

# Gerard Debreu's Secrecy

*His life in order and silence*<sup>1</sup>

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**Abstract:** During research seminars, it hardly ever happened that Gerard Debreu posed a question - and if he did, not without already knowing the answer. While some admired him for the tranquility of his austere rigor, others wondered how little he had to say in economics. To whatever effect, Debreu himself was committed to mathematics since he could earn recognition without exposing himself as a person. This essay presents his life and career in light of this personal need for protection by mathematical purity. My account profited from Debreu's personal papers as well as the memories of former colleagues, friends, and family who communicated more freely about Debreu than he himself ever could.

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# Gerard Debreu's Secrecy

## *His life in order and silence*

### **Apart**

Gerard Debreu was born in Calais in the summer of 1921.

For anyone, whatever interest one may have in Debreu's life, it is difficult to overestimate the losses that befell him during his childhood: at an early age, his younger sister burned to death in her cradle. Gerard was present in the house. At the age of eight, his father committed suicide. A year later, his mother died. As an orphan, he was sent to boarding school with rare visits with his maternal grandmother, although he had several paternal uncles and aunts who could have taken him, but did not because of the shame attached to the children. How heavy must these losses weigh on the shoulders of a young child that lacks any means of making sense of them? His sister, the only remaining bond with his parents' family, would commit suicide thirty three years later in 1962.

For anyone today, with whatever morality one was raised, another fact is still more difficult to overestimate: Debreu never spoke to anyone about these early losses, not even to his closest friends or family. He never broke his silence – his daughter found out about these events only after he passed away in 2004. How strongly must this silence have reinforced the burden of his past, and the feeling of being apart? The secrets he kept overshadow the rest of his life, and everything that can be said about it.

Nevertheless, as historians reluctant in adopting psychological theories, we might easily overestimate Debreu's silence – if he had not incorporated it as *the* way of his life. As absent his early losses have been in what others have known about his past, so present is his silence in every single relationship he kept. Debreu was an introverted, soft-spoken, and protective person. Even in private life, be it in his family or among the few friends he had, Debreu was extremely private. "He could not show emotions. That was something terrible for him," his life-long friend Werner Hildenbrand said. Even to his wife, he appeared not to talk about his feelings. He never told her: "I love you" – never.

*Françoise Debreu:* There was nobody really close to Gerard. You know that my husband was a very difficult man to get through to. He was not talking very much. He was thinking

things inside and not exteriorizing at all. And for long time that was the main thing that frightened me (...).

*Chantal Debreu*: My father also had a habit of staring at you. He would just sit and stare and never break eye contact. It was spooky. (...) He could be quiet for hours. Over a dinner, it wasn't unusual for him not to say one word during a three-hour dinner. (FD, CD)

Debreu's silence created a distance difficult to bear for those who felt close to him.

Hildenbrand shared these feelings:

If I tried to talk to him, simply chatting – that was impossible. When we had a walk, it could happen that twenty minutes pass without a single word. I almost got mad about it. For me, with every step, it got more and more difficult to bear. (WH)

Nevertheless, as historians of science, we might easily overestimate his quiet character – if it had not been the way by which he made a professional career. For only in the institutions of modern science, being apart from others is valued as a professional virtue; only in the institutions of modern science, keeping anonymity can count as professional integrity rather than keeping a secret (see Shapin 1990). This applies specifically to economics where personal bias was nagging at its authority ever since its foundation.

In a profession that rewards brash scholars who overstate the importance of their results, he [Debreu] rose to the very top by making no claims for his work beyond the unvarnished statements of his theorems. (Anderson 2005)

It was by way of his quiet attitude that Debreu became the intellectual father figure of an entire generation of mathematical economists. During research seminars, it hardly ever happened that Debreu spoke up and posed a question - and if he did, not without already knowing the definite answer. He never debated with others; he only informed others about his final and incontestable thoughts. While for him, his perfectionism helped him avoiding exposure, for others it was a reason of admiration. When his community celebrated his 68<sup>th</sup> birthday, Dieter Sondermann gave a toast:

Gerard, we have learned from you to prove our claims. We did not dare to confront you with vague ideas. (...) [Y]ou have set standards by your mere presence. And these standards were: rationality, rigor and beauty. Gerard has always been to us a model of perfection, who

not only talked about rationality but also applied it to perfect his everyday life. Such much perfection creates a certain distance. I think all of us in the beginning had to overcome a certain shyness, which was connected with our admiration. (...) Let's raise our glass and join in singing 'Happy birthday to you, dear Gerard.' (DPC 5)

Others, instead, who could not overcome the distance created by his perfectionism, lived in the "fear of disappointing him and the hope of someday, somehow meeting those high standards." (Chantal Debreu 2005)

One of his former students wrote me after my dad died. It was a heartbreaking letter, where this man said that he so admired my father, but he could not meet that standard. And so he gave up economics altogether and walked away and decided to do something else because he couldn't be Gerard Debreu. How sad! (CD)

Nevertheless, as historians of economics, we might easily overestimate Debreu's authority in a small and closed community – if his work had not become a symbol of economics which, subjected to the authority of mathematics, has little to say: regarding economic theory, Debreu was silent about the Grand Themes of general equilibrium theory (GET), above all issues of stability and dynamics, which his work triggered and frustrated at the same time; regarding other theories, he was silent about the relationship between GET and game theory that many saw as an alternative; regarding applied GET, he was silent about its use in econometrics; regarding politics, he was silent about the use of GET for either social engineering or libertarian justification; regarding method, he was silent about the call for rigorous micro-foundations of macroeconomics; regarding methodology, he was silent about explanatory or pragmatic uses of GET; and, at the top of it, he was silent about these and other countless complaints about the limitations of his insistence on rigor (Düppe 2010). In short, regarding the history of economics, he was silent about the hopes and contests that opened the path for his work, and silent about the hopes and contests that were caused by his work.

The success of Debreu – as much bewailed as little understood – cannot be explained despite the fact that he was silent, but because of it. As his long-time colleague at Berkeley, Steve Goldman, said:

I think had he been more expansive, it would have been easier to dismiss him. If he had been willing to conjecture, then others with more institutional or real world knowledge might have found it easier to push it aside. (SG)

Debreu's limitation to mathematics has caused admiration for some, and estrangement for many, economists, but for Debreu himself it was a way to earn recognition without exposing himself as a person. The anonymity of mathematics shields the mathematician from being expressed in his work, as Debreu wrote in his last publication:

[E]ven though a mathematical economist may write a great deal, it usually remains impossible to make, from his works, a reliable conjecture about his personality, because, in particular, formalized expression has deprived an author of his literary style for several decades. (Debreu 2001: 4)

For any historian of economics giving an account of his career, it is therefore imperative to inquire beyond the context of his work and consider the existential needs his intellectual life met. Without diagnosing his psyche, how could we not associate his silence about his early losses with his career? We cannot conceive of his stunning insistence on mathematical rigour as a matter of intellectual taste, of his genius, of his teaching, of methodological convictions, of beliefs about economic reality, or any other reason internal to the scientific context alone. A past, silenced for his entire life, cannot be ignored when trying to understand his intellectual career, in which reticence became *the* principle of gaining recognition. What were the personal needs his research met? And, what difficulties did he, who never gained trust in a worldly order, face among those, whose intellectual life is motivated by a sense for a just order of the world – a “vision”.

Deprived of expressive means, and captive into himself, Debreu would always feel the need to protect himself from a confrontation with the world. Having never acquired the means to reflect on his early losses, Debreu experienced his life as something he could never control enough, as something he did not actually conduct, but something that happened to him – a “random walk” as he called his intellectual biography (1991) in spite of the immensely detailed 5-year plans he made for himself. Debreu could not live up to the integrity of a subject that conducts its life on equal footing with the world.

It was his daughter, to whom my narrative is indebted, who associated his career with his early losses.

He had this anxiety about control and lack of control, because he'd had a number of really horrendous events that happened to him in his life. (...) I think that explains why he was so socially inept. Where he tried to get his survival was by shining academically, which was something he could do. So he was always looking for where he can exercise control. (CD)<sup>2</sup>

On two occasions, when Debreu offered his condolences to friends after their father's death, he referred to their careers: "I hope that your sorrow is made a little less heavy", he wrote to Graciela Chichilnisky on March 3, 1971, "by the thought that you are living up to what must have been some of your father's ideals." (DP additional carton 3) He neither knew her father, nor his ideals. To Herbert Scarf, he wrote on December 7, 1964: "I want to offer you all my friendship in your sadness. Your career must have been a great reward to him." (DP additional carton 1)

### **Mathematizing oneself**

Debreu's career began at the collège in Calais. Teachers were authoritative and learning competitive. Once a week, for example, pupils had to change seats according to their grades. Gerard was an eager learner, interested in the sciences, in particular physics, and soon got to know the "austere beauty of mathematics." (1991: 3) A major event was the national concours général. Gerard won the second prize, a boat trip to French West Africa. Feeling selected is something that can help with one's feeling apart.

In 1939, he took the baccalauréat and left Calais to enter a preparatory class to the grande école entrance examination. That would have been in Paris. But at this point, another event of a different kind befell him, his city, France, and Europe. WWII began. Gerard was sent for an improvised curriculum to Ambert and Grenoble in the Free Zone under the Vichy government, where children were safe from the tumults of the North. "The isolation of the Ambert novitiate," he recalled, "often made it possible to forget that France was at war." (Debreu 1991: 3)

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<sup>2</sup> Hildenbrand agrees: „He wasn't loved as kids usually are. He grew up lonely. Then, he soon has projected his life in achievement, already during school time (...). It was an escape that others wouldn't need. Plausible, yes. But in case of other characters, this would end differently. It does not prove anything." (WH) No, it does not. Again, the purpose of the present exercise is neither to prove, nor to form a moral judgment, nor to provide a psychological explanation of Debreu's life. The purpose instead is to illustrate a strong case for the difficulties of the conduct of an intellectual life in a social science that draws its authority from mathematical rigour. The concrete lesson differs for economists, mathematicians, mathematical economists, and historians.

