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# AN ACCOUNT OF “THE CORE” IN ECONOMIC THEORY

BY JONATHAN F. COGLIANO

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# An Account of ‘the Core’ in Economic Theory\*

Jonathan F. Cogliano<sup>†</sup>

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## Abstract

The concept of ‘the core’ originates in cooperative game theory and its introduction to economics in the 1960s as a basis for proofs of existence of general equilibrium is one of the earliest attempts to use game theory to address big questions in economics. Discovery of the core was met with enthusiasm among the community of economic theorists at the time. However, use of the core eventually waned and the concept faded into the backdrop of economic theory. This paper makes use of unpublished correspondence between Herbert Scarf, Lloyd Shapley, and Martin Shubik, as well as other archival and secondary resources, to provide an account of the development of the core and the trajectory of this concept, including those who developed it, after its initial appearance. It is found that the core’s eventual decline is explained by the combined effect of the slowing general equilibrium research program in the 1970s, the increasing prominence of non-cooperative game theory, and subtle issues with the concept that shaped Scarf and Shubik’s research programs after the 1960s.

**Keywords:** History of Economic Theory; the Core; Cooperative Game Theory; Herbert Scarf; Lloyd Shapley; Martin Shubik.

**JEL Classification Codes:** B21, B23, B31, C70, C71, D50.

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# 1 Introduction

The concept of ‘the core’ in economics was developed in the 1960s to address open concerns about the original proofs of existence of general, or competitive, equilibrium by Arrow and Debreu (1954) and McKenzie (1954). One such open concern, as D ppe and Weintraub (2014) study, was Gerard Debreu’s desire to provide a proof of existence of general equilibrium (GE) with greater generality than the initial proofs, specifically a proof that did not rely on a fixed-point theorem. As is well known, the initial GE proofs relied on fixed-points, which carry with them restrictive assumptions and are inherently non-constructive—there is no description of how the equilibrium fixed-point might come to exist or how it might be attained. The introduction of the core to economics by Herbert Scarf (1962), Lloyd Shapley, and Martin Shubik (1966) facilitated proofs of existence that did not rely on fixed-points and incorporated more explicit behavioral foundations to construct the equilibrium itself. At the time, the innovation of the core held great promise for the development of economic theory, yet the concept has faded into the background of the canon of economic ideas. On first glance, it would seem that the core was exactly what Debreu sought. This fact, and the current state of the core within economics, raises two main questions. First, where did the core come from and how was it developed to provide proofs of GE? Second, if the core was such a leap forward for economic theory, why did it eventually slip out of the central corpus of the field?

The core comes from the work in game theory being conducted at Princeton in the immediate post-World War II era and is a close descendant of the ideas found in von Neumann and Morgenstern’s (1944) *Theory of Games and Economic Behavior*. Formally, the core is a solution concept for a game involving potentially many agents who can band together in *coalitions* to block any undesired outcome of the game. The set of outcomes that no coalition of agents will block is *the core*. The use of coalitions and roots in von Neumann and Morgenstern’s work means that the core fits within *cooperative* game theory rather than the more popular non-cooperative game theory that dominates economics texts and curricula today. The core’s potential application to economics was first framed by Shubik (1959), where he provided a formalization of ideas found in Edgeworth (1881), but the full introduction of the core to economics in general, and general equilibrium in particular, is, for the most part, the result of the combined work of Scarf, Shapley, and Shubik (hereafter abbreviated as SSS where possible).

All three are products of Princeton and were influenced by the work being conducted in Princeton’s Mathematics Department in the post-war days. Scarf and Shapley were trained as mathematicians, whereas Shubik was an economist by training. Thus, the origin story of the core in economics is partly a continuation of the mathematization of economics that was nudged forward by Arrow, Debreu, and McKenzie. The development of the core as a solution concept in game theory clearly reflects developments in mathematics happening alongside developments in economics. This

makes the core in economics a case of, as Weintraub (2002, p.2) puts it, “how economics has been shaped by economists’ ideas about the nature and purpose and function and meaning of mathematics.” As much can be seen in Shubik’s recollection of the development of the core that is discussed in the presentation that follows.

The development of the core in economic theory is also an early and significant instance of the meeting of economics and game theory. Histories of game theory in the post-war era have been provided by Erickson (2015), Leonard (1992, 1994, 2010), and O’Rand (1992) and show the various points of connection between economics and game theory in the early post-war years, be it instances in linear programming, operations research, or early development of computers, or the intersection of these. All of these cases are significant in how they shaped scientific fields, including economics, in the second half of the twentieth century, but the case of the core in particular is distinct from these in how it emerges, has meaningful impact on developments in economic theory, and then fades from the limelight. Prior histories of game theory and economics do not fully address the isolated story of the core. As will be shown, the rise and fall of the core coincides with the decline of the broader GE research program and the rise of non-cooperative game theory—events that are not unrelated.

Yet another side of the story of the core in economics is that it is a concept that suffers from an “imagined past” (Wilson 2017). Practitioners of economics have created a story about the core and its place within the history of economic theory to suit later developments in the field. If one consults the textbook story of why the core is not at the center of economic theory today it would be something like “the idea of competition that underlies the theory of the core is very unstructured... because of this... core allocations are guaranteed to be Pareto optimal” (Mas-Colell, Whinston, and Green 1995, p.660). The trivial Pareto optimality of the core is viewed as a shortcoming, and while trivial Pareto optimality may be true, it is not the reason the core drifted from the center of economic theory.

Looking further into Mas-Colell, Whinston, and Green’s *Microeconomic Theory*, the core occupies one section of one chapter—and an appendix of a chapter if one includes the brief presentation of cooperative games—of the mammoth twenty-three chapter text. That is eight pages—eighteen including the aforementioned appendix—out of 970. After discussing the core, Mas-Colell et al. quickly move on to non-cooperative foundations of competitive equilibrium, steering things more toward the Nash concept of equilibrium. As Leonard (1992, p.29) notes, Nash equilibrium is the chosen “formalization of interaction” and is placed the heart of economic theory, where it holds a hegemonic position among approaches to understanding decision making and interaction (Leonard 1994, p.492). This is reflected in Kreps’s (1990) textbook, *A Course in Microeconomic Theory*. A search for the term ‘core’ in the text of Kreps (1990) comes up empty; the concept is not mentioned at all.

Varian (1992) treats the core as a topic “in general equilibrium analysis” that does not “conveniently fit” with the rest of the material in his *Microeconomic Analysis* (1992, p.387). Even Arrow and Hahn (1971)—whose book is entirely dedicated to the

topic of competitive equilibrium—only dedicate one chapter to presenting the core, and while they give more attention to its importance than other authors, their chapter on the core appears almost as a diversion. Chapter eight of *General Competitive Analysis* begins, “In the preceding chapters, and indeed in most of the remainder of the book, emphasis has been placed on the allocation of resources through a price system” (Arrow and Hahn 1971, p.183), indicating that the core is altogether different from the rest of the book and that it will appear and then disappear again.

According to the textbook history, the core is a minor brick in the edifice of modern economic theory.<sup>1</sup> This is simply not the case. The core was developed by people who played major roles in the advancement of economic theory, and it shaped the broader GE research program for years following its initial appearance. It may have fallen out of favor, but the textbook history of the core does not adequately explain this trajectory. The core’s decline in use can be attributed to changing interests among game theorists, the slowdown of the GE research program, and some subtle flaws in the core that were well understood by its creators even early on in the development of the concept and its application to GE.

A more complete understanding the place of the core in the history of economics is worthwhile for better appreciating its role in the development of GE theory, the continued mathematization of economics, and the relationship between economics and game theory. This is accomplished through an in-depth presentation of the intellectual development of the core. This account makes use of unpublished correspondence between Scarf, Shapley, and Shubik, as well as other archival resources in the *Economists’ Papers Archive* at Duke University to provide new insight on what went into the core’s development. The specific resources used are the Herbert Scarf Papers (1951-2015) and Martin Shubik Papers (1938-2017). By understanding what went into the development of the core, we can better ascertain the core’s trajectory after its initial appearance and the factors contributing to its eventual decline.

The rest of the paper proceeds as follows. Section 2 provides some brief background on Scarf, Shapley, Shubik, and the climate at Princeton that was formative for them. Section 3 provides an overview of the core and its initial appearance. Section 4 presents and discusses the correspondence between Scarf, Shapley, and Shubik underlying the development of the core. Section 5 provides some discussion of the trajectory of the core after its initial appearance and possible reasons for said trajectory. Section 6 provides some concluding remarks and discusses areas for further work.

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<sup>1</sup>The imagined past of the core is not dissimilar from that of the Nash equilibrium. As documented by Leonard (1994), it is common within economics to place Nash’s concept of equilibrium in a continuum of thought with precursors like Cournot. The implication being that Nash was informed by Cournot and others. However, Leonard (1994, p.502-503) points out that Nash was unaware of such precursors to his ideas when first working on them.

## 2 The People and the Starting Point

### 2.1 The People

The core, as well as Scarf (1930-2015), Shapley (1923-2016), and Shubik (1926-2018), are very much products of the intellectual climate of Princeton's Mathematics Department and the general drift of mathematizing economics that was emerging in the early post-war era. Shapley and Shubik both arrived at Princeton in 1949. Shapley arrived via Harvard and RAND Corporation, was supervised by Albert Tucker while at Princeton, and would remain there until he rejoined RAND in 1954 (Nasar 2011; Roth 2016). Shapley spent over twenty years at RAND before joining the economics department at UCLA in 1981, where he spent the rest of his career. Shapley was the son of renowned astronomer Harlow Shapley and studied mathematics at Harvard. Shapley made innovative and fundamental contributions to the field of game theory, garnering him praise by Robert Aumann as "the greatest game theorist of all time" (Aumann 2005, p.356). Among Shapley's many contributions is the well-known Shapley value, which provides a solution concept for cooperative games. Shapley is also known for the Shapley-Shubik power index, and for his work on stable matching with David Gale. The latter being recognized in 2012 by his selection for the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. In light of his contributions to economics, Shapley always identified as a mathematician (Serrano 2013, p.599).

