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Marcos Thiago Graciani

Adding a "Non-" in Cooperative Game Theory: A Textbook History

Acrescentando um "Não-" à Teoria dos Jogos Cooperativos: Uma História de Livros-Texto

São Paulo
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Prof. Dr. Carlos Gilberto Carlotti Júnior

Reitor da Universidade de São Paulo

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Diretora da Faculdade de Economia, Administração, Contabilidade e Atuária

Prof. Dr. Claudio Ribeiro de Lucinda

Chefe do Departamento de Economia

Prof. Dr. Mauro Rodrigues Júnior

Coordenador do Programa de Pós-Graduação em Economia

Marcos Thiago Graciani

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Orientador: Pedro Garcia Duarte

Coorientador: Marcos Yamada Nakaguma

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Resumo

GRACIANI, M. T. **Acrescentando um "Não-" à Teoria dos Jogos Cooperativos: Uma História de Livros-Texto**. Tese (Doutorado) – Faculdade de Economia, Administração, Contabilidade e Atuária, Universidade de São Paulo, São Paulo, 2023.

Esta tese analisa extensivamente os livros-texto de teoria dos jogos publicados desde 1944, também descrevendo e contextualizando os principais títulos. Três "gerações" de livros-texto são centrais para entender a formação do livro-texto moderno, referentes aos períodos de 1950–1959, 1966–1970 e 1972–1978. Respectivamente, estas gerações se delimitam por grandes transições no desenvolvimento da teoria dos jogos: primeiro, pela passagem da ênfase em jogos de dois para n jogadores; segundo, por uma larga difusão de jogos para outras ciências, inclusive a área de organização industrial em economia. Além desta análise mais exploratória, também acompanhou-se como livros-texto representaram dois conceitos de solução: o equilíbrio de Nash e o núcleo, normalmente atribuído a D. B. Gillies e L. S. Shapley. Quanto ao equilíbrio de Nash, livros-texto indicam ter havido um "agrupamento" entre este conceito e o Teorema do Minimax de J. von Neumann no pós-Guerra, especialmente porque praticantes da disciplina estudavam problemas de dois jogadores. Conforme interesses transitaram para a teoria de n jogadores, contudo, o equilíbrio de Nash não foi capaz de atrair os matemáticos interessados em jogos porque o lado cooperativo da disciplina dispunha de problemas matematicamente mais interessantes. Por fim, já no início da década de 1970 é observável um uso de jogos não-cooperativos em modelos de organização industrial. Nesta linha, a aplicação já contava com elementos característicos do "*boom*" de jogos não cooperativos, como o equilíbrio perfeito em subjogos. Já quanto ao núcleo, livros-texto mostram como o conceito passou de um instrumento para se determinar a solução de von Neumann e Morgenstern para um conceito de solução independente. Também, provêm pistas sobre como o conceito terminou relegado ao final dos livros-texto modernos: aparentemente, nos modelos em que o núcleo mais obteve sucesso, acabou sendo descaracterizado, não mais mantendo uma relação com a teoria dos jogos. Na falta de uma aplicação simultaneamente estratégica em sua natureza e persuasiva dentre economistas, o principal exemplo de aplicação do núcleo continuou sendo um modelo antigo, datado de 1959, pondo em cheque a relevância do conceito dentro de uma apresentação moderna de jogos.

Palavras-chave: história da economia; livros-texto; teoria dos jogos; equilíbrio de Nash; núcleo.

Abstract

GRACIANI, M. T. **Adding a “Non-” in Cooperative Game Theory: A Textbook History**. Thesis (Doctorate) – School of Economics, Administration, and Accounting, University of São Paulo, São Paulo, 2023.