Gerard remained ambitious. In 1941 he won the competition for admission to the École Normale Supérieure in Paris – one of the most prestigious schools in France. Life in Paris was obviously affected by the occupying German forces. In 1943, he had to do forced labour as a *terrasier* re-building streets and bridges for the German troops. The image of Debreu's body enmeshed in raw earth, surrounded by co-workers from all classes, under the auspices of the Nazis, was always startling for his daughter when imagining her father during the war. For her as a child, stiff as he was, she imagined he slept “fully clothed in his dress shirt and bow tie.” (Chantal Debreu 2005)

Debreu took high risks when escaping from this work in order to attend classes at the École Normale, which, until D-Day, were never interrupted. The École Normale provided some protection against the rest of Paris. It represented another world, as he later described in a speech at the École Normale after receiving the Noble prize (June 1, 1984).

Paris de 1941 à 1944 s'était assombri des plusieurs autres façons. Le black-out, le couvre-feu, les nuits interrompues pas les sirènes d'alerte, les nouvelles des la BBC écoutées à travers le brouillage, la difficulté des déplacements, tout contribuait à exercer de l'extérieur sur l'univers de la rue d'Ulm une pression qui paraissait le maintenir en état d'implosion permanente. Dans cet univers régnait une tension intellectuelle dont je n'ai jamais retrouvé l'équivalent. (DPC 14)

This tense pressure on rue d'Ulm intensified and accentuated the feeling of separation and selectiveness that characterizes the atmosphere of elite institutions.

In this “superheated intellectual atmosphere” (1984), coupled with a “vif sentiment de solidarité” (DPC 14), Debreu's intellectual initiation took place. He found a source of satisfaction that placed him far beyond the surrounding world and events that had defined his life until that day.

Entering the École Normale Supérieure in the fall of 1941 meant another initiation. The three years during which I studied and lived at the École Normale were rich in revelations. Nicolas Bourbaki was beginning to publish his *Eléments de Mathématique*, and his grandiose plan to reconstruct the entire edifice of mathematics commanded instant and total adhesion. Henri Cartan, who represented him at the École Normale, influenced me as no other faculty member did. (...) The new levels of abstraction and of purity to which the

work of Bourbaki was raising mathematics had won a respect that was not to be withdrawn. (Debreu 1991: 3f.)

The encounter with his teacher Henri Cartan, the oldest founding member of the Bourbaki collective, shaped Debreu's image of mathematics and impressed his intellectual ethos for the rest of his days, in particular insofar as Bourbaki mathematics 'commanded instant and total adhesion.' Cartan noted his talent. After his Nobel, on January 20, 1984, he sent Debreu a note he wrote in 1942: "Esprit distingué. Pense correctement et avec finesse. Ne donne pas sa mesure en interrogation." (DPC 5) Being immersed in mathematics, Debreu never showed off.

The Bourbaki program and its role in the history of mathematics has been thoroughly researched by Leo Corry (1992), whose work has informed the account of Debreu's Bourbakism in economics by Mirowski and Weintraub (1994). For the present purpose, it is more important to understand what made the adolescent Debreu so responsive to this perhaps most purist project in the history of mathematics. What kind of experience let him speak of his teacher in such lofty tones?

The most striking feature that Debreu must have experienced as liberating was the anonymity of mathematics that "Bourbaki" as a collective represented. Rather than each single member speaking out in his own name, they aimed at letting *mathematics speak for itself*. Keeping the list of members secret, "Bourbaki" indeed became to represent not a particular school of mathematics, but mathematical rigor itself. Bourbaki *is* mathematics. Their choice of using a pseudonym was thus a symbol of their very program. In the incontestable and sublime world of mathematics, there is no place for an author who has to stand for one's work. Mathematics, if it stands at all, has to stand on its own feet. Bourbaki mathematics was self-speaking, self-contained, and invulnerable. It hides the social process of its making and thereby becomes the primordial manifestation of the force of intellectual cogency and compliance (see Livingston 1999). Also Debreu, apart from these lines, never promoted a particular school of mathematics. He was never an outspoken Bourbakist in economics. With Debreu, economics became mathematized, not bourbakized.<sup>3</sup>

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<sup>3</sup> It is thus not surprising that the very name of Bourbaki did not enter economists' consciousness in the same manner as the virtue of mathematical rigor did. Economists, like Debreu, hardly discuss what kind of mathematics they use. Trying to cure this ignorance, as Weintraub (2002) approached the history of mathematical economics, is thus oblivious of the reasons for the success and amenability of mathematics in economics.



But what makes the anonymity of mathematics so appealing? In the moment that structure and meaning are separate and structures are the only object of concern – which is the imperative of the Bourbaki program – below this object meaning is set free from its urge of being articulated, exposed, or defended. Anonymity liberates intellectual life from its inert and humble nature as it usually takes place in discursive activities, in particular contests. The liberation from the weight and burden of meaning discloses a new kind of joy – an “unsurpassed, addictive intellectual pleasure,” as Debreu would say later (1984) – beyond any epistemic trade-offs between the general and the particular, beyond compromises between induction and deduction, beyond the ups and downs of appearance and concealment. Intellectual experience gains an intensity unknown in any other department of mind.

This sensual as opposed to discursive experience resonates strongly in Jean Dieudonné’s description of mathematics as the “music of reason”. Not the members of Bourbaki wrote, but they listened fully immersed to the “music of reason”. Reason is no longer a principle, a faculty, or activity of judgment but an object of experience (see Düppe 2009). The fascination of Bourbaki mathematics, in short, was the possibility of being whole-heartedly engaged and immersed in an intellectual project, without being committed to a particular claim or judgment. Their mathematics is beyond the social negotiations of meaning, and yet their experience is as dense as the most dignified regions of reasons. It is the same elevation that would make others speak of Debreu, as he was called in the family of Frank Hahn – as God.

This non-discursive character of Bourbaki mathematics had two consequences that distinguish their axiomatic mathematics, for example, from David Hilbert’s: an ignorance regarding scientific truth, and a disinterest regarding philosophical justification. The Bourbakists showed no concern for the scientist’s interest in mathematics. Mathematics and science are, as is required by their axiomatic method, separated. The concern for truth and the concern for rigor exclude each other. Their mathematics was not designed for science, but for mathematics itself. It was supposed to provide an axiomatic “foundation for the whole of modern mathematics.” (Bourbaki 1968: v) It is the mathematization of mathematics, and thus liberation from a pragmatic context in science. For this reason Weintraub rightly spoke of an “oxymoron” of “applying Bourbaki.” (2002: 103) For the same reason, the Bourbakists had to remain silent about a philosophical justification or explanation of their work in other than mathematical terms. Listen to a response of Jean

Dieudonné when being addressed about their philosophical foundation, in particular whether their “structures” might correlate with something “real”.

On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism: ‘Mathematics is just a combination of meaningless symbols’, and then we bring out Chapters 1 and 2 on set theory. (Dieudonné 1970: 145)

There cannot be any such thing as an outspoken philosophy of Bourbaki mathematics. Otherwise they had to tackle “the philosopher’s paradoxes”, and take position within the philosophy of mathematics, which would have to take place outside the frame of “Chapters 1) and 2) on set theory”, or outside the “universe of discourse (...) explicitly listed at the outset”, as Debreu requires in his *Theory of Value* at the outset (1959: 3). The problem of Debreu’s Bourbakism was thus not a particular philosophical belief about the role of mathematics in economics, nor any specific belief about economic reality. Debreu never learned to enjoy discursive, explicative, let alone contestable intellectual activities. How could he since here are concrete persons, and there are abstract structures?

The Bourbakian pathos of anonymity was tailor-made for Debreu. In his Nobel Banquet speech, he spoke of “deep personal intellectual needs” that mathematics satisfies (1984). In a speech he gave at the École Normale after receiving his Nobel, he made clear what needs he meant. He spoke of the tradition of the École Normale as requiring “extrême réserve dans l’expression des sentiments.” (DPC 14) The Bourbakian commitment to keeping feelings for oneself was welcoming to someone who had the need for doing so. Mathematics for Debreu was something to hide behind.

Note that the joy of pure mathematics comes at the cost of frustrating the need for living in a comprehensible world, which commonly motivates intellectual practices. Other Bourbakists felt this conflict, and later would sense remorse for their youngish devotion – Alexander Grothendieck becoming political and then recluse is the most well-known example. Claude Chevalley is another case. Politically active in an anarchist group before the occupation, his commitment to Bourbaki mathematics held him back from living out his political concerns. Being asked about this dichotomy in his life, he replied:

I don't know what to say. It's a mistake. What I wrote in the political arena never satisfied me completely. It was only in Bourbaki that I was truly satisfied with what I wrote. (in Guedj 1985: 21)

Seeing the approaching end of the war as well as the approaching end of his studies, Debreu shared Chevalley's impression that the 'grandiose edifice' of Bourbaki was somewhat misplaced. The end of the war suggested an interest in the social sciences, in particular in the question of what holds society together in the absence of a leader. This indeed led Debreu away from mathematics. But since for him, mathematics served other than intellectual needs, he would back away from living out his social concerns. The impression was suggestive, but did he have the means for responding to it? For only a slight move away from Bourbaki had required an intellectual reorientation no less radical than the Bourbakist program itself.