Shubik came to Princeton after studying political economy at the University of Toronto and his dissertation research was supervised by Oskar Morgenstern. Shubik would be at Princeton until 1955 when he joined the Center for the Advanced Study in the Behavioral Sciences at Stanford. He would then have stints at General Electric, the Cowles Foundation in 1960, and IBM from 1961 to 1963 before joining Yale in 1963 and remaining there for the rest of his career, eventually becoming the Seymour H. Knox Professor of Mathematical Institutional Economics—a somewhat confounding title that is, nevertheless, indicative of Shubik's abiding interest in the application of mathematics to the study of institutions.<sup>2</sup> Shubik worked extensively on many topics in game theory with applications in economics, political science, and defense. These contributions include work on the core and cooperative games, general equilibrium, money and general equilibrium, voting power, and experimental games, among others. Many of these contributions were the result of joint work with Shapley.

Shubik and Shapley were close during their time at Princeton. They "roomed close to each other" and John Nash "at the Graduate College" and "there was considerable interaction" between them during these years (Shubik 1992, p.155). Shubik and Shapley worked together for decades following their time together at Princeton, pub-

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<sup>2</sup>Shubik provides a survey of the field of "mathematical institutional economics" in a note consisting of a single sentence at the beginning of a paper: "As mathematical institutional economics is deemed by many to be a contradiction in terms and is regarded by most as a nonexistent subject, the current survey is completed" (Shubik 1975, p.545 fn.1).

lishing fourteen articles spanning 1953 to 1977. Shubik (1984, p.vii-viii) notes the influence he and Shapley had on each other in describing how the dense two-volume work *Game Theory in the Social Sciences* is heavily based on their work together, and noting elsewhere that Shapley credited Shubik for what he knew of economics. Shubik's work with Shapley often involved applications of cooperative game theory to economics and the close connection between the two is cast in clear light in the correspondence studied later in this paper.

Scarf came to Princeton in 1951 after completing his undergraduate degree in mathematics at Temple University. His dissertation research was supervised by Solomon Bochner and Scarf would remain at Princeton until 1954 when he joined the RAND Corporation (Scarf 2011, p.46). After his time at RAND, Scarf would spend five years in the statistics department at Stanford before joining Yale in 1963, where he spent the rest of his career. Scarf, like Shapley, was a mathematician by training and never took a single economics course (Roberts 2015). In fact, Scarf decided fairly early on in life that he was a mathematician, waking "one fine morning" as a teenager "with the realization that [he] was a mathematician" (Scarf 2014). Scarf notes that he taught himself a great deal of mathematics while in high school, keeping his passion a secret until winning a "Mathematics Tournament offered by Temple University for all high school students in Pennsylvania" to the "shocked surprise" of his teachers and relatives (Scarf 2014). Scarf continued his self-study of mathematics on a scholarship from Temple by rarely attending class, studying on his own, and showing up for exams. He clearly did well since he was able to gain admission to the graduate program at Princeton.

Even with his training being exclusively within mathematics, Scarf became "a remarkable economist" (Shoven and Whalley 2015) and made contributions covering "inventories, the core, computation of equilibria, and integer programming" (Arrow and Kehoe 1994, p.161). He is perhaps best known for the Scarf Algorithm, which made the Arrow-Debreu-McKenzie model usable by providing a method for computing the competitive equilibrium. Scarf knew Shapley and Shubik when they overlapped at Princeton, and they were very friendly with each other, but he was not as close to Shapley and Shubik as they were to each other. Scarf did not study game theory at Princeton. He was interested in studying mathematical problems that had practical applications, but he wound up working on more abstract topics in topology for his dissertation research (Scarf 2014). It was not until later during his time at RAND that Scarf would directly encounter game theory and learn of its potential relation to general equilibrium (Leonard 2010, p.310-311 fn.43).

## 2.2 The Place

All three of SSS made lasting marks on the field of economics, and they shared a common starting point at Princeton. To provide some context for the story of the core and competitive equilibrium it is helpful to have a sense of the atmosphere at



Princeton during this time. This context is important for understanding how these three crossed paths, or walked them together, throughout their careers, as well as for situating the development of the core within the broader developments in economics and mathematics of the era. Scarf (2014) and Shubik (1992) provide some helpful first-hand reflections on Princeton that give a sense of the climate in the mathematics and economics departments.

Shubik (1992) notes that his intention upon arriving at Princeton was to study game theory, already having worked through von Neumann and Morgenstern's *Theory of Games and Economic Behavior* on his own in the University of Toronto's library, but he quickly found that his "enthusiasm for the potentialities of the theory of games was not shared by the members of the economics department" (Shubik 1992, p.151). As Shubik describes it, Morgenstern was alone in the economics department and interested students needed to venture to the math department to get more than the one course in game theory Morgenstern was able to offer (Shubik 1992, p.151-152). Adding to the status of game theory in Princeton's economics department was Morgenstern's lack of popularity caused by his antagonistic style of interaction with peers (Leonard 1994, p.494).

Both Scarf (2014) and Shubik (1992) note that the math department was not necessarily dedicated to the advancement of game theory, but it was not going to stifle any potentially fruitful new ideas. In Princeton's math department, those working on game theory in some way, included Richard Bellman, Albert Tucker, Harold Kuhn, David Gale, Sam Karlin, and John Nash, with a slightly younger generation including Ralph Gomory and Robert Aumann. Most of the first generation was present when Shapley and Shubik arrived and Scarf was a member of the younger generation. Shubik describes the atmosphere among this group as "electric" compared to the economics department's "business-as-usual conservatism of a middle league conventional Ph.D. factory" (Shubik 1992, p.153).

von Neumann and Morgenstern's book was influential not just on Shubik but on the work in game theory at Princeton in general. With the benefit of hindsight, Shubik identifies four major ways that *Theory of Games and Economic Behavior* was foundational to work in game theory at the time: (1) "the theory of measurable utility"; (2) the language of decision-making, including extensive form and game tree presentation, and the "reduction of the game tree to the strategic form of the game"; (3) the two person zero-sum game; and (4) "the coalitional (or characteristic function) form of a game and the stable-set solution" (Shubik 1992, 153). The theory of measurable utility and the coalitional form of a game play a central role in the story of the core and competitive equilibrium, as will be seen in Sections 3 and 4.

First-hand accounts from Scarf and Shubik provide a sense of the atmosphere in Princeton's mathematics department at the time. It was a lively place where a vibrant exchange of ideas took place, and the excitement was clearly palpable to those engaged in the department, even if they were venturing over from other fields like Shubik did. The vibrant exchange in Princeton's mathematics department—and the

cross-pollination with the Institute for Advanced Study—led to developments in pure mathematics, applications like linear programming, and continued development of computers. Alongside these developments was the creation of game theory. As Erickson (2015, p.78-79) details, game theory did not quickly take off in economics or find a nurturing home in mathematics. In fact, game theorists often found themselves as outsiders among mathematicians working in pure mathematics, and game theory initially struggled for acceptance in economics, relying on fields like operations research and institutions like RAND and the Office of Naval Research to survive at first (Erickson 2015, p.19). Mathematics at Princeton was the place to be and even though game theory struggled for acceptance in mathematics at first, these early years were formative for the field and how it would eventually become integral to economics.

## 2.3 The Connections

The accounts referenced above can be supplemented by secondary data to provide further contextualization for the key figures in the development of the core. Crucial to this contextualization is knowing the people SSS worked and interacted with, the institutions they were connected to, and how their ideas spread through their students, colleagues, and institutional connections. Following Claveau and Herfeld (2018) and Herfeld and Doehne (2018), contextualization beyond first-hand accounts can take the form of a network analysis detailing the personal and institutional connections of Scarf, Shapley, and Shubik. This is done by combing the Mathematics Genealogy Project (2018) to build a dataset capturing relevant information on the faculty at Princeton who supervised SSS and students the three advised, including the institutions where advising took place and the students' placements thereafter, including subsequent moves to other institutions after initial placement. This direct genealogical information is supplemented by including other key figures from Scarf, Shapley, and Shubik's time at Princeton, i.e. people like Aumann, Gale, Kuhn, Nash, and von Neumann. The data constructed from the Mathematics Genealogy Project are then arranged in Mathematica and network maps are generated using Gephi.<sup>3</sup>

The network map below in Figure 1 shows the personal and institutional connections of Scarf, Shapley, and Shubik. The nodes in the diagram show either people somehow connected to the core or institutions these people were connected to. The centrality, or degree of connectedness, of an institution or person in Figure 1 is shown by the size of the node. The larger a node, the more connections to people and other institutions this node has. All of the specific connections between people and institutions are marked by lines of varying weight. Heavier weighted lines denote multiple connections between a person and institution or between people, i.e. the heavy-weighted connection between John Nash and Princeton reflects his multiple connections to the institution. Notable in the diagram is the centrality of Princeton—the largest node—but this is not surprising given the fact that everyone involved in

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<sup>3</sup>Data files are available from the author upon request.

developing the core went through Princeton. Similarly notable are the high degree of connectedness of Scarf, Shapley, and Shubik, and eventually Yale as Scarf and Shubik would spend much of their careers there.

