as theoretical. He always remained open for innovations in method 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 interest led him to his 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. He was also active in the spreading institutions of mathematical economics side by side Operations Research. Jointly with Mordecai Kurz, at Stanford, he launched the *Institute for Mathematical Studies in Social Sciences* as well as the department for Operations Research.

It is not the point here to review the manifold contributions of Arrow. Suffice it to say that it was also he, in advanced age, who co-organized the 1987 conference on complexity in Santa Fe – a conference that has been celebrated as the end of economist's favor for deduction which Arrow-Debreu symbolized. Arrow was exceptional in the history of economics, in that he did not have any inhibitions in crossing theoretical and methodological boundaries.

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Arrow and Debreu 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. When and how did the profession react to Arrow and Debreu 1954? Let me identify four sources of increasing popularity.

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 settle down in economists' consciousness. Notably, this happened less in "microeconomics" – as general equilibrium theory was oddly assigned to – but in the so-called micro-foundation of macroeconomics. Arrow-Debreu thus helped finalize what began with Samuelson in the 1950s – the so-called "neoclassical synthesis" (see Hands 2010). This happened not only via the utilization of fixed-points for computational algorithms – the so-called "applied general equilibrium theory –, but mainly via its foundational use in finance, international trade, and other macroeconomic fields. How ironic given that at the same time Debreu with Sonnenschein and Mantel has proven that aggregation is rigorously under-determined!

This late arrival of Arrow and Debreu 1954 is well-documented by the amount of references to their article[6]: During the first 10 years, there are 36 references, most of them in Econometrica from colleagues of either Arrow or Debreu. Until Arrow's Nobel in 1972, roughly the same amount referred to the article, though an increasing number with computational use.[7] Since then, until Debreu's Nobel in 1983, references increased considerably to above 80 – many of them from applied macro fields such as finance, monetary theory, international trade, and even regional studies. Until today, references did not cease to increase, though, to be sure, references in Econometrica decrease. Even if we consider the references per capita – given the substantial growth of the profession

[6] Based on data from SCCI and Google scholar. For similar results, see Oehler 1990.

[7] 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 GET among historians during the 1970s and 1980s, one may wonder how much they have contributed to the making of Arrow-Debreu as a trade-mark of rigorous economics.

during these decades – the paper became a benchmark of research no sooner than the mid 1970s.

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: 188). Arrow-Debreu became a benchmark of rigor in economics, more than in research, for its amenability to raise the entrance barriers into the profession of economics – the highest of which has been built in 1996 by Mas-Colell at 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 source of popularity of the Arrow-Debreu model is certainly its use for economists' political advice. Ever since the 1980s, policy advice was increasingly based on computable general equilibrium models, the influence of which was reinforced by the institutions of the World Bank and the IMF that imposed economists' expertise on other countries. Given the empirical under-determination of the model that Debreu had considered its strength, how could it be considered a professional virtue in this context?

The fourth, 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. For an entire industry of post-Keynesian and otherwise heterodox economists, lamenting the limits of Arrow-Debreu is constitutive for their identity as economists. It was Arrow, however, who due to his eclecticism, could hardly be charged with it, and it was Debreu, who always knew of these critiques all along.

(4) 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 personality, different backgrounds, different paths by which they arrived at joining forces, different roles, and different lessons they drew from their work.

Summing up these observations, while Arrow represents the continuity within the Walrasian and Hicksean 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 two loosely, if not unrelated questions. Accordingly, while Arrow after completion immediately turned to more pressing explanatory uses of GET, 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 of the axiomatization of GET. While Arrow opened a channel for a variety of scientists entering economics as a field of "applied science", Debreu opened a channel for mathematicians entering economics without being trained in it. When macroeconomists later used Arrow-Debreu for rigorous "microfoundations", Arrow had already acquired a skeptical attitude about a satisfying deductive treatment of these issues, and Debreu had already proven mathematically the structural indeterminacy of GET.

Arrow and Debreu 1954, therefore, was not the result of a 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 arguing that conflict through and confronting their different interests, compromised. Neither of them could really identify with their joint work. Debreu accepted his role as a mathematical proof-reader, 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 (in Feiwel 1987: 195 f.)

Without identifying with the final result, Arrow and Debreu could identify even less with the way Arrow-Debreu would later be referred to. Having different notions of their work, none of them would prevail in their reception. Besides their influence upon research in economics, others sources stigmatized Arrow-Debreu – teaching, political advice, and criticism from heterodox economists. Regarding all three additional sources, one is inclined to believe that Arrow-Debreu is *present* economics, in spite of the innovations and proclaimed plurality in current economic theory. Arrow-Debreu persists as the bulwark between economics and other disciplines. Perhaps without Arrow-Debreu, economists would soon lose their bearings when demarcating their research from other disciplines – such as psychology and political science – which they otherwise can embrace free from fears of losing their identity. The success of Arrow-Debreu tells us more about the needs of the profession than about the intentions of their originators.

Arrow and Debreu can be 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 GET. 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?

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Arrow and Debreu remained friends for the rest of their days without joining forces in academic writing again. Instead, they played against each other as coaches of the faculty's football team of Berkeley and Stanford respectively. They also helped each other's career – Arrow would support Debreu's Nobel Prize as well as his students in finding jobs, and Debreu would support Arrow's political campaigns such as the Nobel Statement in favor of NAFTA.