### **Half-heartedness**

The reasons why Debreu left mathematics remained obscure to him. When he was later asked why he left mathematics, his immediate reply was "the reasons aren't clear to me." (1987) Nor did Marcel Boiteux, the only academic fellow who spent the years of transition next to him, ever understood why (MB). Here are the telling lines in which Debreu described, in hindsight, how it happened to him that he entered economics:

(B)y the end of 1942, I began to question whether I was ready for a total commitment to an activity so detached from the real world, and during the following year I explored several alternatives. Economics was one of them [he considered astrophysics, too, but his teacher, Jewish, had to flee from France]. In 1943-44 the teaching of the subject in French universities paid little attention to theory, and the first textbook that I undertook to read reflected this neglect. The distance between the pedestrian approach I was invited to follow, and the ever-higher flight I had been riding for several years looked immense, perhaps irreducible. Reason counseled retreat to a safe source. What kept me on an unreasonable heading? The formless feeling that the intellectual gap could be bridged? The wishful thought that the end of the war was near, and the perception that economists had a contribution to make to the task of reconstruction that would follow? An improbable event brought my search to a close. Maurice Allais, whose *À la Recherche d'une Discipline Economique* had appeared in 1943, sent copies of his book to several class presidents at the École Normale. (1991: 3-4)

Debreu could have easily rationalized his decision to enter economics by the historical situation. But he did not. He made the decision in light of, but also against his concerns for the social world around him. Given his training in being reserved regarding the expression of feelings, he had not learned how to take such concerns serious. He felt ‘detached from the real world’ because of the pressing political situation, in which the Bourbakist project seemed inappropriate. But given his intellectual values, he could experience such impulse merely as a “formless feeling”, a “perception” which he could not take seriously. He felt the impulse to move on, but was repelled by following it. He had not learned from the Bourbakists how to reflect on intellectual concerns. Their very existence made him suspicious of an “unreasonable heading” of “wishful thought”.

What then did he do? Did he solve the dilemma between his intellectual values and social concerns? As the quote suggests, he did not. Instead, he entered economics by a chance encounter with an authority: Maurice Allais. His book, though hardly at the top of Bourbakian rigor, made him believe that his mathematical training could be of use after all. Following an authority, Debreu did not decide to enter economics; he simply had not acquired the intellectual ethos of taking such a decision. Entering economics half-heartedly, Debreu would always have a reason to hide his Bourbakism, and he would always have a reason not to enter economics too deeply.

The path was not yet set. In spring of 1944 Debreu was supposed to take the agrégation de mathématique, but D-Day caused an interruption. He had to go to the French Army, first at officer school in Algeria, and, after May 1945, for six weeks in Germany – the only “opportunity to experience a life outside the academic cocoon.” (Debreu 1991: 4) At the end of the war, another weighty decision had to be made. He proposed marriage to Françoise Bled, whom he had been dating for three years. But in this case, too, it wasn’t really his own decision: “When he proposed to me, it was not a surprise. He was pushed a little bit by some of his friends. (...) Finally he did it, a little awkwardly.” (FD) After the wedding, Debreu took up studying for his agrégation that he took at the end of 1945. In August 1946, their first daughter, Chantal, was born.

With a degree from the École Normale he easily got a job from Allais as an Attaché de Recherches at CNRS. But Allais could not provide him the prospects of an academic career. Mathematics was seen with suspicion by economists. Allais, located at the École Polytechnique, and known as a “mathematically mad person”, was by far a celebrity in

French academia (Debreu, in Weintraub 2002: 137). He could not provide much guidance. According to Boiteux, who spent these three years next to Debreu, the main joint activity was less discussing their work but to do physical exercise in Parc de Saint-Clou (MB). Left alone with the economics of engineers, Debreu had no idea how to continue his career. In hindsight, he believes to recall the option of going into industry since others around Allais, the so-called “French engineers,” came to be attached to *Électricité de France*. “It was more or less clear that I would be an economist, and that I would not be a professor of economics at a university. Possibly I had in mind vaguely jobs like, let us say, Boiteux’s job.” (in Weintraub 2002: 139)

Yet the work of the French engineers formed Debreu’s implicit image regarding the destination of economic theory. Edmond Malinvaud, who would be friend with Debreu for the rest of the days, remembers a lunchtime group where they read, for example, Abba Lerner’s *Economics of Control* (see Kruger 2003: 184). There Debreu got to know the politics of mathematical economics. It must have been from this experience that he spoke later of the “reconstruction to be made”, which the others of this group really did.<sup>4</sup> In his first economic article (1949) – still using calculus instead of convex analysis – he showed considerable respect for the interpretive dimensions of GET. Here, for the only time of his life, he made the reader aware of a “certain danger” regarding the welfare interpretation of a Pareto-optimum that once was at the heart of the calculation debate: he warned of the “risque de faire passer pour un absolu ce qui est éminemment relatif.” (614) In this situation of unclear future, Debreu was at his most economist.<sup>5</sup>

Two and a half years later, at a Seminar in American Studies in Salzburg, Debreu had his first contact with the Anglo-Saxon sphere and thus derived new hope for an academic career. He learned of an environment that would support more advanced mathematics than was possible with Allais. This was less because of the meeting with Leontief and Solow, but because he encountered another book, the *Theory of Games*, and thus the use of Bourbaki-proof mathematics that made him overlook the anti-Walrasian thrust of this illustrious book. It convinced him to apply for a Rockefeller fellowship in

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<sup>4</sup> It was the only group, the interests of which Debreu would later support by his personal fame. In 1994, during the negotiations between E.D.F and Framatome, a company building nuclear plants, he sent letters to Jacques Chirac arguing for state control (DPC 10). But Boiteux, president of E.D.F. between 1967 and 1987, had no idea why he did so (MB).

<sup>5</sup> Yet he never learned how to express his intellectual passion in differential calculus and continued work in the Bourbakian sphere. He translated de Finetti (1949) without publishing it (DPC 9).

1949, where he spent most of the time in the library of Harvard continuing his reading.<sup>6</sup> At a short visit at the Cowles Commission in October, he must have made a considerable impression, since he was immediately offered a position as a Research Associate beginning in June of 1950. Prior to that, he visited Uppsala and Oslo. For this period he wrote in his intellectual chronology, which he kept in his papers “Solution of Pareto optimality via calculus.” (DP additional carton 3) While working on this solution, in February of 1950, his second daughter was born. He was apparently already so involved in mathematical economics that he did not interrupt his stay in Scandinavia and only saw her for the first time months later.

### **Discreetness**

The U.S. was welcoming to the Debreu family. When Françoise Debreu took the boat with the two babies, she was recognized by Eveline Weil, the wife of André Weil, one of the Bourbakists’s figureheads. Though they were not close in Paris, Eveline Weil knew of Gerard Debreu coming to Chicago. She addressed Françoise and they became friends.

Their husbands would become closer too. André Weil, at Chicago since 1947, was one of those who welcomed Debreu in Chicago. They met on a personal basis and discussed each other’s work. The 1950s were the years when the Bourbaki group published most frequently and won evermore authority in U.S. mathematics departments, though, to be sure, mathematical purity had never been as ingrained in U.S. academic culture as it was in France. It was operational for scientism during the Cold War, no more. Debreu was grateful for Weil’s presence.<sup>7</sup>

A Chicago il y a un excellent département de mathématiques, très rigoureux, qui a été profondément influencé par l’école de Bourbaki dont l’un des représentants principaux était le grand mathématicien André Weil. (...) En ce qui me concerne, dès que je suis arrivé à Chicago, j’ai senti cette influence. (in Bieri and Bini 1998: 19)

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<sup>6</sup> Boiteux applied, too. Both were selected, and Allais had to decide who would go to the U.S first. Debreu always emphasized this decision as determined by chance. They tossed a coin: “La pièce tourna longtemps sur elle-même sur la table et finit par décider que je quitterai Paris a la fin de 1948” (DPC 14). He must have told this story on so many occasions that this was the first thing his wife told me when I began talking to her.

<sup>7</sup> Debreu also collaborated with Israel Herstein (1952), and was in personal contact with Saunders MacLane and Armand Borel.

More importantly, the Cowles Commission was welcoming of Debreu's Bourbakism, too – in particular Tjalling Koopmans, who had just become Director of Research. He welcomed Debreu because he helped him to push Cowles from its motto *Science is Measurement to Theory and Measurement*. For both, the replacement of “is” by “and”, and the replacement of science by theory, Debreu would be vital. At his arrival Debreu did not notice much of the former, empiricist spirit (the Activity Analysis conference was in June, 1949). Now he felt liberated from the suspicion that lay on mathematicians in France:

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)

When Debreu arrived at Cowles, he stood in the midst of a generation of economists that underwent their scientific socialization during WWII, which comprised the application of mathematics in military planning (Dantzig), in socialist planning (Marschak), and in Keynesian “planning” (Klein). There, in a “very closely knit group”, as he recalls, he met those economists who “had a contribution to make to the task of reconstruction.” (1991: 4) But Debreu again stood apart. He was the only one whose intellectual socialization in mathematical purity was the protection rather than confrontation with WWII. What he knew from Allais was of little help in living up to an identity strong enough to enter the Cold War milieu. “I was left alone to do my work during the five years from 1950 to 1955, a marvelous opportunity that I tried to use fully.” (Debreu, in Feiwel 1987: 256) For Debreu, arriving at Cowles had the taste of both entering an overwhelmingly powerful institution of economics as well as returning to Bourbaki.