Figure 1: Complete Network of the Core

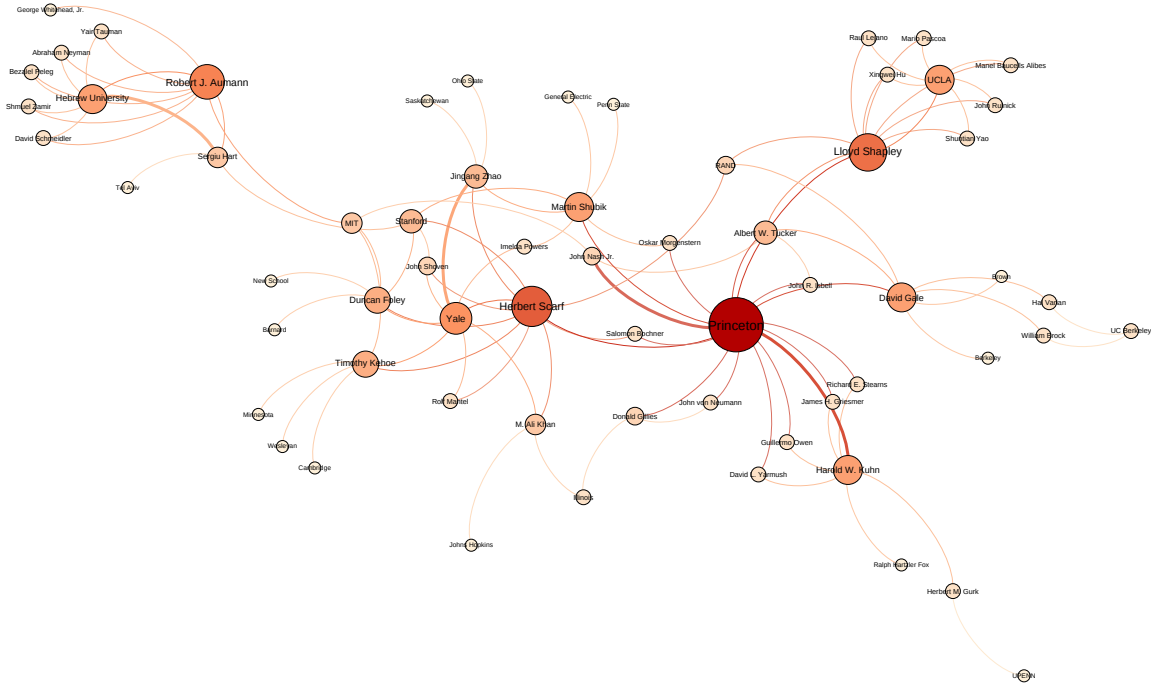
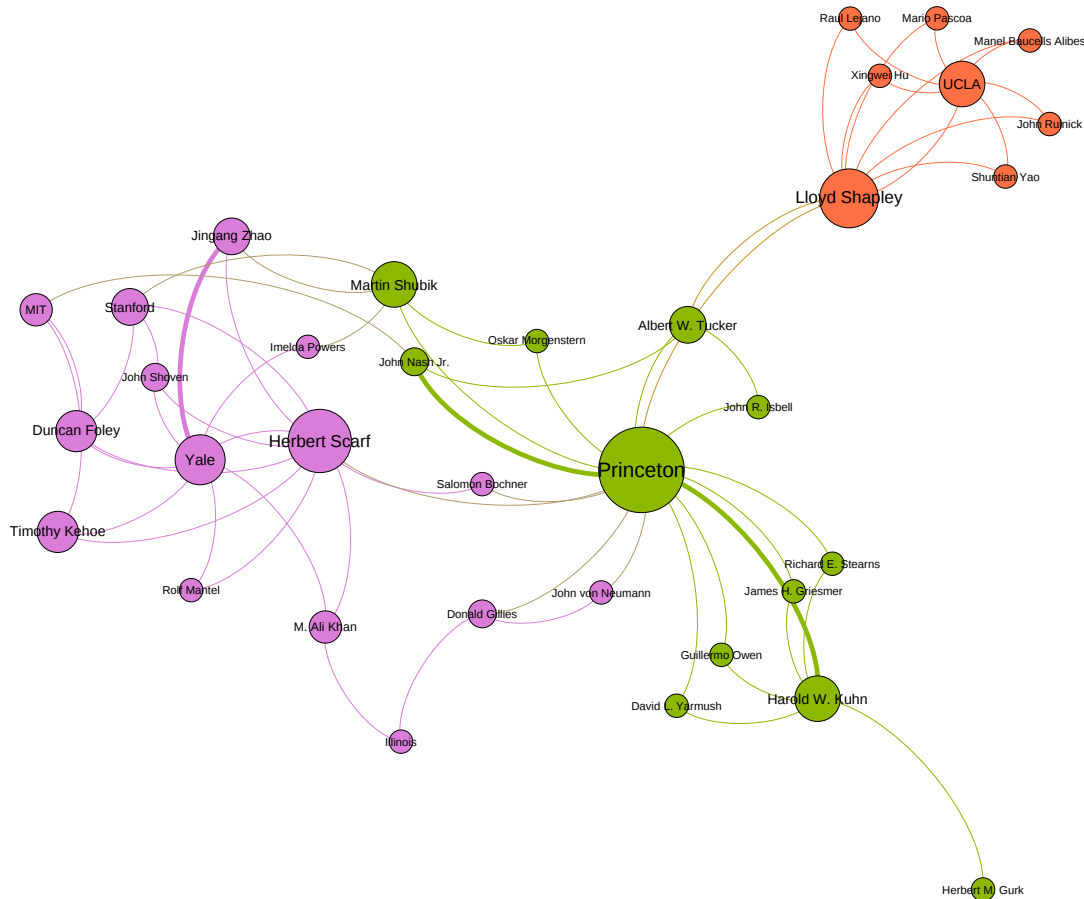


Figure 1 provides at least a partial picture of the network of people in the vicinity of work on the core. Contemporaries of SSS at Princeton are connected to them through the institution, and these figures would have contributed to the intellectual climate that led to the eventual work on the core. Some of the other institutions usually associated with early post-war developments in mathematics and economics, particularly RAND, are also shown in Figure 1 given the connections that both Scarf and Shapley (as well as others) had with the corporation. Figure 1 includes the mentors of people like Scarf, Shapley, and Shubik, as well as some prominent figures who preceded them at Princeton, the contemporaries of these three, and their students. However, this is the extent of data included in Figure 1, thus the potential range of influence of the core is not necessarily captured in the diagram. Although, this is not necessarily the intention of this particular application of network analysis. The interest in the network map of Figure 1 is to give a sense of the community of scholars and associated institutions around SSS.

Figure 2 refines the network of Figure 1 to hone in on these communities immediately surrounding Scarf, Shapley, and Shubik. This is done by running Gephi's "Modularity Class" routine to find communities within the network and then by re-

stricting the map to show only those nodes and communities with at least two degrees of connection to others. Figure 2 shows three communities around SSS: a community around Yale for Scarf, one around UCLA for Shapley, and one around Princeton for Shubik. Despite the fact that Shubik spent more time at Yale than Princeton over the course of his career, his multiple connections to the latter place him closer to the community around Princeton. Scarf is closely connected to a community at Yale given his numerous students. Shapley is closest to UCLA for similar reasons. Figure 2 is only a rough first pass at examining the communities SSS were closest to, but it does give some confirmation as to the influential role prominent figures in the development of game theory in general would have had on SSS.

Figure 2: Scarf, Shapley, Shubik Network



As Herfeld and Doehne (2018) describe, this type of use of network analysis lends itself to a relational perspective, which focuses on how the relationships between SSS and others influenced the development of the core, how it appeared in the literature, and how its impact was cemented. Despite the limited range shown in Figures 1 and 2, they also point to the interaction of developments in economics within the larger

context of developments in mathematics and computer science. This is in-line with another reason Herfeld and Doehne (2018) suggest for the use of network analysis in the history of economics: networks can easily show the “exchanges, inter-relations, and mutual influence” between fields as new ideas develop. Figures 1 and 2 fit the kind of relational perspective and context of mutual influence across people and institutions that Herfeld and Doehne (2018) describe as helpful applications of network analysis to the history of economics, and the presentation in Section 4 further reinforces the critical relationships between Scarf, Shapley, and Shubik for the development of the core.

As Figures 1 and 2 and Scarf and Shubik’s first-hand accounts indicate, Princeton was the site for the development of game theory and would serve as the launching pad for later work on the core and bringing game theory to economics. Princeton and those at the institution would also play roles in the spread of game theory through the RAND Corporation and Cowles Foundation, among other institutions. In fact, as Leonard (2010, p.324-326) notes, conferences organized by Princeton played a critical role in bringing game theory researchers affiliated with RAND and Cowles together with those at Princeton. These conferences generated multiple influential conference proceedings volumes. Two such conferences were held in 1953 and 1955. Shapley and Shubik presented at both conferences, but it was not until the 1955 conference that Scarf came into the fold. Game theory and economics grew closer at the conferences. As much is evident in the papers presented by Scarf, Shapley, and Shubik. At the 1953 conference Shubik presented “Non-cooperative Games and Economic Theory”, and at the 1955 conference “Game theory models of economic situations”. In 1955 Shapley presented “Markets as cooperative games”, and Scarf presented a paper on sequential games. Leonard (2010) documents that the 1955 conference was where Scarf and Shapley began working together on game theory, eventually producing their 1957 paper “Games with Partial Information” (1957).

Another product of the activity in game theory at Princeton during this time was the four volume book series *Contributions to the Theory of Games*. These volumes were edited by Dresher, Kuhn, Tucker, Luce, and Wolfe, with the last volume dedicated to the memory of von Neumann. The volumes contain papers by all of the major players in the early days of game theory: Aumann, Blackwell, Dresher, Gale, Gillies, Harsanyi, Karlin, Kuhn, Milnor, Nash, Raiffa, Scarf, Shapley, Shubik, Tucker, von Neumann, and others. The volumes were produced by Princeton, but the papers were funded by contracts from the Office of Naval Research and RAND, further highlighting Erickson’s (2015) and Leonard’s (1994) description of the central role these institutions (including the U.S. government) played in providing early momentum for game theory.

The papers contained within these volumes were highly influential in the development of the broad research program in game theory, but most importantly for our current focus is the appearance of a 1959 paper by Shubik in the fourth volume: “Edgeworth Market Games”. This paper initiates the research on the core and eco-

nomics that would be picked up by SSS in the conversation presented in Section 4.2. The conferences at Princeton and the book series on games provided fertile ground for the continued development of game theory and its applications in economics. As Shubik (1992) notes, it was during this time in the early-to-mid 1950s and the years immediately following that he and Shapley would begin to formulate their ideas of the core and its possible relation to economic theory.

### 3 The Core

To understand how the core found its way to GE, it is first helpful to see some of the issues with the initial proofs of existence of GE that troubled people like Debreu. Prior to the introduction of the core as a method for proving the existence of GE, proofs of existence relied on fixed-point theorems, where the price vector consistent with the competitive equilibrium  $p$  exists in a convex, compact space for which some continuous, multi-valued function  $\zeta(p) = p$ , thus  $p$  is a fixed-point. Proofs of existence of this variety, broadly speaking, can be found in the canonical contributions by Arrow and Debreu (1954) and McKenzie (1954).<sup>4</sup> In addition to convexity of the price space, this vintage of GE existence proofs relies on strict convexity of agents' preference sets. Later developments by Debreu (1956, 1959) and McKenzie (1959) are able to relax the convexity of the price space, yet still rely on fixed-point theorems and the convexity of preferences and technologies. Debreu may have been interested in existence proofs that do not rely on convexity assumptions via a general fixed-point argument, but this was never within reach for him (Düppe and Weintraub 2014, p.161).