As late as 1995, Arrow and Debreu 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 and social choice, Debreu did not. They had difficulties agreeing what articles count as "Landmark papers in General Equilibrium Theory". Debreu did not want to decide by himself, but made a survey of what his fellows 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" (DPC 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)

11.050 words

References

Arrow, Kenneth J. 1951a. “Alternative Proof of the Substitution Theorem of Leontief Models in the General Case,” in Koopmans, Tjalling (ed.), *Activity Analysis of Production and Allocation*. Cowles Commission Monograph, 13. John Wiley: 155-164.

– 1951b. *Social Choice and Individual Values*. New York: Wiley.

– 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: 507-532.

– 1952. “Le Role Des Valeurs Boursières pour la Répartition la Meilleure des Risques,” *International Colloquium on Econometrics*, CNRS, Paris: 1-8.

Arrow, Kenneth, and Gerard Debreu 1954. “Existence of an equilibrium for a competitive economy,” *Econometrica*, 22 (3): 265-290.

– 2001 (eds.). *Companion to general equilibrium. Landmark Papers in General Equilibrium Theory, Social Choice and Welfare*. Edward Elgar.

Arrow Kenneth, and Alain Enthoven 1956. “A Theorem on Expectations and the Stability of Equilibrium,” *Econometrica*, 24 (3), pp. 288-293.

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

Arrow, Kenneth, and H. D. Block and Leonid Hurwicz 1959. “On the Stability of the Competitive Equilibrium, II,” *Econometrica*, 27 (1), pp. 82-109

Arrow, Kenneth, and Frank Hahn (1971). *General Competitive Analysis*. Amsterdam et al.: Elsevier.

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

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

– 1951b. “Saddle point existence theorems,” Cowles Commission Discussion Paper Mathematics 412, January 4.

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

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

– 1953. "The Continuity of Multivalued Functions," Cowles Commission Discussion Paper Economics, 2079.

– 1956. "Market Equilibrium," *Proceedings of the National Academy of Sciences,* 42 (11).

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

– 1983. „Autobiography," in *Les Prix Nobel: The Nobel Prizes 1983*, Wilhelm Odelberg (ed.), Nobel Foundation: Stockholm.

– 1984. "Economic theory in the mathematical mode", *The American Economic Review*, 74 (3), pp. 267-278.

– 1998. "Foreword: Economics in a mathematics colloquium," in Menger, Karl, *Ergebnisse eines Mathematischen Kolloquiums* (Dierker, Egbert; Siegmund, Karl eds.). Wien: Springer, pp. 1-4.

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

Düppe, Till 2010. "Gerard Debreu's Secrecy: his Life in Order and Silence," working paper.

Feiwel George R. (ed.) 1987. *Arrow and the Ascent of Modern Economic Theory*. London.

Gale, David 1955. "The law of supply and demand," *Mathematica Scandinavica*, 3: 33-44.

– 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*.

Hands, Wade 2010. “The rise and fall of Walrasian General Equilibrium Theory: The Keynes Effect,” working paper.

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 tatonnement: 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.), *Activity Analysis of Production and Allocation*. Cowles Commission Monograph, 13. John Wiley. 155-164.

Lange, Oscar 1942. “The Foundations of Welfare Economics,” *Econometrica*, 10 (3/4): 215-228

McKenzie, Lionel 1954. “On Equilibrium in Graham's Model of World Trade and Other Competitive Systems,” *Econometrica*, 22: 147-161.

– 1960. “Stability of Equilibrium and the Value of Positive Excess Demand,” *Econometrica*, 28 (3): 606-617

Mirowski, Philip 2001. *Machine Dreams: economics becomes a Cyborg science*. Cambridge University Press

Morgenstern, Oskar 1941. “Professor Hicks on Value and Capital,” *The Journal of Political Economy*, 49 (3): 361-393.

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 University Press. 13-42.

– 1936, „Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes," in Menger, Karl (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 1953 [1944]. *Theory of Games and Economic Behavior*. 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.

Scarf, Herbert 1960. "Some Examples of Global Instability of Competitive Equilibrium," *International Economic Review*, 1 (3): 157-72.

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

– 1977 [1972]. "Competitive and Controlled Price Economies: The Arrow-Debreu Model Revisited," in Schwodiauer, G. (ed.), *Equilibrium and Disequilibrium in Economic Theory*, Dordrecht: D. Reidel: 213-224 [Cowles Foundation Discussion Paper 337, 1972].

Slater, Morton L. 1950. "Lagrange multipliers revisited," *Cowles Commission Discussion Paper*, Mathematics 403.

Wald, Abraham 1951 [1936]. "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), pp. 637-670.)

Weintraub, Roy, and Ted Gayer 2001. "Equilibrium proofmaking," *Journal of the History of Economic Thought*, 23 (4): 421-442.

Weintraub, E. Roy 2002. *How economics became a mathematical science*. Durham and London: Duke University Press.

Interviews

Goldman, Steve (SG). Monday, September 14, 2009. UC Berkeley.

Hildenbrand, Werner (WH). Tuesday, March 23, 2010. University of Bonn.

Archive material

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

Leonard, R. 1992. Interview with Gerard Debreu, April 15, Evans hall, Berkeley. Debreu papers, Carton 4.