Note, that the theoretical turn at Cowles was not only a matter of taste, but of politics. Debreu's widening of the gap between the pure and the applied was decisive for both Cowles's claim to non-partisan authority when confronting Friedman or appeasing McCarthy and their funding by making promises to RAND and the Air Force.<sup>8</sup> As vital as Debreu's Bourbakism was for Cowles in order to maintain these links, it was just as important for Debreu not to be involved in them. Within the high ideological density of the 1950s, Debreu was not in control of the channels that lead from theoretical to political claims – though he contributed to their inscrutability. He would not feel at ease when being

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<sup>8</sup> For a similar argument see Mirowski and Sent (2002: 22).

addressed about economic contests, anxious of saying something wrong in light of the omnipresent threat of being blamed for the consequences. Too easily would a claim lead against the political respectability of the author. So better not to claim too much! To be rigorous, and nothing but rigorous, now meant not being suspicious. Avoiding the question ‘What does that mean?’ – which defines the good mathematician according to Bourbaki – now meant not being a bad economist – whatever that means.

And so Debreu remained discreet, in all respects. He never took a stance in Cowles’s internal affairs concerning the role of theory, although he had plenty of occasions to do so.<sup>9</sup> Debreu embodied mathematical rigor, but did not become active in cultivating economists’ taste for it – even if he knew that his mathematical skills were more advanced than that of others, including Koopmans:

I do remember that he [Koopmans] was not familiar with the definition of a Banach space, because somebody had used the concept of a Banach space, and he asked for a definition, so I imagine that he was not familiar with infinite dimensional spaces. (in Weintraub 2002: 148)

Debreu merely imagined. He apparently did not even talk about his or Koopmans’ mathematical backgrounds. How then could he have written Koopmans’ *Three Essays*, even if he had agreed with them word for word?

The same holds true for his involvement in Cowles’s external affairs, above all regarding its relationship with RAND. Debreu never engaged in the actual design of the Cowles’s research program as a supplier of social and political design. Between Cowles and RAND stood Bourbaki in the person of the unknowing Debreu, who was ignorant about what his colleagues did “during the summer at RAND”. At least so he asserts later, somewhat ambiguously.

Some of the mathematical economists I knew spent a significant part of the summer at RAND. I did not do that and that may be due to some extent, but not entirely, because I was not a U.S. citizen, and RAND was doing a number of things for the army. (...) I do not know who from Cowles went to RAND in the summer. (in Weintraub 2002: 143/145)

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<sup>9</sup> Next to Koopmans’ theory vs. measurement debate of 1947, and the tensions with Friedman’s economics department, mathematics was also debated within Cowles, as for example via the platform of the Econometric Society in 1953 (see Mirowski 2001: 394 ff.), or via the debate launched by Novick in the *Review of Economics and Statistics* in 1954.



Some went, Debreu knew. But he preferred not to ask who went. Who knows what they do there? So better not ask. With such an attitude, it is a sheer impossibility to imagine Debreu sitting next to von Neumann in a row with Norbert Wiener and Margaret Mead in the Macy conferences. He would have been deeply embarrassed by the insipidity with which the political clamor defiled the music of reason. Debreu felt safer in the shadow of those who mediated between him and the economics profession.

Debreu's career would have not lasted longer than the early Cold War years if there had not been the single most important collaboration of his professional life with Kenneth Arrow (1954). I cannot go through the evolution of this celebrated piece of work as it presents itself in their extensive letter exchange (see D uppe forthcoming). Let me only mention the many ways in which they differed. Personally, Arrow was extroverted, outspoken, and was saying immediately what came to his mind, exactly the opposite of Debreu. While for Debreu, their joint work would frame his research for the rest of his days, for Arrow, it was one among many projects he was working on simultaneously. Regarding their motivations, Debreu was grateful for the chance to show rigor, and Arrow wanted to know more about welfare implications of GET. Debreu would note mathematical flaws, which Arrow showed to be economically trivial, while Arrow would point to economic flaws, which Debreu ruled out mathematically. Debreu would suggest a deletion in case of a "controversy about the interpretation of the text", and Arrow would write the introduction and the historical note. While Debreu continued with yet more rigid work, Arrow continued in welfare economics and social choice.

These differences became apparent in Debreu's book version – the *Theory of Value* (1959), the purest piece ever written in economics: the proof is more general using the Debreu-Gale-Nikaido Lemma, does not use the term "competition", and gives no reference to the equivalence with "games" (i.e. the artificial player). For the year 1954, Debreu wrote the most extensive entry in his intellectual chronology. He notes that he already then had a ready version of his more general proof, and of the first four chapters of his monograph. He discussed his proof with Armand Borel and Andr  Weil, and sent it to von Neumann for publication in the NAS Proceedings. Since von Neumann was too ill, publication was delayed (1956). Arrow and Debreu could not possibly have joined forces another time.<sup>10</sup>

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<sup>10</sup> In 1995, Arrow and Debreu once more tried to collaborate in editing the Elgar "Landmark papers in General Equilibrium Theory". It was a difficult task since they could not agree on what to include – though Debreu

In the summer of 1955 the Cowles Commission, including the Debreus, moved to Yale, and thus away from the quarrels with the economics department in Chicago. Debreu, 34 years old, was made Associate Professor of Economics without tenure. His future in the U.S. was still uncertain, and he continued working in mathematics in direct contact with Shizuo Kakutani (Debreu 1960). But for most of the time at Yale, he was devoted to his monograph fostering evermore purity. Since Cowles was politically in a calmer situation than in the immediate post-war years, Debreu's purity, however, was less operational for Cowles's authority than it was in Chicago. And so the diverting interests between him, who advanced mathematical tools whatever the effects on economic theory, and the other Cowlesmen who advanced economic theory with whatever tools became visible. All of his colleagues showed greater hopes for the expressive future of mathematics in economics: Simon began computer simulations; Marschak moved to information issues with experimental designs; Patinkin included monetary theory in GET; and, most notably, Arrow and Hurwicz worked on stability (1958).

Debreu did not contribute to this research. About Hurwicz and Arrow's work he merely "shook his head. He knew at the outset that this leads to nowhere." (WH) Debreu did not share the hopes that his work with Arrow had generated regarding explanatory purposes of GET. An equilibrium, for him, had no referential meaning, but was a condition of a consistent theory: "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. "In proving existence one is not trying to make a statement about the real world, one is trying to evaluate the model," he explained much later in his life (in Feiwel 1987: 243).

That hardly anyone has shared this view of his work became most apparent in the reviews Debreu received for his monograph. All of the seven reviews showed reservations about its purity, and noted the regrettable exclusion of monopoly, externalities, and money. Particularly, Cowles's reviewers did not hold back their skepticism. Leonid Hurwicz wrote 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: 416) He put it at the standard of an explanatory theory when writing "one's understanding of the problem would

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conducted a survey among his peers regarding what counts as a "landmark". 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). The edition was published in 2001.

have been greatly deepened by examples lacking equilibrium due to the failure of one or another of the assumptions.” (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; but he did not defend himself. Without the immediate success of his book, his career was still at a tipping point. It may have not continued if he had not brought others, notably game theorists, back on board: together with Herbert Scarf, he shored up the so-called Edgeworthian program regarding the equivalence of strategic and competitive behavior, which gave occasion to the introduction of measure theory (1963). But here, too, Debreu did not share the same expectations with his collaborators: “Even if it converges, he always said, so what? It is still no dynamic. But only a few have thought that way in these days.” (WH)

In 1960, during a stay at the Centre for Advanced Studies in Behavioral Sciences in Stanford, the offer came from UC Berkeley. But he also had the opportunity to return to France as a researcher – he didn’t have the agrégation de l’enseignement supérieur for a full professor in economics. Though returning to France would be a step back in his career (his work was unapproved by the Bourbakists), the differences between him and the U.S. scene must have been so grave that he decided to pass the offer and return to France. His wife refused. “[W]hen we were in France, on the beach, in 1962 [1961?], he had the possibility to stay in France. He would have taken it if I had said yes. He resented the fact that I used my veto.” (FD) On May 10, 1961, he informed Koopmans about “his” decision: “If I judge by the time I spent pondering it, it has probably been the most difficult dilemma I ever had to resolve.” (DPC 8) Debreu stayed in the U.S. for the sake of his family, particularly his daughters, who were more adapted to the U.S. than him. Back home, his sister committed suicide in 1962.

### **Feeding false hopes**

The decision for Berkeley to call on Debreu was certainly no less controversial. Roy Radner supported his recruitment, but others shared the doubts present at Yale, where one

was reluctant in granting him tenure. For Berkeley was hardly a stronghold for mathematical economics; it was no stronghold for any particular school, but rather eclectic.<sup>11</sup>

During that time the department had more of a sort of senatorial system, where there were individual people who were responsible for particular areas and they were more or less left alone to run those areas. (...) The department was pretty content to let each group run its own affairs. (RA)

Debreu could easily relate to an environment, in which one would let others conduct their own business. His discreetness found its natural soil – claiming expertise in one’s own sphere without inquiring into, conjecturing about, or even challenging the expertise of others. Although the intellectual community in Berkeley was hardly as tightly knit as at Cowles and the École Normale, this made no difference to Debreu: again, he was left alone to do his work.

Debreu’s tolerance for other people’s work had its limit. “[W]hen it came to mathematical economics he had very strong tastes, and very strong preferences about what he considered to be mathematical economics.” (SG) Indifferent tolerance about various fields of economics coupled with intolerance about the standards of mathematics was Debreu’s general attitude to economics. This attitude held him back from cooperating with Harsanyi, Sheppard, and Dantzig who did operations research at the engineering department and the business school. Though he never imagined mathematical economics without economics around, he represented the only serious approach within mathematical economics.