This dissertation offers an extensive documentation of game theory textbooks published since 1944 in order to analyze and contextualize the main titles. The evolution of textbooks towards the ones currently used indicates three distinct “generations,” each corresponding to the books published in 1950–1959, 1966–1970, and 1972–1978. Two important transformations in game theory lay behind these different generations: first, a switch from two- to n-person games; and second, a large diffusion of game theory to other areas of economics, such as industrial organization, and to other sciences. Besides such an exploratory analysis, the dissertation also tracked more closely how textbooks presented specific solution concepts: the Nash equilibrium and the core, usually attributed to D. B. Gillies and L. S. Shapley. Concerning Nash’s equilibrium, textbooks suggest that practitioners “confused” it and J. von Neumann’s Minimax Theorem, especially because they focused on studying two-person games in the post-War era. As their interests moved to n-person games, however, Nash’s equilibrium failed to occupy a central place in textbooks because cooperative games offered problems that were mathematically more interesting. Finally, in the early 1970s, textbooks applied non-cooperative games to industrial organization problems, at a time when those games were becoming increasingly popular. With respect to the core, textbooks show how it went from an instrument devised for helping one find a game’s solution (in von Neumann and Morgenstern’s sense) to a solution concept in its own right. The history of the core through textbooks also elucidates how this concept ended up being neglected to the final chapters of the modern texts. Seemingly, in models in which it attained most success (in general equilibrium theory), the core became characterized as not belonging to game theory; and in models in which it remained closely tied to game theory, such as of industrial organization, it failed in producing remarkable results.

Keywords: history of economics; textbooks; game theory; Nash equilibrium; core.

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Introduction

The history of game theory is familiar even to non-historians of economics. The *Theory of Games and Economic Behavior* of John von Neumann and Oskar Morgenstern promised to revolutionize economics but, despite what its authors pledged, it took decades for game theory to actually become part of economics. Moreover, the kind of game theory that made its way into economics was not that of the *Theory of Games*, but it was instead that which J. F. Nash called "non-cooperative approach" (which forbids players to team up).[1] Indeed, it is likely most graduate students of economics finish their training in game theory without knowing about von Neumann and Morgenstern's "cooperative game theory;" or perhaps they only hear their names when they study expected utility theory—and not game theory. Contemporary textbooks are artifacts of such state, as they condense most of what students ought to master to become economists. Microeconomics textbooks, such as Mas-Collel, Whinston and Green's (1995) and Jehle and Reny's (2011), and specialized textbooks, such as Fudenberg and Tirole's (1991), Myerson's (1991), and Osborne and Rubinstein's (2012), all emphasize non-cooperative games and rarely mention cooperative game theory. From a modern perspective, the non-cooperative game theory of Nash *is* game theory. To understand how such a thing came to be, it is necessary to look further into that history.

The Known History

The *Theory of Games* was a 625-page book which promised to recast economics over a new foundation, that of "games of strategy." Its opening chapter, "Formulation of the Economic Problem," is filled with bold statements about deficiencies that economics had by 1944. For example, von Neumann and Morgenstern ([1944] 2007, p. 1) said "the case of the exchange of goods, direct or indirect, between two or more persons, of bilateral monopoly, of duopoly, of oligopoly, and of free competition" are subjects whose proper definition and subsequent solution could "only" occur through game theory. Apart from its criticism, the "Formulation" also included an axiomatization of expected utility theory and a rough summary of von Neumann's way of solving games of coalition formation. Such formulation of expected utility theory would initiate a significant debate but, apart from it, the *Theory of Games* at first had little impact on economics. The War context drove what practitioners made of the *Theory of Games*, at least initially, Leonard (2010) showed.[2]

[1] To be more precise, non-cooperative games admit that players may act together, but only if their agreements are self-enforcing.
[2] See also Moscati's (2019) book, especially its Chapters 9 and 10.

The known history that followed von Neumann and Morgenstern's book has two main lines: one concerns its development of expected utility theory (in axiomatic form), and another on game theory in itself. Although nowadays economists perceive expected utility theory as a fundamental piece of both micro and macroeconomics, it was not so around 1944. When von Neumann and Morgenstern published their *Theory of Games*, economists had just finished a long debate about how should they interpret utility and preference representations (through utility functions). By then, most economists agreed that "utility" had no concrete meaning (that is, utility functions do not measure individual well-being, pleasure, or any related feeling). Besides, they also agreed utility functions were unique up to increasing transformations. The expected utility functions of von Neumann and Morgenstern were unique up to positive affine transformations—economists interpreted such property was related to utility measuring some sort of well-being. Thus, the *Theory of Games'* expected utility would cause some fuss. Although economists perceived expected utility theory as a fundamental point of the *Theory of Games*, von Neumann and Morgenstern did not think so, at least initially: von Neumann did not bother to include a demonstration for his expected utility theorem in the *Theory of Games'* first edition. That development came only in its second edition, of 1947, in an appendix.