Düppe and Weintraub (2014, p.139) attribute Arrow and Debreu's initial use of a fixed-point to Debreu's intellectual upbringing in the Bourbakian tradition of mathematics and, as a result, his predisposition for thinking in terms of topological structures, of which fixed-points are a type.<sup>5</sup> However, the fixed-point is restrictive in the sense that it provides no description of how an equilibrium is achieved. Stated differently, fixed points do not provide a constructive description of equilibrium. Debreu had an abiding interest in finding proofs of existence that did not rely on fixed-point theorems even after the publication of *Theory of Value* in 1959 (Düppe and Weintraub 2014, p.173-173, 181), yet it was not until Herbert Scarf entered the general equilibrium picture that a promising alternative to fixed-point-based proofs of existence would emerge.

Scarf came into contact with Debreu after stints at the RAND Corporation and Stanford University where he worked on general equilibrium via questions of the stability of equilibrium, which were being explored by Arrow, Leonid Hurwicz, and

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<sup>4</sup>Complete histories of the development of the initial proofs of existence of GE can be found in Düppe and Weintraub (2014) and Weintraub (1983, 2011).

<sup>5</sup>Further study of Debreu's Bourbakian upbringing in mathematics is explored by Weintraub and Mirowski (1994).

Hirofumi Uzawa at the time.<sup>6</sup> Scarf's (1960a) contribution demonstrating the possibilities for instability of general equilibrium garnered him an invitation to spend a year at the Cowles Foundation (1959-1960) where he met Debreu. As Scarf (2011, p.50) notes, this year at Cowles established a relationship with Yale University that would be formalized in the spring of 1963, with the interim years being spent at Stanford working on the concept of the core.

In 1962 Scarf's first published contribution to the concept of the core appeared in a volume from a conference hosted at Princeton; Shubik (1992, p.157) notes how difficult it is to locate these proceedings. In this paper Scarf shows that as the number of agents  $N$  in an exchange economy becomes large, the possible allocations of goods achieved through the exchange of initial endowments becomes ever smaller, with at least one allocation of goods being consistent with a competitive allocation. This set of feasible allocations that no coalition of agents will block is the core. All allocations in the core are Pareto optimal in that they represent improvements for all agents over the initial allocation of endowments, and the core contains a competitive allocation consistent with GE. In Scarf's own words:

We know that under exceptionally general conditions [(Debreu 1959)] at least one competitive allocation will always exist. Since we have reason to suspect that for a large number of participants the core will be fairly small, it seems at least reasonable that the result of Edgeworth [(1881)] will be correct in the general situation discussed at present. As we shall demonstrate, in the remainder of this paper, the result is indeed correct. As the number of participants in the market tends to infinity... the core will, in the limit, consist only of competitive allocations (Scarf 1962, p.130).

Scarf's demonstration of the existence of a competitive allocation within the core shows that for some initial set of endowments  $I$  for the  $N$  agents, if there is some set of allocations of goods  $T$  that is not in  $I$  then it can be shown that there is a hyperplane  $p$  separating  $I$  and  $T$ . With a sufficiently large  $N$ , the hyperplane  $p$  is the competitive general equilibrium since it corresponds to an allocation that "cannot be improved upon by any finite collection of consumers, on the basis of their own initial holdings" (Scarf 1962, p.149). This result proved a step forward for GE theory by setting aside the need for a fixed-point to prove the existence of a competitive allocation. The innovation of the core as a means for proofs of existence piqued the interest of Debreu, as will be discussed below, but it is interesting to note the fact that Scarf gives a great deal of credit to Martin Shubik and Lloyd Shapley in the development of the concept of the core. As Scarf states in the opening footnote to his 1962 paper:

I was very fortunate to have participated in a conversation with Lloyd Shapley

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<sup>6</sup>See Arrow and Hurwicz (1958) and Uzawa (1961).

and Martin Shubik, in which I first became aware of the problem discussed in this paper. Since that time we have had many conversations on this topic and it has become increasingly difficult for me to separate my own ideas from those contributed by Shapley and Shubik. I have also benefitted greatly from talks with Kenneth J. Arrow, Gerard Debreu, Abba Lerner, and Marc Nerlove. Professor Debreu has recently communicated to me an exceptionally elegant and simple proof of the main theorem of this paper, which will be used in place of the current proof when these results are eventually published in a journal (Scarf 1962, p.127).

The results would eventually appear in a journal in 1963 as a jointly authored piece with Debreu. This was, of course, after Debreu published a short piece “On a Theorem of Scarf” earlier in 1963 in which he relaxes some of Scarf’s (1962) assumptions, but is careful to note his thanks to “Herbert Scarf for the privilege of seeing his ideas develop in the spring of 1961. To these conversations I owe my interest in the subject of this article” (Debreu 1963, p.177).

What was the content of these conversations that clearly motivated Scarf’s work on the core and eventual collaboration with Debreu? If these conversations were so influential on Scarf, and if he owes so much to Shapley and Shubik, why do Scarf’s contributions to the core and general equilibrium, including Debreu and Scarf (1963), appear independently of those of Shapley and Shubik since they were already well-acquainted from Princeton? To flesh out the story of the core it is helpful to trace out its origins and to get a sense of the discussions happening behind its published form.

## 4 Constructing the Core

### 4.1 The Origin

The concept of the core originates in a thesis—at Princeton, of course—by Gillies (1953), and Shubik (1992, p.156) credits Shapley with coining the term “the core”, while Shapley credits Shubik with identifying “the core as a solution concept”:

I believe that Shapley named the set of undominated imputations, the core of an  $n$ -person game. I was under the impression until I talked to Shapley that it was he who suggested considering it as a solution concept by itself. He pointed out to me that the idea of the core as a solution concept in its own right came up in our conversations when (as I was the only one in the group of us who was meant to know some economics), I observed that, in essence, the idea of the set of undominated imputations was already in Edgeworth (1881) in his treatment of the contract curve, along with the idea of the replication of all players in order to study convergence (Shubik 1992, p.156-157).



Shapley named it and Shubik postulated it as a full-fledged solution concept to games with an arbitrarily large number of agents. This conversation started sometime while Shapley and Shubik were at Princeton, but it took quite some time after Princeton for the concept of the core to fully coalesce: “Sometime between 1952 and 1959 as we began to better understand what we were saying to each other... we understood the core as a separate solution concept” (Shubik 1992, p.157). This better understanding would eventually result in the presentation appearing in Shubik’s 1959 paper.

This paper is the first published point of connection between the core and economics. Reflecting on the paper, Shubik remarks that he “recognized that the treatment in Edgeworth was of a game without transferable utility” but he found it much easier to construct the game in “side payment or transferable utility” form (Shubik 1992, p.157). In his presentation, Shubik carefully considers the problem of bargaining laid out by Edgeworth in which there are two people  $a$  and  $b$  with preferences over goods 1 and 2. If person  $a$  has the endowment  $\omega_a = (x_1, 0)$  and person  $b$  has  $\omega_b = (0, x_2)$ , where  $x$  denotes the quantity of good 1 or 2, what set of exchanges and corresponding prices will these two people accept? The solution to this problem is a set of distributions of goods that will leave each person at least as well off as they would be if they made no exchanges. This solution is the classic *contract curve* seen in nearly all microeconomics textbooks. Shubik sees Edgeworth’s solution as close to von Neumann and Morgenstern’s in that it is “the set distributions which do not dominate each other but dominate all other distributions” (Shubik 1959, p.268). The example considers two people and two goods, but Edgeworth also conjectured that if the number of players became sufficiently large then the contract curve would shrink to a single distribution of the two goods, a “distribution that would correspond to the market price under pure competition” (Shubik 1959, p.269).

Shubik sets out to formalize this conjecture by constructing a game with  $N$  people and two goods, all people have the same utility function, and players are divided into two groups corresponding to persons  $a$  and  $b$  above. If the game is symmetric—equal numbers of people in each group—then as  $N$  becomes large the core will become small, approaching a single solution. This single solution is a distribution of goods  $x^*$  across the  $N$  traders that no coalition of  $N$  will block.  $x^*$  also determines a relative price  $p$ . In fact, any distribution of goods that is in the core will correspond to some price  $p$  and it is possible that multiple solutions exist, but in the case that  $N$  is large enough  $p$  is the competitive price.

Shubik’s “Edgeworth game” had some limitations. For instance, the two-good world is limiting and a more general approach would be able to consider many goods, and his game relies on transferable utility. The notion of transferable utility means just that, utility can be transferred from one player to another. As mentioned in Section 2, the notion of transferable utility is taken from von Neumann and Morgenstern (1944) and Shubik would take this notion as a description of money in his later work. The notion of transferable utility was used without event in game theory, but in economics transferable utility was unthinkable, “While this concept has been readily accepted in

game theory, it has remained foreign to the mainstream of economic thought” (Debreu and Scarf 1963, p.236). The alien nature of transferable utility to the “mainstream of economic thought” would prove troublesome for Shubik (and Shapley) and plays a role in explaining the gap in time between the publications of Scarf-Debreu (1962; 1963) and Shapley-Shubik (1966). As Shubik describes it:

A fairly standard criticism of any attempt to interest the community of economists in cooperative game theory was that the representation of a game by a characteristic function entailed the implicit or explicit assumption of the existence of a magic substance or “utility pill” with a constant marginal utility to all traders. This assumption is called the [transferable utility] assumption. The prevailing attitude of economists in the 1950s appeared to be that this assumption was so damaging as to make the application of cooperative game theory virtually useless (Shubik 1992, p.157).

This attitude made it difficult to Shapley and Shubik to publish their work on the core and competitive equilibrium in an economics journal, as is seen in their correspondence discussed below. This attitude also contributes to the separate appearance of Debreu and Scarf (1963) and Shapley and Shubik (1966).