But again, Debreu withdrew from discursively explaining, promoting, or defending his axiomatic approach, though his standards of rigor became increasingly attacked in the profession at large as well as at Berkeley.<sup>12</sup> While he avoided conflicts, his colleagues

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<sup>11</sup> The most theoretical work, apart from Roy Radner, was done by Dan MacFadden. But there was also Robert Gordon’s group doing business cycle theory and labor economics, George Break representing public finance, Dale Jorgensen in econometrics, Hansen’s macroeconomics group, to name a few. The big name was Abba Lerner, interested in the politics of equilibrium theory but not in Debreu’s mathematics, as also applied to Benjamin Ward. Not to forget, Berkeley was and still is strong in economic history as represented by Carlo Cipolla. It was this plurality that made it possible to accept an outlier like Debreu adding his share.

<sup>12</sup> Debreu earned direct critique in his colleague’s monograph *What’s Wrong with Economics* (Ward 1972). Another occasion of considerable publicity was in 1975, when at the AEA meetings Robert A. Gordon gave his infamous presidential address on the trade-off between rigor and relevance (1976), and Dan Mc Fadden was given the John Bates Clark medal at the same meeting.

sometimes had to judge his work, such as at occasions of PhD defenses of his students. Then he openly faced the difficulty of being accepted as an economist. In 1974, one of his most promising students, Graciela Chichilnisky, defended her thesis. In the Examination report of Dan McFadden, we read that she

had not yet demonstrated that her particular construction is of economic interest. The committee feels that it is essential that she incorporates in the thesis a careful exposition of the economic motivations underlying her analyses. (...) The committee was unable to come to a determination on the technical merits of the appendix. (DP additional carton 3)

Such skepticism, and his general disposition not to take on controversial tasks, held him back from becoming active in the organization of the department. Roger Hahn, a historian of science from France who became close to Debreu because of their wives' friendship, recalls: "I served on several committees with him, and he wasn't particularly interested in what went on. He would go along with whatever was said. But he didn't probe at all." (RH) Nor did Debreu probe when Hahn tried to engage him in a discussion about scientific determinism that interested Hahn (RH).

Tolerant, aloof, and disengaged, the most visible fact that separated him from his colleagues was his cultural background, which had been less apparent among the émigrés at Cowles. Though Berkeley was comparably friendly to Europeans, he stood out in his distant social manners. "The way they [he and his wife] lived in Berkeley – it was an island." (WH) At lunch time, for example, instead of joining others in the Men's Faculty Club of the University, he used to go home to have lunch with his wife every single day. Also in his language customs, apart from his French accent, he stood apart. He was the only one who was addressed as "Professor." (SG) A rather disturbing event occurred at a dinner at Debreu's home with Steve Goldman and his wife. There was a bell on the table and his daughters would act as waitresses. His two daughters, growing up in two worlds, would not do that for long. They left home early. Florence did not want to attend university, and at 16 Chantal went to Harvard to study political science.

Cultural more than professional reasons determined Debreu's social life. Apart from the contacts he had via his wife, he became friends with the economic historian Carlo Cipolla, who shared his continental manners and humanist education. "They had something in common, because they were both culturally knowledgeable, whereas many of the

American economists were parochial.” (RH) Next to cultural vicinity, Debreu felt a closer connection with natural scientists. A telling expression of this preference was his relationship with Edward Teller, perhaps the most contested figure on the Berkeley campus. They got in contact since his daughter, Chantal, married Teller’s son, Paul Teller – although Debreu warned her of the risks of being the spouse of a child of a famous person.

He [Debreu] was very pleased about the idea that he would be associated with a big name, a physicist, even though, as you probably know, Teller was hated by his colleagues in Berkeley because of his political stance. He wasn’t so much proud of his daughter, as he was of the association with Teller. (RH)

Apart from natural scientists, Debreu looked up to the mathematics department – the big name was Alfred Tarski and his cultural contact person from France, Lucien Le Cam. Admiring them on the one hand, he would be too proud to ask them for help, Hildenbrand recalled. Debreu would not have built up contacts with mathematicians, if they had not become curious about him. Apart from David Gale, whom he already knew from Cowles, the most important encounter was Steve Smale. He approached Debreu not only with a vivacious curiosity about economics, but also with his collection of minerals – a hobby austere enough to attract Debreu’s attention and admiration (WH). Smale, since receiving his Field Medal in 1966, was a big name in mathematics and crucial for Debreu’s reputation. He was vital for the recognition of mathematical economics as an “applied” field in mathematics. He widened the gate through which mathematicians could enter economics without being trained in it.

Smale, unlike Debreu, however, had not only an eclectic interest in mathematics but also a rather politicized interest in economics. In the late 1960s, when the campus became the hotspot of anti-war radicals, Smale was one of the leading figureheads. Debreu, instead, socialized with the academic imperative of “extreme reservation regarding the expression of feelings,” was enraged about the insipidity of academic culture. “I would enjoy (...) seeing a University which looks like a University,” he wrote to Radner on May 18, 1970 (DP additional carton 1). While Debreu’s political abstinence may have been operational for the success of mathematical economics in the 1950s, how could it be the same in the late 1960s when academia carried away the re-politisation of society unimagined in the early Cold War years?

Berkeley was clearly not the place to build up a research community. Although still left alone to do his work, the difference between his situation now and at Cowles was that there was no authority around him who would take care of the recognition and meaning of mathematical economics. He, who had put the least hopes in the existence proof without tackling any of the Grand Themes, now had to gather his own community. He would succeed, but his reputation would increase by the hopes that flourished among those which he never had wanted in his closed community of rigor.

What was merely a phenomenon in the 1950s, evolved during the 1960s as an identifiable school in economics: Neo-Walrasianism – though hardly any of Debreu’s devotees may have read Walras. One could distinguish a first generation of mathematicians who moved to economics during the 1960s (such as Drèze and Hildenbrand), and a second, self-grown generation, who concluded their PhD in mathematical economics during the same period (such as Mas-Colell, Varian, and Schmeidler).<sup>13</sup> His growing community, spread around the globe, is distinct from other mathematical economists in that they followed Debreu’s lead in putting mathematical structure first, and ignoring the Grand Themes of equilibrium theory. Besides the orthodox endeavors for existence proofs with ever “weaker assumptions”, much intellectual effort has been put in the Edgeworthian program mentioned above.

The hotspot for working with Debreu was his weekly Monday seminar. Apart from regular visitors from Berkeley (Radner, Gale, McFadden, and Thomas Marschak) the spirit was set by his many guests from abroad who were formally better trained than economists in the U.S. One of them was Werner Hildenbrand, Visiting Professor between 1966 and 1971. They became close, first, because his wife was French and quickly befriended Françoise, and second, because Hildenbrand also knew Bourbaki’s book by heart. “It was a friendship between two married couples. The wives got on with each other, and the men could work well with each other.” (WH) A decisive contact was Jacques Drèze from Belgium, whom Debreu had already known for being a guest at Cowles in 1954. In 1966, Drèze launched the European equivalent of Cowles: the Center for Operations Research and Econometrics (CORE) financed by the Ford Foundation. Drèze tried to recruit Debreu without success – Françoise did not want to live in Belgium. Most of Debreu’s devotees

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<sup>13</sup> In Denmark there was Karl Vind, in England Frank Hahn, in New Zealand A.D. Brownlie. An important link was to Robert Aumann in Israel, who built up his group at the mathematics department of Hebrew University. David Schmeidler (PhD 1969), and Bezalel Peleg (PhD 1964) grew up there. In the U.S., his first devotees were Hugo Sonnenschein (PhD 1964), and later Andreu Mas-Colell (PhD 1972), whom Debreu would recruit to Berkeley.

were located at, or once passed CORE (Grodal, Schmeidler, Mertens, Gabszewics, Kirman, and Bewley, to name a few). From CORE, several other institutions in Europe arose, to which Debreu was travelling at the end of the 1960s.<sup>14</sup> His community also grew by his PhD students. Most of them, apart from Truman Bewley, were not from the U.S., and continued their career back in their home countries.<sup>15</sup> None of them would become a new “Debreu.” They profited from the authority that mathematical economics gained at large, but did not direct the profession into yet deeper Debreuvian waters.

All in all, the Neo-Walrasian community flourished more on European than American soil. In the U.S., his influence grew less via devotees, but by increasing reference to what became the trade-mark of rigorous economic theory: the “Arrow-Debreu model” – even more so after Arrow’s Nobel in 1972. In operations research, mechanism design, computable general equilibrium models, finance, international trade, and other macroeconomic fields “Arrow-Debreu” became *the* benchmark of research. It became the initiation rite in graduate education, and, even more surprising for Debreu, a standard for policy advice. This reference to “Arrow-Debreu” was clearly one-sided. The big difference was that the profession used GET as a benchmark for actual explanatory purposes, which Debreu did not embrace. In 1977, he spoke of those who applied GET for such purposes as being *seduced* by the axiomatic method (DPC 14). He would embrace even less the way he was referred to after 1974 when the explanatory void of GET was rigorously proven by the so-called Sonnenschein-Mantel-Debreu results. The results gave a mathematical proof of Debreu’s discreteness.