Apart from such a detail, and as Moscati (2019, p. 148) contended, a long debate on expected utility followed the *Theory of Games'* publication: "the normative plausibility of von Neumann and Morgenstern's axioms for EUT, the descriptive power of the theory, and the nature of the cardinal utility function featured in the expected utility formula became the subject of intense debate in which all major utility theorists of the period took part". This debate lasted for around a decade and, as it came to an end, "the major outcomes of this debate were the acceptance of EUT by the large majority of utility theorists, a reconceptualization of the very notion of utility measurement, and the rehabilitation of cardinal utility in the economic theory of decision-making", Moscati (2019, p. 148) concluded.[3] This part of the story of the *Theory of Games* is critical: it demarcates a first use economists found for von Neumann and Morgenstern's book, even if was far from what it intended. This dissertation does not concern such part.

The known history has a second segment, which is related to game theory in itself. Leonard (2010) aptly put the *Theory of Games* in biographical context, making an extensive use of Morgenstern's diary, available in the Economists' Papers Archive of Duke University. Most of his work explores both von Neumann and Morgenstern's backgrounds, explaining how a gifted mathematician and an Austrian economist ended up together trying to lay new foundations for economics. Toward its end, his book

[3] Moscati (2016) also provided an account focused on Paul A. Samuelson's case, who was initially skeptical toward von Neumann and Morgesntern's axiomatic expected utility but, in exchanges with Leonard J. Savage, changed his mid.

showed something particularly important to the dissemination of game theory: Leonard (2010) displayed how the War steered mathematically-inclined researchers to push game theory forward. Importantly, they did it in a very particular way. The Armed Forces needed practical solutions for their War-related problems, and while game theory initially fostered hopes that it could solve them, quickly practitioners realized von Neumann and Morgenstern's theory would not fulfill that goal—its usefulness ended being that of reminding everyone about how important strategic considerations are.[4]

Other contributions further substantiate such narrative, viz., that game theory had only an instrumental use for some years after the *Theory of Games* was out. In special, the *History of Political Economy* supplement of 1992 brought together narratives of professional historians and also recollections of key practitioners of game theory, such as M. Shubik and H. Raiffa, telling what happened with game theory in the first two decades after the *Theory of Games'* publication. These accounts reinforce that point in Leonard's (2010) book, and they also underline another important feature of the history of game theory: Nash's know consecrated contribution took a good while to influence economics. In particular, economists ignored Nash's contribution for almost 30 years, only to later put it at their discipline's heart, what raises two questions of "how:" one regarding its neglection back when Nash published his main papers, and another concerning its eventual rise.

This part of the story appears in Giocoli's (2003) book, which focuses on how economists understood "rationality". Still, his narrative brought in discussions related to the history of game theory. Giocoli (2003, 2004, 2009) attacked both such problems of "how" regarding Nash's contribution. The disregard issue followed from a certain disciplinary image incompatibility, Giocoli (2003, 2004) pointed. Back in 1950–1951, economists cared about questions of "how and why" of economic equilibria to an extent that Nash's work, read as an application of a fixed-point theorem, could simply not satisfy. The image of economics would have to change before economists could absorb Nash's framework and equilibrium concept, in a way that guaranteeing existence would be enough in studying economic equilibria. That is, such questions of "how and why" would lose importance. But such a change does not wholly account for how non-cooperative games and Nash equilibria became staples in economic models. Later Giocoli (2009) provided other explanations for such an episode, all related to J. Harsanyi's famous modeling of incomplete information games. The takeaway lesson is that concurrent forces drove Nash's work into appreciation in economics.