## 4.2 The Conversation

Turning back to the conversations between SSS that Scarf (1962) acknowledges in his first paper on the core. There are a number of letters between these three available in the Herbert Scarf Papers (1951-2015) and Martin Shubik Papers (1938-2017) that shed light on the behind-the-scenes work on the core, including an intellectual disagreement over transferable utility that explains part of the story of the core. To supplement these letters are accounts provided by Scarf and Shubik in interviews and their published works. The conversation about the core begins with SSS all involved and working together, but there is a point where they eventually diverge and work the core separately. The divergence results in the separate publications of Debreu and Scarf (1963) and Shapley and Shubik (1966). The conversation between Scarf, Shapley, and Shubik is examined in three parts below. First, is the phase of the conversation where they work together, second is the phase where their paths diverge into two camps, Scarf-Debreu and Shapley-Shubik, and third is the phase after the 1963 publication by Debreu and Scarf where Shapley and Shubik struggle in getting their version published until 1966.

### 4.2.1 Scarf, Shapley, and Shubik Discover the Core

The earliest mention of the conversations Scarf (1962) acknowledges between himself, Shapley, and Shubik is in a letter from Shubik to Scarf in October 1960. Shubik (1992, p.157) recalls that these conversations began between him and Scarf “on a walk from

Columbia University to downtown New York". Scarf also recalls that after the walk downtown with Shubik the two entered an apartment where Shapley was waiting and the three of them dug into the problem of the core right away.<sup>7</sup>

In the letter dated October 26, 1960, Shubik writes "That was a fine session on Friday night: Lloyd [Shapley] and I carried on until about 5:30. We are going to attempt to have a chapter on the money and transferability nonsense".<sup>8</sup> In the same letter, Shubik also refers to an example of the disappearance of the core that the three had discussed. Scarf, in reply, says that he also enjoyed the session, and he "really think[s] that the version of the problem (find the core) that was formulated without transferable utility is really of considerable economic significance... Especially now that the stability problem is dormant, it is an interesting question to find *some* justification for the competitive equilibrium" [emphasis in original].<sup>9</sup> These letters capture two sticking points in the development of the core as a means for proofs of existence that would follow: (1) cases in which the core is empty; and (2) the transferability versus non-transferability of utility, i.e. relying on an explicit utility function versus relying only on a preference relation. The former would become a prominent theme in Scarf's broader research program and influence his later work in integer programming and the computation of equilibria. The latter would prove to be a point of disagreement between Scarf and Shapley-Shubik, which would set in motion their parallel contributions to the general equilibrium research program.

In the same letter to Shubik, Scarf writes:

I've done some calculations along these lines and sent them to Lloyd [Shapley]. Let me give you a summary. I considered the case where everyone has the same utility function, namely,  $U(x_1, \dots, x_n) = x_1 \dots x_n$ , but with different initial endowments. It turns out in this case that it is quite easy to write down the core for an arbitrary finite number of consumers. Generally, it is bigger than the competitive allocation. However, if you take an infinite sea of players, then the core shrinks down completely to the competitive point.

I'm going to try, in the next few weeks, to calculate a more general case. If I have any luck, I'll bring you up to date on the results.<sup>10</sup>

Work on showing the core's usefulness as a means for proofs of existence that were *constructive* and did not require a fixed-point theorem was underway. The notes that Scarf mentions sending to Shapley, as described in this letter, were presumably the basis for Scarf (1962) and the later Debreu and Scarf (1963).

Shapley and Shubik continued their collaboration on game theory, specifically the simulation of price-quantity games, throughout 1960, returning to the question of games and equilibrium in early 1961. In a letter dated January 11, 1961, Shubik

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<sup>7</sup>Scarf Interview with Robert Leonard, December 5, 1991. Herbert Scarf Papers, Box 2.

<sup>8</sup>Shubik to Scarf, October 26, 1960. Martin Shubik Papers, Box 8.

<sup>9</sup>Scarf to Shubik, October 31, 1960. Martin Shubik Papers, Box 8.

<sup>10</sup>Scarf to Shubik, October 31, 1960. Martin Shubik Papers, Box 8.

writes to Shapley (copying Scarf) with a brief outline of how he sees the “equilibrium problem”, noting two issues in particular: (1) “What are the conditions for which a unique competitive equilibrium has been established?”; (2) “Supposing that there is a unique equilibrium, when is it also a joint maximization if the utilities are measurable?”. Shubik raises this point “because it seems to [him] that the example ‘cooked up’ by Herb Scarf has the property that although the core of the no-side payment game will converge to the competitive equilibrium, this does not hold true of the side-payment case”.<sup>11</sup> Shubik goes on in this letter to describe how he thinks it is possible to construct a model in which the “core of the side-payment game will converge to a point on the parato [*sic*] optimal suface which will give the equilibrium solution and furthermore the core of the no-side payment game”.<sup>12</sup> Thus, to Shubik, there is a possible convergence of viewpoints where the equilibrium attained with Scarf’s approach of non-transferable utility should be the same as Shubik’s proposed avenue of transferable utility.

Scarf promptly responds unable to “understand this fixation with transferable utility” finding it “silly”, especially because it does not simplify the problem. Referring to the example he “cooked up”, Scarf continues:

I do have a positive result, which I’m almost tempted to publish. I can characterize the non-transferable core completely, in the case where all consumers have the same homogeneous, but otherwise arbitrary utility function (aside from concavity, etc), but with different initial holdings of stocks. Any fixed number of commodities is possible. Moreover, as the number of consumers tends to infinity, the core shrinks to the competitive equilibrium.<sup>13</sup>

The divergence in Scarf’s thinking from that of Shapley-Shubik was taking root and Scarf even foreshadows the eventual independent publication of their respective contributions to the core and GE. Shubik stands firm in his view in a response to Scarf, stating that he believes the transferable utility case to be easier than “explicitly solving games with non-transferable utilities”, which “may be a minor point” but is worthwhile in maintaining some connection to the “von Neumann and Morgenstern solution”.<sup>14</sup>

The conversation resumes later that year. In an undated hand-written letter (some-time before August 1961), Scarf writes to Shubik from Mexico updating him on the situation there, “I give lectures 3 times a week in a language nobody understands, and in a subject no one is interested in. . . On the core. I don’t really understand what it means yet. I think as far as I’m concerned, the next step is to calculate an example with convex consumers and non-convex producers. . . To the revolution in economic

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<sup>11</sup>The distinction between transferable and non-transferable utility is some also described in terms of the presence of ‘side payments’ or ‘no side payments’, respectively.

<sup>12</sup>Shubik to Shapley, January 11, 1961. Martin Shubik Papers, Box 8.

<sup>13</sup>Scarf to Shubik, January 19, 1961. Martin Shubik Papers, Box 8.

<sup>14</sup>Shubik to Scarf, February 16, 1961. Martin Shubik Papers, Box 8.

theory”.<sup>15</sup> Up to this point it would seem that all three were still on the same page in working together toward the “revolution”. This is confirmed when Shubik responds to Scarf on August 3 the same year, commenting specifically on the nascent idea of the core in GE and some computations that he and Shapley had done and sent to Scarf, in which they provide a “nice and reasonably general example” of how the core can disappear quite easily, “if you wish the core to disappear only when the number of people in the economy contains the fourth Mersenne number<sup>16</sup> as a factor, this too can be arranged”. Later in the letter Shubik introduces the idea that he, Scarf, and Shapley put together a joint piece on the core: “Lloyd and I feel that it would be a good idea to get a paper out in the near future. Furthermore, we are perfectly happy to either do a three way paper, otherwise Lloyd and I would do a joint paper, while you put out something of your own”.<sup>17</sup>

#### 4.2.2 Divergent Paths

This is the point at which the paths of the core diverge and distinct contributions by Scarf-Debreu and Shapley-Shubik start to take shape. As early as October 10, 1961 Scarf had been in communication with Debreu about proofs of existence using his version of the core of a game without side payments. Debreu is clearly excited by the exchange with Scarf and rattles off a series of typed and hand-written letters on October 10-11, 1961. These letters respond to a draft Scarf sent to Debreu and feature Debreu’s generalizations of some of Scarf’s proofs.<sup>18</sup> In a later letter on October 20 Debreu tells Scarf that he hopes Scarf will use his suggestions about the proofs as though Debreu were “an informal referee and incorporate them in [Scarf’s] paper”. He also suggests that if Scarf does not want to revise the paper at this point, Debreu could send his proofs as a note to the journal where Scarf’s paper is submitted to be published as a brief follow up.<sup>19</sup>

Then the most informative part of the letter comes:

The importance of your contribution and of mine are so much out of proportion as to rule out joint publication. You were a very generous friend when you considered it. In conclusion, I would be happiest if you used my comments as you pleased in your paper. But if the alterations involved look too tiresome to you, I shall also be happy to write up the proof that I sketched in my letters.<sup>20</sup>

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<sup>15</sup>Scarf to Shubik, undated. Martin Shubik Papers, Box 8.

<sup>16</sup>Mersenne numbers are prime numbers of the form  $M_n = 2^n - 1$  where  $n = 2, 3, 5, 7, \dots$  is the sequence of prime numbers.

<sup>17</sup>Shubik to Scarf, August 3, 1961. Martin Shubik Papers, Box 8. In this letter Shubik mentions attaching an outline of a proposed paper with a space reserved for Scarf should he be interested in contributing to it. However, a copy of the outline is not included in Shubik’s record of the correspondence.

<sup>18</sup>Debreu to Scarf, October 10 and 11, 1961. Herbert Scarf Papers, Box 1.

<sup>19</sup>Debreu to Scarf, October 20, 1961. Herbert Scarf Papers, Box 1.

<sup>20</sup>Debreu to Scarf, October 20, 1961. Herbert Scarf Papers, Box 1.

Debreu's letter to Scarf is in response to a letter from October 16,<sup>21</sup> thus Debreu's position on co-authoring a paper with Scarf is in response to an offer made by Scarf in the October 16 letter. Declining Scarf's offer of co-authorship and insisting on the importance of Scarf's independent contribution is interesting in light of the issues of credit for the original GE proofs detailed by D ppe and Weintraub (2014), where McKenzie arguably arrived at the proofs of existence of GE first, with a stronger claim (D ppe and Weintraub 2014, p.166), and submitted his paper for publication before Arrow and Debreu. McKenzie's paper was held up in the refereeing process by an anonymous referee who happened to be Debreu himself. Despite the delayed refereeing process, McKenzie (1954) appeared one issue before Arrow and Debreu (1954), but credit for the existence proofs often goes to Arrow and Debreu. However, by 1961 it seems that Debreu had grown more interested in ensuring credit went where it was due, particularly to Scarf in the case of the core in no-side payments games.