Hugo Sonnenschein showed courage when asking: if one had so great troubles proving more than existence, perhaps, after all, nothing is in it? From our point of view, the results read like an apology by Debreu for the promises his work offered since it proved that there is really nothing to say in axiomatic equilibrium theory. Was the result surprising for Debreu? Are the existence proof and the structural indeterminacy proof not the same in reverse? In 1954, he showed that one could prove existence, and this, he showed in 1974, is

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<sup>14</sup> In Paris, CEPREMAP, going back to Pierre Masse, was launched in 1967, where Debreu was in contact with Florenzano, Fourgeaud, and Cornet. Hildenbrand, back in Bonn since 1979, spread Debreu in cooperation with Krelle. In 1977, the *European Doctoral Program in Quantitative Economics* was founded in cooperation with LSE, Bonn, and later *École des Hautes Etudes en sciences sociales*, and, again later with University of Pompeu Fabra. In 1981, Jacques Laffont founded GREMAQ at Toulouse. Other institutions were Delta in Paris, and CentER in Tilburg.

<sup>15</sup> Worth mentioning are Thierry de Montbrial (1971), Jean Michel Grandmont (1971), Volker Boehm (1972), Jean Pascal Benassy (1973), Bryce Hool (1974), Tatsuro Ichiishi, Lawrence Blume (1977), and Beth Allen (1978).



indeed woefully little – at least not enough for an actual economic claim in causal terms, or in terms of comparative static.

When he wrote the excess demand function paper, I asked him, if he had always expected that result. He said no, but he knew that an equilibrium concept as a fixed point argument is of no use, in particular for dynamic questions. As it is defined, there is nothing in it. (WH)

Had the Neo-Walrasian discourse been more accessible to economists, the results could have provided material enough for an actual revolution in economic theory. But they did not. For not even within the neo-Walrasian shrine the results triggered an open reflection on the nature of their research program. Interpretive concerns were simply not in spot. In the correspondence between the three main actors, one finds no remark whatsoever on the consequences for economics. Though some have drawn serious conclusions (such as Sonnenschein, Kirman, and Hildenbrand), Debreu himself perceived the results as beautiful, simple, and elegant. No surprise, the results had no clear effect on the discipline. Some developments in economic theory after 1974 could be seen as informed by them – the strengthening of game theory, econometrics, and behavioral economics. But hardly anyone of these innovators referred to the results, while most of them flew the flags of rigor as high as before. Other developments were evidently uninformed by them – such as the call for micro-foundations in macroeconomics! In microeconomics, in turn, one usually dodges the issue by assuming Cobb-Douglas, which had been mere ad-hocery for Debreu. Hildenbrand, for example, feels his work is not taken seriously: “Economists are bunglers. Basic research is out.” (WH)

Apart from being unable to interpret the results in economic terms, economists were already too deeply committed to founding their science on equilibrium analysis.

It had made too many people unemployed. Once I said in the Econometric Society that economists should forget Lagrange only for one year in order not to think continuously of maximization under constraints. You are mad, they replied: “You destroy human capital.” This is what they have learned, it was difficult enough, and thus they continue applying it. (WH)

Debreu's 1974 article represents his last entry in his intellectual chronology. Without ever giving an interpretation of his research program, Sonnenschein-Mantel-Debreu completed it. Now, in his mid-50s, he backed away from the stage of economics.

Nevertheless, in the following years his popularity again increased in terms of both a second generation of devotees and reference to his work – be it as a benchmark for orthodox, or as a point of demarcation for heterodox, economics. In the early 1980s, mathematical economics was as successful as never before. Unlike today, economic theory *was* mathematical economics. Literary economics was as absent from the leading journals as never before. Mathematization arrived in basically all fields of research, and the Neo-Walrasian community claimed leadership in the entire profession while being at the same time rather exclusive. Debreu was the leading star at the firmament of mathematical economics. No economist ever exceeded his purity. In 1983, only Keynes's *General Theory* and Marshall's *Principles* were cited more often than his *Theory of Value* – in terms of actual reading, statistics might differ. He received several academic honors, was granted the civil rank *Chevalier de la Légion d'Honneur*, and more informally, was proud to lead the departmental football team of Berkeley against Arrow's at Stanford. He may have been content with having implemented a sense of rigor in economics, without feeling responsible for how the profession continued in other directions. There was no longer any question of being an economist or not. Gerard Debreu had made it.

And so his life calmed down, while his family grew. Spending time with his young grandchildren “helped me becoming more human,” he said in an interview (DPC 5). He also indulged in his hobby, astronomy. Recall that, back in 1943, he considered going into astrophysics instead of economics. He did not forget his passion:

By 1982 it seemed that time was softening the edges a bit. (...) [M]y father seemed warmer and less formal; he hiked at Point Reyes, played bridge with his grandchildren, and loved to get out his telescope on starry summer nights and for special events like solar and lunar eclipses. (Chantal Debreu 2005)

Watching the stars must indeed be a closer experience to the aesthetic appeal of Bourbaki mathematics than doing economic theory: a safe distance from the world – elements and sets, stars and clear nights. In the early 1980s, Debreu must have believed his days in economics were numbered.

“Then in October 1983 came the thunderbolt out of the blue: the Nobel in Economics. I don’t think any of us in the family at the time recognized it for the disaster that it was to be.” (Ibid)

### **Insanity**

On October 17, 1983, at quarter to four, a phone call rudely awakened Debreu. The way he was informed about winning the Bank of Sweden Prize would only forebode what inescapably had to follow in the coming months: “A radio station in New York City relayed the news from Stockholm and wanted to know what grade I would give President Reagan on his economic policy.” (DPC 14)

After years of professional discreetness, Debreu’s silence was now celebrated in greatest publicity. From one day to the next, he stood in the spot of the world *as an economist* – the same world, in which he always tried to keep a low profile, and which he only knew from primitive concepts. Now there was no authority he could rely on. He himself was addressed: the prize was given to him having achieved something. What a challenge for someone who drew all his intellectual satisfaction from the fact that he did not have to reflect on the meaning-bestowal of his work. Receiving the Nobel was as if the walls around an ivory tower fell apart, and the elevation that once provided protection turned into the elevation of the world scene. How did he deal with this sudden honor?

Planning ahead five years, Debreu was not prepared for this event. The Nobel Prize *happened* to him. About the first moments, he later would say:

A press conference, T.V. cameras, photographers are waiting, and the media sometimes seem to believe that every October in Stockholm and in Oslo, Pentecost is reenacted, that the new Apostles have acquired universal wisdom and can now foresee the future. (DPC 14)

At the press conference, Debreu still assumed that he could accept the prize for reasons he himself would have wanted to. With his opening remark he anticipated wrong-headed questions: ‘I do not want to discuss my views on the Reagan Administration’s economic policies’ (New York Times, 10/18/83). He nevertheless was asked if his work helps in dealing with the present crisis. He swerved, as Roger Hahn re-paraphrased: “no, I don’t think it has anything to do [with that]. But I have given my wife all of my earnings from the

Nobel Prize and she will help to revive the economy.” (RH) The actual irony was that the money he gave his wife was counted down to the penny before and after spending it.

His further remarks made clear that he wanted to accept the prize as a mathematician, confusing for his colleagues from the economics department.

Unless you listened carefully, you wouldn't know that he got a prize in economics. There were almost no references to the economics department, no reference to the economics profession. He talked about the importance of funding the mathematical sciences, nothing about funding the economics profession. It was all about mathematics. (SG)

The support of the mathematics department may have surprised most, but pleased some. Leon Henkin, the head of the mathematics department, was present. Debreu would later find a note from him in his office inviting him to closer cooperation (DPC 5). If only Debreu had received the Field Medal, such congratulations would be the only kind he had to deal with. But after the press conference it must have dawned on him that even if he had wanted to accept the prize as a mathematician, it would not be given to him as a mathematician.

This lesson awaited him when reading the newspaper the following days. While one newspaper simply wrote that his work “is a plaudit for pure research” which “for most of us, could pass as the true link between drowsiness and sleep,” (Newsday, 11/14/83) others made stronger claims, calling him “the man who proved what others thought they knew.” (Financial Times, 18/10/83)

La presse fut à plusieurs reprises pendant cette étrange période de sept mois la source d'un autre type de frustration en réfléchissant des images des la réalité profondément altérée par les miroirs déformants utilisés quelquefois. (Debreu, DPC 14)

One of these deformations was an article in Le Figaro Magazine headed “La supériorité du libéralisme est mathématiquement démontrée.” (3/10/1984) This libertarian reference that caught most public attention, was not made up by the press, but was part of the committee's Whig reasoning for granting Debreu the prize: Debreu's work had been relevant in that he solved the age-old problem of Smith's invisible hand. It is doubtful that anyone in the Neo-Walrasian community has ever used that reference before the Nobel committee, but it was a welcome reference to explain his work in public, and welcome for Marxists, such as

Resnick and Wolff to suspect the committee for being biased. (1984: 30) The committee indeed suggested that Debreu and Smith had the same in mind, but Smith had only a vague intuition and Debreu the scientific proof:

Adam Smith had already raised the question of how [self-interested market] decisions apparently independent of one another, are coordinated (...) [His answer was that] price systems automatically bring about the desired coordination of individual plans. Toward the end of the 19th century, Leon Walras formulated this idea in mathematical terms as a system of equations (...). But it was not until long afterwards that this system of equations was scrutinized to ascertain whether it had an economically meaningful solution, i.e. whether this theoretical structure of vital importance for understanding the market system was logically consistent. Arrow and Debreu managed to prove the existence of equilibrium prices, i.e., they confirmed the internal logical consistency of Smith's and Walras' model of the market economy. (Press Release)

In these lines, the identification of mathematics and economics that underlies the irony of Debreu's Bourbakism in economics had been made explicit. Be it bad journalism, revealing for the historical insensitivity that spread in the profession since Debreu, or perhaps even justified because of a general formalist tendency in market theory – for us it is important to consider the pressure this link exerted on Debreu. The tragic point is clear: Although the aloofness of Debreu's work necessitated Whigish reasoning in order to justify its worth, the reasoning would relentlessly be turned back on him as the question: what does it mean that you proved that 'markets works automatically'?

Debreu had the option to straightforwardly open a debate about the political meaning of GET, which certainly would have been a healing experience for the profession. His students did respond to the co-optation of their teacher in the liberal camp. In a letter to the editor of the Wall Street Journal, they

object to your editorial of October 20, which claimed Prof. Debreu proved 'the invisible hand of the market works (...). Your attempt to identify his theories with the particular economic policies you advocate does a disservice to both the man and the prize. (DPC 6)

Also Steve Smale, in the California Monthly, referred to Adam Smith but added that "the theory allows grossly unequal shares of the good of society. Thus, substantial government

mediation of the decentralized price system is required.” (11/4/83)<sup>16</sup> And Debreu? In a letter to Siamack Shojai on May 20, 1987, he writes:

I also have read the article by Resnick and Wolff in the Monthly Review, December 1984 without feeling [the need] to reply to the two authors. I have consistently tried to say clearly, sometimes with simple mathematics as in the enclosed paper, what the theory of general equilibrium had to contribute to the understanding of economic processes. I expect proponents of alternative theories to do the same, and to let readers judge. (DPC 5)

With the same attitude, Debreu wrote his Nobel Lecture. He prepared the speech with the painful feeling that the reasons for giving him the prize would not match the terms in which he could explain his work. The preparation put so much pressure on him that he even considered cancelling the invitation by the French officials in order to save time (FD). His strategy was to remain as internal as possible, that is, to avoid any controversial interpretations. Again, he did not want to expose his work, but show by means of examples what it accomplished. Here is how he formulated the idea for his lecture in his notes:

Combination of non-trivial mathematics and interesting economics (...). Focus on the mathematical examples that solved various economic problems: Pareto Optimum – Minkowski; Existence – Brower-Kakutani; Core - Lyapunov. Give precise history of the mathematics involved. (DPC 8)

And then, Stockholm:

The trip to Stockholm which we all took together was magical in many ways. We attended the ceremonies, shook hands with the king and queen, were interviewed on television, had banquets at the royal palace and the embassies, and danced at the ball. My sister's 8 year old son, Jeremy, lost a baby tooth in the middle of the banquet with the king and queen and brought his bloody tooth wrapped in a white linen napkin for his grandfather and the queen to see. (Chantal Debreu 2005)

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<sup>16</sup> Others in the Neo-Walrasian community, who profited from the prize without being exposed to it, had no reason to intervene in public. When I asked Hildenbrand whether the prize reinforced the misunderstandings that the Sonnenschein-Mantel-Debreu results could not clear up, he replied: “I can't say, since I was not in contact with those people. For us, in our circle, it was clear. And if you get upset because of every misinterpretation, then you had to rectify continuously, in whatever publication you look at.” (WH)

Though letting oneself celebrate is not a test for meeting the reasons for being celebrated, being able to relate to the reasons in some way helps for enjoying the festivities. In the most shining moment, after the King of Sweden handed over the prize, and the committee's reasoning was read aloud, Debreu, too, referred to the invisible hand in his banquet speech. Surprisingly, he used it in order to show how *little* he could relate to the reasons for giving him the prize.

(A) scientist knows that his motivations are often weakly related to the distant consequences of his work. The logical rigor, the generality, and the simplicity of his theories satisfy deep personal intellectual needs, and he frequently seeks them for their own sake. But here, as in Adam Smith's famous sentence, he seems to be 'led by an invisible hand to promote an end which was not part of his intention', for his personal intellectual fulfillment contributes to promoting the social interest of the scientific community. (...) It was my great fortune to begin my career at a time when economic theory was entering a phase of intensive mathematization and when, as a result, the strength of that invisible hand had become irresistible. (Debreu 1983)

By using the metaphor of the invisible hand, Debreu admitted that the primary concern of his intellectual life was less the "social interest of the scientific community" – whatever that may be. Instead, he engaged in his research for its own sake. He moreover admitted that his influence had gone far beyond his own intentions. Debreu was surprised by his success, and showed that he never considered the possible unintended consequences of mathematical virtues in economics. His Banquet Speech, when reading between the lines, was a way of saying sorry for having caused a misunderstanding about mathematical economics.

In the following months Debreu had to pay the price. He had to learn to be a public figure. "After the trip to Stockholm, it becomes clear that one must reconsider the hypothesis that the world had a temporary fit of insanity, and that one has to come to terms with the new situation," he commented at a dinner in honor of his Nobel at UC Berkeley (DPC 14). He received an enormous amount of mail, and, duty-bound as he was, he responded to all of the letters. He was addressed by people who he never would have thought could be interested in his work, who had not the slightest concern about structures in economic theory, but exclusively about the question that Debreu always carefully and rigorously avoided: What Does That Mean?

The world's expectations didn't help either. Suddenly because he had done some remarkable work in economics my father was contacted by politicians, political activists, physicists and scientists in other disciplines and even by the Pope. (Chantal Debreu 2005)

Political activists? The Pope? What did they want from Debreu? What could he have said apart from: No, sorry, I did nothing that could help you in your mission, John Paul! Debreu was helpless in finding reasons why to follow the Pope's invitation:

I remember he called me to talk about it, and he said "should I go?" I said "if you go, why are you going?" He didn't seem to be able to untangle. There was all that honor associated with these famous powerful people calling him to meet him, to talk to him. But he didn't seem to have a sense of purpose any more. What's my role in this? (CD)

It demanded all of Debreu's energies to keep his profile as low as possible – in particular in politics. After he refused to engage in political activities for decades, and never quite developed his political consciousness, now he was made a political figure. Anxious that every word he would say could be used in several ways, the strategy in dealing with this pressure was to remain as a-political as possible, which for him meant, above all, to support human rights. He supported the right to free research of single scientists such as Shkarov and Mendez, and went on a NAS human right mission to Chile to testify on behalf of Pinochet's victims – missing Koopmans's funeral. Being asked if he ever was member of a party, he replied: "No. I'm an independent. I would hesitate very much to give unqualified support to a man. That is why I have become active in human rights issues" (DPC 5) – in the transcript he added a question mark to "that is why". Debreu indeed had to learn that human rights, contrary to his belief, are a product of politics rather than of universal accord.<sup>17</sup>

In the years following the Nobel, Debreu increasingly alienated himself from the equilibrium of silence on which his life was based. Roger Hahn commented:

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<sup>17</sup> A telling example for his support of human rights as being politically neutral was his invitation by Adolfo Perez Esquivel, which turned out to be politically tinted. He made personal notes about the telephone call: "Tel. call Feb 1, 86: I told her that according to the information she sent me, and that I received on Feb. 12, Human Rights were, contrary to my initial understanding, only one of several issues to be discussed by Perez. The other issues are complex economic questions with high political coloring. I told her I would not [underlined] sponsor any of the events connected with Perez's visit." (DP additional carton 4)



When he received his Nobel Prize, his personality changed. Of course, he was very proud, anybody would be proud. But he talked about it constantly, his stature, his responsibility, and his elevated position. (RH)

Hildenbrand recalls his struggle with the public:

He once said: „Koopmans was the only one, who did it right: After three weeks he said, that’s it, no more interviews, I am no more willing to appear as a Nobel laureate in public”, and thus could continue his work. Debreu never again found his way back to regular work. (WH)

It is perhaps too much to say that Debreu regretted accepting the prize since he would not admit this feeling to himself. But there are clear signs of such struggle. In his reply to Arrow’s AEA lunch speech in 1984, he spoke about the old “cross-discipline average age of laureates”, and added:

These statistics might look bleak to future economics laureates, if it were not for the fact that the Nobel Medal has a reverse, widely underestimated, and from which it may be wise to shield the younger members of our profession. (DPC 14)

In private, Debreu gathered information about everyone who had ever rejected the Nobel Prize. As his daughter recalled, he talked to a laureate in genetics, who left academia after the prize. The most explicit remark made in public that alludes to a sense of remorse, was the following at the UC Berkeley dinner in 1987:

When that New York radio station called in the early morning of October, 17, 1983, I was not lucid enough to even think of making a cost-benefit analysis that accepting the prize should call for. When, some four hours later, the Secretary of the Swedish Academy of Sciences finally succeeded in reaching me on a constantly busy telephone, the question had answered itself in an obvious manner. (DPC 14)

During these years, some of his fellows noted a personality change. The feeling of being apart, that accompanied Debreu’s entire life, now was accentuated and reinforced. In his struggle to live up to his new identity, Debreu increasingly found it difficult to relate to his fellow men. “He began to think of himself as a public figure, rather than as a private

individual.” (RH) His wife put it in the simple words: “after he was on the top, there was only one-way he could go: and that was down. The Nobel was a catastrophe for him.” (FD) His daughter:

But from that day forward I believe he never felt he could live up to the honor that had been done him. His esteem for his own work did not match the high esteem that others had put upon it (...). It was from that time onward that I saw my father withdraw from us. He was unwilling for any of us to see him as less than he had been judged in that brief shining moment in Stockholm. He could not live up to the myth that had been created around him. We deprived him and he deprived himself of his humanity, of his right to be flawed and his right to be loved no matter the current level of his achievements. (Chantal Debreu 2005)

The exposure Debreu experienced as a Laureate accentuated the struggle that stamped his scientific life in meeting other than intellectual needs which he always kept for himself. In a letter to his friend Pierre Landrieu, in April 1984, he associated the lack of courage after the Nobel with the lack of courage at his times in Calais.