After the conversation between Scarf and Debreu begins, the exchange between Shapley and Shubik continues alongside it. On October 26, 1961 Shapley writes to Shubik, "I think cores are very interesting", and mentions a talk he gave at Princeton discussing "cores and solutions". Apparently, Scarf also gave a talk on this occasion at Princeton, "Herb gave an excellent talk on his stuff, but he had nothing we had not already heard".<sup>22</sup> Shapley's mention of Scarf's work as "his stuff" pointing to the now differing views on transferable utility and divergent paths of Debreu and Scarf (1963) and Shapley and Shubik (1966). The divergent paths seem clear as on November 2 Debreu writes to Scarf telling him that he will send his note following up on Scarf's proof to Robert Strotz, then editor of *Econometrica*, assuming Scarf has already sent his paper to the journal.<sup>23</sup>

Toward the end of November, Shubik writes to Shapley with an update on their project, stating the need to add a little bit more and the aim to "get the whole thing done and out within the next two to three weeks"<sup>24</sup> despite some lingering issues of finding prices and a core with production and the problem of the core not containing the competitive equilibrium in the presence of "external economies or diseconomies".<sup>25</sup> This points to a major issue for GE. If there are external economies, commonly known as *increasing returns to scale*, there will always be a core "(given convexity)" but there may not be a competitive equilibrium. It is worth noting that Scarf (1986, p.401-402) recounts finding this to be the case—in the presence of increasing returns the core is non-empty but there is no competitive equilibrium—when working on the 1963 paper with Debreu, yet the result was not published until 1986 in a volume in honor of Debreu.

<sup>21</sup>This letter is not available in the Herbert Scarf Papers.

<sup>22</sup>Shapley to Shubik, October 26, 1961. Martin Shubik Papers, Box 8.

<sup>23</sup>Debreu to Scarf, November 2, 1961. Herbert Scarf Papers, Box 1.

<sup>24</sup>Shubik to Shapley, November 20, 1961. Martin Shubik Papers, Box 8.

<sup>25</sup>Shubik to Shapley, November 22, 1961. Martin Shubik Papers, Box 8.

The lack of competitive equilibrium in the situation of increasing returns is typically thought to be a result of non-convexity introduced by the fixed costs that make increasing returns possible. Shubik comments on this to Shapley, continuing the earlier line of discussion:

There is possibly one more small section to come, but otherwise this is the works. We had the good fortune to have six meaningless articles all appear in the October 1961 issue of the *Journal of Political Economy* (you should look this up; it is in the library). They are all on various misinterpretations of non-convexity. I think that the stuff we have for Chapter 7 is really at least three different articles, the first one of which we now have pretty well complete; that is the non-convexity article. We should group the stuff together on this fairly fast and probably send it to The J.P.E.<sup>26</sup>

Meaningless articles in the esteemed *Journal of Political Economy*? The articles in question, in order of appearance, are Koopmans (1961), Bator (1961a), Farrell (1961b), Bator (1961b), Rothenberg (1961), and Farrell (1961a), which only span from pages 478 to 493 of the issue. These are more comments and short reflections than full-fledged articles, all of which revolve around and respond to questions posed in an earlier article by Farrell (1959).

Farrell raises the question of whether or not the convexity assumptions on preferences and production sets are truly necessary for a competitive equilibrium to be optimal. The discussion in the October, 1961 issue of the *JPE* highlights the potential problems introduced by non-convexity, but Shubik seems to think that this discussion is missing the mark. Is it the focus on increasing returns in production that is misguided? Is it the emphasis on preferences and, inherently, the inclusion of non-transferable utility? Shubik's letter to Shapley on November 27, 1961 does not shed much light on what renders the *JPE* discussion meaningless, but in the next available letter to Shapley, Shubik suggests, as a publication strategy for their project, four separate articles: (1) a non-technical paper for *JPE*; (2) another more technical piece; (3) a non-mathematical piece aimed at the *American Economic Review*; and (4) possibly a contribution to the pending volume in honor of Oskar Morgenstern.<sup>27</sup>

Another strand in the conversation thread between Debreu and Scarf also emerges on November 27, 1961. Scarf writes to Debreu with a conundrum about what to do with the two papers, one by Scarf and a short follow-up by Debreu. Scarf again urges Debreu to consider co-authoring a paper using their combined results. Scarf also mentions that "Shubik and Shapley have just recently obtained some very interesting results about the non-convex case," and it seems like it would be possible to do the same in the non-transferable utility case Scarf and Debreu are working on.<sup>28</sup> Debreu

<sup>26</sup>Shubik to Shapley, November 27, 1961. Martin Shubik Papers, Box 8.

<sup>27</sup>Shubik to Shapley, December 4, 1961. Martin Shubik Papers, Box 8.

<sup>28</sup>Scarf to Debreu, November 27, 1961. Herbert Scarf Papers, Box 1.

responds to Scarf on December 1 saying he “would be delighted to” write an article together, but “cannot accept” the offer in this case “because it is too generous”. In Debreu’s mind, Scarf has “proved one of the few really beautiful results of mathematical economics, and credit must go to [Scarf] alone.”<sup>29</sup> Debreu then also suggests that Scarf use Debreu’s improved proof in his paper, and assures Scarf that this “solution is fully satisfactory”. The correspondence between Debreu and Scarf drops off at this point once Debreu moves out to the west coast and he and Scarf and presumably discuss things in person. The solution for publication ultimately wound up being Scarf (1962) publishing his proof independently in a conference proceedings volume, and Debreu (1963) publishing his note on Scarf’s proof independently. Both of these appeared prior to the eventual Debreu and Scarf (1963).

There is no available correspondence between Shapley and Shubik for 1962, but there is a letter from Shubik to Robert Aumann in which he notes that:

Lloyd and I have several rather interesting results including some stuff on the competitive equilibria with non-convex preferences and also the convergence of the Nash non-cooperative equilibrium points in a closed economic model to the competitive equilibria. While the core is shrinking on the Pareto optimal surface, the non-cooperative equilibrium point comes up from below the surface until in the limit it reaches it at the competitive equilibrium point.<sup>30</sup>

By the middle of 1962 Shapley and Shubik were still plugging away at their version of the core. The lack of mention of Scarf and the date of the letter further implies that the two parties had split at this point and given up on a joint contribution.

Near the end of 1962 a letter from Strotz arrives on Shubik’s desk at IBM. Strotz asks Shubik for an update on “this business of papers by you, Debreu, Scarf, and Shapley” having already received “an ‘information’ copy of a paper by Debreu and Scarf called Technical Report No. 31 dated July 25, 1962...What gives?”<sup>31</sup> Was Strotz expecting two papers, respectively by Debreu-Scarf and Shapley-Shubik? This letter would seem to indicate as much, why else would Strotz ask “what gives”? Shubik responds to Strotz, telling him that the co-authored paper with Shapley is finished and they are just waiting on RAND to get the copies out, and that there “may have been some sort of foul up” with the paper by Debreu and Scarf since he has “heard rumors that they finished their paper and submitted it to the Japanese Journal”.<sup>32</sup> The “Japanese Journal” is, presumably, the *International Economic Review*, which was established at Osaka University in 1960 as an outlet for the growing body of technical work in economics. This was the outlet for Debreu and Scarf (1963).

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<sup>29</sup>Debreu to Scarf, December 1, 1961. Herbert Scarf Papers, Box 1.

<sup>30</sup>Shubik to Aumann, June 5, 1962. Martin Shubik Papers, Box 9.

<sup>31</sup>Strotz to Shubik, December 12, 1962. Martin Shubik Papers, Box 9.

<sup>32</sup>Shubik to Strotz, January 22, 1963. Martin Shubik Papers, Box 9.



### 4.2.3 Shapley and Shubik's Delay

In February 1962 in a letter to Strotz on another matter Shubik tells him that he will be sending him a joint paper with Shapley, which has unfortunately “become more technical than we have expected” in a week or two.<sup>33</sup> After nearly a year elapsed Shubik writes to Strotz sending him a copy of Cowles Foundation Discussion Paper No. 166, “On Prices, Fiat Money, Credit and Transferable Utility”, and telling the editor that he “can either accept this as an answer from Shapley and Shubik to the referee’s comments or you can consider it as submitted to *Econometrica* if you think it’s of sufficient interest, or you can consider it as both or neither”.<sup>34</sup> This Cowles paper was a lengthy response to the referees on Shapley and Shubik’s original submission of a paper on the core and equilibrium. The response was so extensive that it almost constituted a paper by itself, hence Shubik’s comment that Strotz could accept it as a response or as a submission. There is no response from Strotz available in Shubik’s papers.

After a lengthy turnaround, Shapley writes to Strotz on October 20, 1965 resubmitting the paper with Shubik with the new title “Quasi-Cores in a Monetary Economy with Nonconvex Preferences”. Shapley notes that the “mathematical substance is unchanged, but the exposition has been recast, particularly the first two sections, in an effort to make the conceptual bases of our model less anathematic to the anti-transferable-utility people”.<sup>35</sup> Strotz writes to Shapley (copying Shubik) saying that he is “not going to fuss around with any further refereeing” of their manuscript “but accept it for publication as it stands” and that it should appear by “July or October of [the] next year”.<sup>36</sup> The length of time between Shapley and Shubik’s conversations on their paper on the core and the appearance of the published version in 1966 shows the time it took to rework the presentation multiple times so that transferable utility could be palatable to the audience of *Econometrica*.