J’y ai découvert sans grande surprise mais néanmoins avec plaisir, de gens communs: Proust, Venise, Bach. J’y ai aussi découvert une profondeur et un courage que je me suis reproché de ne pas avoir perçus plus [?] à Calais. (DPC 5)

During these years after the Nobel, he, too, talked to his daughter about suicide. What a healing effect it would have had for Debreu, for the profession of economics, and for his family, if he had only refused the prize!

### **Aging**

In 1991, at age of 70, Debreu retired from UC Berkeley. The last years of continued pressure as a public figure took its costs in private: he separated from his family. He left the house to his wife and Berkeley with a suitcase in the one hand, and his new partner in the other. The separation and new relationship with a visiting professor was public at the department. Some of his colleagues had no understanding for leaving his wife at that age. It was perhaps the first time that Debreu was blamed personally. He did not know where to go, but his students would not bail on him. He called his friend Hildenbrand in Bonn. In 1994, he then finally returned to Europe, but broken – personally, and financially. The

money from the Nobel Prize was spent in his house in Walnut Creek (Berkeley), and little was left of his retirement payments from the university. Though Hildenbrand could raise some funds for him, he had to stay in University housing. The two years in Bonn were not his best. He did not work. Expectably in such a situation, his new relationship ended. Again, he was left alone – but his work was done.

After two years, Hildenbrand could no longer finance his presence. So he went to Barcelona to the University of Pompeu Fabra for another two years. In this new environment he cheered up somewhat, and even gave an introductory course. In 1997, he finally got the chance to return to Paris. With the help from Bernard Cornet, he was offered the Blaise Pascal chair at the Sorbonne, a prestigious and well-paid professorship. In addition, he found a new partner, a widow without any academic background, with whom he would spend the rest of his days. With Cornet he once more started a last, most remarkable intellectual effort. They planned the equivalent of the Bourbaki program in economics! (WH) An anonymous group of several mathematical economists should work in various fields, meet, cross-read, and publish jointly an axiomatic elucidation on the whole of economics. The group met several times, in Grenoble and other places, and two volumes have been completed, but never published.

Debreu could have taken ease at living a bourgeois life – if there had not been his broken past. He would see his wife and two daughters only once more when they tried to contact him, in 2001. But when the three women stood in front of his door, he sent them away with the words “you shouldn’t have come.”

After 2000, his physical state rapidly worsened, as became obvious to the audience of his final Blaise Pascal lecture. Also his mental state quickly crumbled into some kind of dementia. When Hildenbrand asked Debreu about his disease, he would come up with various excuses, having had a slip, or an accident. Suffering from the shame of being seen in such a sad physical and intellectual state he covered his head with the sheet as he sent visitors away (CD). In his last years, Debreu no longer recognized people. Alone.

On New Year’s Eve 2004, Debreu passed away. He was buried at Père Lachaise Cemetery in Paris. A second memorial was held on campus in Berkeley in March 2005. Kenneth Arrow spoke about their collaboration – speaking more of himself he left the impression that his role was more eminent. Graciela Chichilnisky spoke, showing how difficult it was to earn his recognition as student. Robert Anderson spoke, and noticed his perfectionism and protective character. But the most moving, and for some shocking speech

was given by his daughter, Chantal Debreu. She made public what has guided the preceding narrative – namely, the double life Debreu conducted: one with himself, the other with the world, separated by the sublime walls of mathematics through which no piece of meaning, feeling or love could pass. Silence.

My only regret is that I was not able to tell him at the end, “Papa, You came into this world to love and be loved. You deserved to be loved, not for what you achieved professionally or for your brilliant mind, but because you were my father. I love you; I have admired you and learned so much from you. I wish it had been easier for you to let that love in all along the way from all those who felt such love toward you. (Chantal Debreu 2005)

## References

- Arrow, Kenneth, and Gerard Debreu 1954. "Existence of an equilibrium for a competitive economy," *Econometrica*, 22 (3): 265-290.
- (eds.) 2001. *Landmark papers in general equilibrium theory, social choice and welfare*. Edward Elgar companion to general equilibrium. Edward Elgar.
- Arrow, Kenneth, and Leonid Hurwicz 1958. "On the stability of competitive equilibrium I," *Econometrica*, 26: 522-552.
- Bini, Piero, and Luigino Bruni 1998. "Intervista a Gerard Debreu," *Storia del Pensiero Economico*, 35: 3-29.
- Bourbaki, Nicolas 1950. "The architecture of mathematics," *The American Mathematical Monthly* 57 (4). 221-232.
- 1968 [1939]. *Elements of mathematics: theory of sets*. Reading: Addison-Wesley.
- Corry, Leo 1992. "Nicolas Bourbaki and the concept of mathematical structure," *Synthese*, 92: 315-348.
- Debreu, Gerard 1949. "Les fins du système économique," *Revue d'Economie Politique*. 600-615.
- 1956. "Market equilibrium," *Proceedings of the National Academy of Sciences*, 42/11: 876-878.
- 1959. *Theory of value: an axiomatic analysis of economic equilibrium*. New Haven, London: Yale University Press.
- 1960. "On 'An identity in arithmetic'," *Proceedings of the American Mathematical Society*, 11(2): 220-221.
- 1974. "Excess demand functions," *Journal of Mathematical Economics*, 1, 15-21.
- 1984. Banquet speech/biography. In *Les Prix Nobel*, ed. Wilhelm Odelberg. Stockholm: Nobel Foundation.
- 1987. TV-Interview with Frank Elstner, *Die stillen Stars*, screened at 8/18/1986, ZDF.

- 1991. “Random walk and life philosophy,” *The American Economist*, 35 (2).
- 2001. “Introduction,” in *Economics essays: a festschrift for Werner Hildenbrand*, eds. Gerard Debreu, Wilhelm Neuefeind, Walter Trockel. Berlin et al.: Springer.
- Debreu, Gerard and I.N. Herstein 1953. “Nonnegative square matrices,” *Econometrica*, 21(4): 597-607.
- Debreu, Gerard and Herbert Scarf 1963. “A limit theorem on the core of an economy,” *International Economic Review*, 4(3): 235-246.
- Dieudonné, Jean 1970. “The work of Nicolas Bourbaki,” *The American Mathematical Monthly*, 77 (2). 134-145.
- Düppe, Till 2010. “Debreu’s apologies for mathematical economics after 1983,” *Erasmus Journal for Philosophy and Economics*, 3 (1): 1-32.
- 2009. “Listening to the music of reason: Nicolas Bourbaki and the phenomenology of the mathematical experience,” Working paper.
- forthcoming. “Arrow and Debreu de-homogenized,” [add before publication]
- Feiwel, George. 1987. “Oral history II: an interview with Gerard Debreu,” in *Arrow and the ascent of modern economic theory*, ed. George Feiwel. London: Macmillan, 243-257.
- Finetti, Bruno de 1949. “Sulle stratificazione convesse,” *Annali I Matematica: Pura ed applicata*, Serie 4.
- 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.
- Gordon, Robert A. 1976. “Rigor and relevance in a Changing Institutional Setting,” *American Economic Review*, 66 (1): 1-14.
- Guedj, Denis 1985. “Nicolas Bourbaki, collective mathematician: an interview with Claude Chevalley,” *Mathematical Intelligencer*, 7 (2). 18-22.
- Hurwicz, Leonid 1961. “Review of *Theory of Value*,” *American Economic Review*, 51 (3): 414-417.

- Krueger, Alan B. 2003. "An interview with Edmond Malinvaud," *Journal of Economic Perspectives*, 17(1). 181-198.
- Livingston, Eric 1999. "Cultures of proving," *Social Studies of Science*, 29 (6). 867-888.
- Mirowski, Philip 2001. *Machine dreams: economics becomes a cyborg science*. Cambridge University Press.
- Mirowski, Philip, and E. Roy Weintraub 1994. "The pure and the applied: Bourbakism comes to mathematical economics," *Science in Context*, 7 (2): 245-272.
- Mirowski, Philip and Sent, Esther-Mirjam 2002. "Introduction", in Mirowski and Sent (eds.), *Science bought and sold: essays in the economics of science*. Chicago University Press.
- Resnick, Stephen A., and Richard D. Wolff 1984. "The 1983 Nobel Prize in economics: neoclassical economics and Marxism," *Monthly Review*, December: 29-46.
- Shapin, Steven 1990. "'The mind is its own place': Science and Solitude in Seventeenth-Century England," *Science in Context*, 4 (1): 191-218.
- Shubik, Martin 1961. "Review of *Theory of Value*," *The Canadian Journal of Economics and Political Science*, 27 (1): 133.
- Ward, Benjamin 1972. *What's wrong with economics?* New York: Basic Books.
- Weintraub, E. Roy 2002. *How economics became a mathematical science*. Durham and London: Duke University Press.

### *Archive material*

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

### *Interviews*

Anderson, Robert (RA). Monday, September 14, 2009. UC Berkeley.

Boiteux, Marcel (MB). Tuesday, September 28, 2010. Paris.

Debreu, Françoise (FD) and Chantal Debreu (CD). Tuesday, September 8, 2009. Walnut Creek.

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

Hahn, Roger (RH). Friday, September 18, 2009. UC Berkeley.

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

*Unpublished manuscripts*

Anderson, Robert 2005. Gerard Debreu Memorial Speech; UC Berkeley, March 4.

Debreu, Chantal 2005. Gerard Debreu Memorial Speech; UC Berkeley, March 4.