In the time between the start of the conversation walking through the streets of New York City and the appearance of Shapley and Shubik (1966), Scarf (1962) managed to produce a generalization of Shubik’s (1959) initial presentation of the core and get Debreu (1963) on board in further generalizing the proofs of existence. Scarf and Debreu generalized Shubik (1959) by including an arbitrarily large number of goods and by adding production. They also put together a presentation that relied only on a preference relation  $\succsim$  rather than an explicit utility function. Their presentation stuck to the view that Scarf communicated in letters to Shapley and Shubik: the convergence of the core to the competitive equilibrium could be shown in a game with non-transferable utility.<sup>37</sup> The proof that the core converges to the

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<sup>33</sup>Shubik to Strotz, February 28, 1963. Martin Shubik Papers, Box 9. Strotz follows up on this letter on March 16, 1963 expressing his anticipation of Shapley and Shubik’s paper.

<sup>34</sup>Shubik to Strotz, February 6, 1964. Martin Shubik Papers, Box 10.

<sup>35</sup>Shapley to Strotz, October 20, 1965. Martin Shubik Papers, Box 11.

<sup>36</sup>Strotz to Shapley, November 4, 1965. Martin Shubik Papers, Box 11.

<sup>37</sup>Debreu and Scarf (1963) base their version of the core without transferable utility on Aumann

competitive equilibrium without transferable utility is the biggest difference between the Scarf-Debreu and Shapley-Shubik approaches, and as Shubik (1992) describes, was more palatable to economists than the von Neumann-Morgenstern assumption of transferable utility. There is, however, another difference between the approaches that introduces an interesting facet of developments in the broader general equilibrium program.

The Shapley-Shubik approach considers non-convex preferences and shows that the core still converges to the competitive equilibrium in the presence of non-convexity. The non-convexity is introduced by the specific form of the utility function chosen, in which there is one good  $\xi$  over which individuals have the same preferences while they can have very different preferences over the other  $x_1, \dots, x_m$  goods. For Shapley and Shubik,  $\xi$  represents money, and the presence of money renders preferences non-convex. Shapley and Shubik go on to show that even in the presence of this non-convexity the core is still non-empty and contains the competitive equilibrium, but the introduction of something like money in general equilibrium is interesting in its own right. Later work in the general equilibrium program would find that the framework had some issues handling an adequate concept of money and this was, conceivably, part of the eventual slowdown of the broader research program in GE—some find that the issue of money in GE remains an open matter, see Starr (2012).

## 5 Discusson

### 5.1 General Equilibrium After the Core

By the time the above discussed work on the core appeared in published form it seemed as though the general equilibrium research program was on the upswing and bound for a growing body of fruitful work. The core allowed for proofs of existence of the competitive equilibrium without the need for a fixed-point theorem. The general equilibrium research program still had quite a bit of steam left in it, even if it was destined to eventually dry up. General equilibrium is still a widely taught set of ideas and a concept that is studied and applied in a number of ways, but the core is rarely the focus of much attention. If the core allowed freedom from the fixed-point, why did it fade away?

There are two primary explanations for the fading of the core: the shift in the focus of game theorists to non-cooperative games; and the fizzling of the GE research program. The history of the former is well documented in Erickson's (2015) account of the rise of the Nash program in game theory. The latter was partly due to the Sonnenschein-Mantel-Debreu results concerning the problems of aggregate excess demand functions, and also due to the problems of finding reasonable dynamics to actually arrive at a competitive equilibrium.<sup>38</sup>

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(1961) and Aumann and Peleg (1960).

<sup>38</sup>To further emphasize the reach of Scarf's influence on economic theory, Rolf Mantel was a

After the core initially appeared the broader research program in GE expanded to cover a wide range of topics and applications, including asset markets, public goods, fair division, market structures, various preferences, temporary equilibria, intertemporal equilibria, international trade, further questions of stability, uncertainty, and nearly everything in between. Despite this forward progress the Sonnenschein-Mantel-Debreu results brought the efficacy of the GE framework into question in the 1970s. The conclusion of Sonnenschein-Mantel-Debreu is that the aggregate excess demand functions resulting from GE need not be rational like those at the individual level, and the equilibrium may not be unique. This result cast doubt on continued prospects for GE and contributed to a shift in interests for many economic theorists.

In addition to the problems of aggregate excess demand functions, there were lingering issues of stability and dynamics associated with GE. The stability issues first appear in the late 1950s in the work of Arrow and Hurwicz (1958, 1960, 1962), Hahn (1962), Hahn and Negishi (1962), Scarf (1960a,b), and Uzawa (1961, 1962) being done during Scarf's time at RAND and Stanford. The stability of GE was shown to be possible under conditions of weak gross substitutability, but Scarf was able to counter this possibility with some simple examples demonstrating the likelihood of cycling about the competitive equilibrium or failing to reach it unless the model starts at the equilibrium point. While these issues were dormant during the development of the core, once the core came into the picture there were some renewed questions about stable core allocations and exchange dynamics able to achieve these. Questions along these lines were explored by Graham, Jacobson, and Weintraub (1972), Graham and Weintraub (1975), and Green (1974), among others. However, there was not agreement on the best or correct set of dynamics that could achieve stable core allocations.

Close in timing to the Sonnenschein-Mantel-Debreu result and growing concerns over exchange processes capable of achieving GE was the rise of the Nash program in game theory. Erickson (2015) explains the eventual dominance of non-cooperative game theory as part of a move away from the problematic cooperative game theory. As indicated in Section 4.2, the problem of cooperative game theory for many economists was its use of transferable utility (Erickson 2015, p.67-68). The efforts of people like Scarf and Debreu to sever the connection between cooperative game theory and transferable utility may not have been enough for the field of economics to be convinced of the merits of cooperative game theory. There is also the added fact that economists, at the time, tended to like to base theories on rational behavior, and the efforts of Nash in building non-cooperative game theory were, in part, an attempt to rebuild game theory on rational behavior—a point that figures like Aumann and Franklin Fisher emphasize, as noted by Erickson (2015, p.116, 241). The later work of John Harsanyi and Reinhard Selten would also play a role in the proliferation of the non-cooperative program in game theory.

When reflecting on the developments in game theory since its early days Shubik

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student of Scarf at Yale (see Figure 1).

(2011, p.1) remarks that “Game theory has been a victim of its own successes” and that as “noncooperative theory swept the field in many applications, cooperative theory virtually disappeared from many textbooks” (Shubik 2011, p.6). Shubik’s tone implies that the disappearance of cooperative game theory is something he views as unfortunate since he stuck to his view of money as transferable utility through the publication of Shubik (1984). His overall reflection on the state of game theory in 2011 does confirm that noncooperative game theory received the most attention since the early days of game theory at Princeton in the 1950s, thus explaining part of the decline of the core in economic theory.

As game theorists’ interests shifted toward non-cooperative games work on the core and cooperative game theory started to wane. The coincident decline of cooperative game theory and GE led to the fading of the core as economic theory started to look more toward applications of non-cooperative games to economic problems. The three elements described above are part of the broad narrative of the slowdown of GE, and with it, the core. There are, however, other more subtle reasons contributing to the decline of the core that can be seen in the trajectories and concerns of Scarf and Shubik after the publications of their results.

## 5.2 Subtle Faults in the Core

There is an interesting clue about some of the reasons Scarf and Shubik grew doubtful of the core and GE in the correspondence presented in Section 4.2. This clue is the issue of non-convexity mentioned by Shubik to Shapley. It would turn out that the core could not always address issues of non-convexity of interest to economists, particularly increasing returns to scale and money.

Scarf’s work after the core revolved around the computation of equilibria (Scarf 1973)—the work for which he is, arguably, most well-known—which required the use of fixed-points. In order to compute an equilibrium one had to be able to find a fixed-point numerically, thus this allowed Scarf to focus his attention on constructive approaches to fixed-points using Sperner’s Lemma (Scarf 2011).

The computation of equilibrium became an interest of Scarf’s after discovering, during his work on the core, that if a production technology exhibited increasing returns to scale it was very easy for a competitive equilibrium to not exist. Scarf wrote up a draft of a paper on this in 1963 but did not publish it until 1986 (Scarf 1986, p.401). The published version contains a preface written after the original 1963 paper to contextualize the paper and to explain its lack of publication. In closing this preface, Scarf remarks:

In publishing this paper so many years after its writing, I am offering a public argument for my reluctantly acquired feelings that a replacement for the Walrasian model, incorporating economies of large scale production, cannot be based on the concepts of cooperative game theory. In order to obtain such a the-

ory we must have recourse either to considerations of imperfect competition and non-cooperative game theory (Hart 1982), to non-strategic equilibrium concepts (Brown and Heal 1983, 1985), or to the study of indivisibilities in production (Scarf 1981a,b, 1984) (Scarf 1986, p.406).

For Scarf, it was not just the fact that the Nash equilibrium became the more conventional tool in economic theory that led him to move away from the core and cooperative game theory, it was the fact that the core could not handle increasing returns to scale in production. For many, the existence of increasing returns is an obvious empirical reality of modern economies, but it is somewhat curious that one of the biggest advances in economic theory of the post-war era could not handle this, or maybe it is not so curious since the core was grafted onto a set of ideas that someone like Debreu never intended to represent the economy in reality (Weintraub 2002, p.121).

Scarf's reaction to the problem of increasing returns led him to the computation of equilibrium, but integer programming in particular since the kinds of problems posed by increasing returns (or minimum activity levels necessary to make an activity profitable) could be viewed as problems of integer programming. Scarf's concerns about the problem of increasing returns is apparent in a letter to Ralph Gomory on August 29, 1991, where he states that he has been "intrigued for almost thirty years by the problems posed for economic theory by the presence of increasing returns to scale in production", noting the typical convexity assumptions in standard presentations of competitive equilibrium. Scarf also explains that he "always found the assumption of convex production sets unrealistic" because of the obvious "economic advantages of large-scale production" that have been a "central [feature] of economic life in the last two centuries", and "[i]n the presence of increasing returns to scale the competitive equilibrium will typically fail to exist". Scarf goes on to tell Gomory about the example he found in the early 1960s where increasing returns lead to a non-empty core but no competitive equilibrium, "[t]his conclusion was so dismaying" that Scarf "put it away for more than twenty years, before publishing the result in 1986" (the Scarf (1986) paper).<sup>39</sup> Later in the letter Scarf describes how the problem of increasing returns for the competitive equilibrium, and game theory's inability to solve the problem, drove him to integer programming, which still had not yet been fully connected back to the issues of increasing returns.

Further indication of the connection between the core and problems of increasing returns in Scarf's mind is shown in an interview he gave with Robert Leonard in 1991:

I was very much concerned, and I have been for a number of years, about problems induced by economies of scale in production. And I thought at one point that the notion of the core would be useful here, that you construct theoretical resolutions of problems where production took place with economies

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<sup>39</sup>Scarf to Gomory, August 29, 1991. Herbert Scarf Papers, Box 1.

of scale using the core. But as early as 1963 I had managed to prove a theorem which said: as long as you have any departures from convexity, you can always find a perfectly fine neoclassical model with economies of scale in which the core was empty, so there was no hope for that. And I began to think that one would have to make very serious departures from that mode of analysis: game theory wouldn't be useful, cooperative or noncooperative game theory for that problem.<sup>40</sup>

It seems that, for Scarf, increasing returns and non-convexity are a major problem for GE without an obvious resolution.

Part of the history of the problem of existence of equilibrium in the presence of non-convexities—of which increasing returns or specialized production are a type—has been studied by Rizvi (1991), but Rizvi's account does not cover the role that the core played in the development of the GE program. Because the core is left out of Rizvi's account the contributions of Scarf to GE and Scarf's arrival at the problems of increasing returns (and specialized production) are missing. Also missing is the story of the emergence of the core from cooperative game theory and its disappearance as non-cooperative game theory dominated the field. In his interview with Leonard, Scarf expresses deep doubts about the cogency of non-cooperative game theory, especially its trajectory post-Nash and the development of concepts like the trembling hand perfect equilibrium, saying that strands of non-cooperative game theory have “wasted the energies of a generation”.<sup>41</sup>

For Shubik, the non-convexity of interest to consider in GE was money. This is why, as mentioned in Section 4.2, Shubik thought those discussing non-convex preferences were missing the point, and why Shubik and Shapley stuck to their choice of the von Neumann-Morgenstern utility function. To them, the unique good over which everyone had the same preferences could represent money. This can be seen even in Shubik's later writings, many of which are based on work he did with Shapley. Examples of this work featuring commodity money can be found in Shubik (1973) and Shapley and Shubik (1977), as well as the two volume *Game Theory in the Social Sciences*. The latter lists only Shubik as the author, but in the acknowledgements he attributes the two volumes to “many years of joint work with Lloyd Shapley” (Shubik 1984, p.vii). Shapley's name not appearing on either volume is likely due to his perfectionist streak (Nasar 2011). In numerous letters between Shapley and Shubik—mostly from Shubik to Shapley—there are tentative plans for the papers they are working on to be chapters of a jointly authored book, but no such book appears.<sup>42</sup> It seems that *Game Theory in the Social Sciences* is what Shubik viewed as the book he had been working on with Shapley for so many years.<sup>43</sup> The use of the

<sup>40</sup>Scarf Interview with Robert Leonard, December 5, 1991. Herbert Scarf Papers, Box 2.

<sup>41</sup>Scarf Interview with Robert Leonard, December 5, 1991. Herbert Scarf Papers, Box 2.

<sup>42</sup>Various letters from Shubik to Shapley throughout the 1960s, Martin Shubik Papers, Boxes 8, 9, 10, 11, 12, 13, 14, 127.

<sup>43</sup>Shubik (1984, p.178) also recognizes the potential issues with non-convexity introduced by in-

von Neumann-Morgenstern utility function and transferable utility serving as money is present throughout much of this joint work.

The focus on the importance of money is also apparent in the massive three volume book series Shubik finished in 2011: *The Theory of Money and Financial Institutions*. While this book series is not the same GE approach in Shubik's earlier work, it is still heavily focused on a game theoretic approach to money and financial institutions. The influence of Shubik's thinking from the time during the development of the core is still apparent in these later writings.

The issue of money in GE is not unique to Shubik's work. This is an issue that others like Foley (1999), Hahn (1971, 1982), and Starr (2003, 2012) have grappled with since the Arrow-Debreu-McKenzie existence proofs, and expressed concern over whether or not money can be adequately incorporated into GE.<sup>44</sup> These concerns over money are interesting given the proximity of Foley and Starr to the developments in the core and GE. For instance, Foley (1967, 1970a,b) addresses questions around the core and public goods, fair division, and money. As recounted in Foley (1999, p.72-75) money was a central concern in much of this work on the core, and it all stemmed from Scarf's course on Mathematical Economics at Yale in the 1960s. Starr (2012, p.viii) highlights the questions around money in GE, recounting that "the most scientific conversations" at Cowles "took place in the coffee lounge", and it was here that his ongoing project to incorporate money into GE began. Starr (1969) also made strides to prove the existence of competitive equilibrium in the presence of non-convex preferences, albeit in the McKenzie (1959) vintage of GE rather than core-based proofs of existence. Thus, it seems that issues of non-convexity, money, and increasing returns were present in the conversations about GE even shortly after the initial proofs of existence—the problems may have been there from the start.

Shubik was not just concerned with issues of money and financial institutions in GE. After the initial work on the core he grew doubtful of GE in general, but particularly the class of models relying on cooperative game theory—a position somewhat contrary to the views presented in Shubik (2011). A 1975 article in *Kyklos*, "The General Equilibrium Model is Incomplete and Not Adequate for the Reconciliation of Micro and Macroeconomic Theory", is particularly telling of his evolving views on the subject as the GE program was stalling. In this article he goes through multiple problems with the current GE framework, particularly that it is "a conceptual straightjacket which makes it poorly suited for dealing with complex information conditions or with states of disequilibrium" and that the "current models of general equilibrium are a special case of a far more general set of models which use a somewhat different solution concept—that of a noncooperative equilibrium" (Shubik 1975, p.545). In this article, Shubik (1975, p.555-556) also points to the problem of money

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creasing returns, but does not provide much coverage of the issues.

<sup>44</sup>The problem of money in GE arguably pre-dates the 1954 existence proofs. For instance, the issue of the relationships between money and general equilibrium price systems was studied by Patinkin (1948, 1949, 1950, 1952) prior to the 1954 GE existence proofs.

in GE and the problems of dynamics described earlier:

The general equilibrium model describes the functioning of the system in only a “straightjacketed” state. The type of handwaving typically done in discussing recontracting or tâtonnement, the ignoring of bankruptcy conditions and the slurring over the description of information conditions are all earmarks of the fact that the general equilibrium model is unable even to fully describe the state of the economic system when in disequilibrium. (It obviously does not describe the process by which equilibrium is restored) (Shubik 1975, p.556).

The above problems also extend to the core, and even Shubik’s early work on the subject. According to Shubik (1975, p.559), “The recent work on the core and other cooperative game theoretic solutions also do not contain the key element for the formation of a competitive price system”. Here Shubik’s evolving concern about disequilibrium and processes for attaining equilibrium come into clear focus, “As an early proponent of the core. . . I am completely willing to admit that to a great extent the results on the core have helped to direct the attention away from the understanding of the *competitive process* in low information and communication decentralized markets” [emphasis in original] (Shubik 1975, p.560)—the core being a high information, communication-based solution. The crux of the 1975 paper is that Shubik now firmly believes that general equilibrium is only a subset of the broader non-cooperative game theoretic approaches to social science. Shubik’s timing coincides with the timeline Erickson (2015) presents of game theorists’ shift in interest to non-cooperative games, and the problems Shubik identifies reflect as much. Toward the end of the article Shubik (1975, p.568-570) labels many of the central developments in economic theory over prior decades as “red herrings serving to misdirect research effort away from the key aspects of understanding an economy with money and financial institutions”, these include Edgeworth’s barter model, the core, general equilibrium theory, perfect foresight, tâtonnement and recontracting, multiple equilibria, and real transactions costs. In other words, much of economic theory from the 1950s onward.

The trajectory of Scarf and Shubik’s research programs after the initial appearance of the core reflects their own concerns about unresolved problems in economic theory, but the trajectories also fit within the broader drift of the field of economics. Scarf’s work on the computation of equilibrium, which would generate an entire area of applied (or computable) general equilibrium models, and Shubik’s step toward non-cooperative games coincide with the changing interests of game theory and the deflating of the GE research program. These evolving trajectories of Scarf and Shubik also reflect their longstanding goals: Scarf’s goal of working on applied mathematical problems that were relevant to the real world; and Shubik’s interest in developing a mathematically-oriented approach to studying institutions and the economy. Specific interest in the core may have faded over time, partly due to problems that existed from the start, but for Scarf and Shubik the overarching objectives of their work remained consistent.



## 6 Conclusions

The account above presents the story of how and why the core was developed and the key actors involved. Drawing on archival evidence from Herbert Scarf and Martin Shubik, it is shown how they, along with Shapley worked to develop the core in an effort to move the general equilibrium program forward. Unlike other instances in the history of general equilibrium, there is no issue of credit here: each of Scarf, Shapley, and Shubik seem to be willing to give credit to the others. Even when Scarf-Debreu and Shapley-Shubik are working to publish separate papers on the core, there is an effort by Shubik to have these published together in the same volume. It seems that any healthy competition which emerged during this development of the core did not sour the relationships between Scarf, Shapley, and Shubik, especially since Scarf and Shubik were colleagues for decades at Yale.

The interesting issues that crop up when studying the core are its rapid emergence, clear impact, and eventual decline. The fact that the core was such an elegant way to prove the existence of GE without a fixed-point was a marvelous achievement, especially in Debreu's mind. Unfortunately, the core fell victim to the problems that GE succumbed to after the Sonnenschein-Mantel-Debreu episode. With the cogency of GE in doubt, there was only more room for non-cooperative game theory to spread and become more enmeshed in the heart of economic theory (Erickson 2015, p.253).

The disappearance of the core seems to be a story of the simultaneous propagation of non-cooperative game theory and the exhaustion of the more abstract pure theory approach to general equilibrium that was not meant to say anything about real economies, the theoretical problems being exacerbated by faults like GE's inability to address increasing returns and money. Despite the core's virtual disappearance, it is not the minor digression that a textbook might lead one to believe. It helped to push research in economic theory forward and opened up new areas of research in issues of non-convexity, fair division, public goods, and exchange procedures, to name just a brief selection. The core was also one of the first moments where game theory took aim at a significant issue in economic theory with notable success. Thus, the textbook story does not provide an adequate account of the core in relation to general equilibrium or its place within the development of economic theory.

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