This dissertation is somewhat related to Leonard's (2010) and Giocoli's (2003) accounts: it also goes through matters of early dissemination of game theory and through how economists came to embrace Nash's contribution. However, it nonetheless diverges

[4] See Chapter 13 in Leonard's (1992) book.

from Leonard's (2010) and Giocoli's (2003) books. Contrarily to what Leonard (2010) did, this text do not put much emphasis on applied game theory; that is, it focuses more on the "theory" part of game theory. Regarding Giocoli's (2003) contribution, this text does not look into rationality and decision theory—even if game theory did lay a normative agenda of characterizing what "rational behavior" is, such a feature was not central in textbooks, save for one exception or another. Another critical divergence with past contributions is that this dissertation does not put much emphasis on interpretation issues, which is central in understanding how game theory and economics ended up together. These differences are a matter of methodology: as its title suggests, this dissertation is a "textbook history;" being focused on game theory textbooks, it naturally emphasizes what game theory textbooks emphasizes. Importantly, most past textbooks were written by mathematicians, and they focused on mathematical aspects of game theory. In such a vein, points concerning rationality, decision theory, applied games, and interpretation go out of radar.

Even if game theory only became a part of economics decades after the *Theory of Games'* publication in 1944, game theory survived and changed throughout all those years. When it finally captivated economists, game theory was in a much different stage in comparison with that of 1944–1951, when von Neumann and Morgenstern launched game theory and Nash proposed a non-cooperative take for it. Until Nash's approach made its comeback, von Neumann and Morgenstern's cooperative framework reigned unchallenged, even if practitioners had some quarrels with it. During that time, concepts of game theory, such as Nash's equilibrium, were shaped and reshaped. This research is precisely about that: it provides an account of what happened with game theory before it became popular among economists, also detailing how well-known concepts sailed through large shifts of game theory for around four decades.

The Project

Historians of economics have studied both the period soon after the publication of the *Theory of Games*, and that in which Nash's contributions spread in economics. There are few studies about what happened between such endpoints. For instance, Erickson's (2015) book interpreted game theory as a bag of tricks from which different scientists, from different communities, and at different periods, borrowed elements and developed them according to their needs. Nonetheless, little is known about how that War-focused two-person game theory developed into an n-person cooperative game theory, and how it vanished as Nash's non-cooperative games gained traction. To understand such large movements, two problems are central. First, game theorists produced an enormous volume of papers and books about game theory for decades since 1944. There are no shortcuts into that volume of research, so it is troublesome to pick up routes of more specific transformations of game theory to look into. Second, most of such research happened through mathematicians' hands, so many interesting changes are hidden behind convoluted theorems and proofs.

Because of such difficulties, a textbook-based history is appealing. On the one hand, textbooks provide general snapshots of game theory. Their authors make a natural effort of observe what happened with game theory, paying attention to important changes, selecting what was most essential of them—as they must communicate what every practitioner should know—, and imposing a sort of order, fitting game theory's different pieces into a sequence of developments. Comparing textbooks of different epochs allows us to keep track of the significant movements that happened in game theory. Inherently, textbooks omit details, and such "snapshots" they provide are artificial. But considering that historians do not know much about n-person cooperative game theory of von Neumann and Morgenstern, a textbook account serves as a first map of that uncharted ground, even if only rudimentary at times. On the other hand, writing a textbook involves incorporating pedagogical practices to what research papers offer. For instance, textbook authors frequently need to explain questions of "why" they cover something, or "why" they characterize it in certain way; and they frequently compare and connect subjects, and so on. The making of a text suited for teaching and learning brings in information that is not present in theorem-and-proof papers.

This dissertation has two parts. The first is a "Survey of Game Theory Textbooks." It comprises a sequence of chapters mapping (possibly) all game theory textbooks ever published, detailing what distinguishes each "generation" of textbooks. While it leaves many questions unanswered, such a survey works as a rough guide to what happened with game theory, from its official inception in 1944 until its adoption in economics, around 1980. Its information functions is the raw material for the subsequent analysis. The second part is "Textbook Accounts of Selected Concepts," which includes more details about how textbooks presented two concepts—the Nash equilibrium and the core—in game theory through time.

Why Textbooks

Historians of economics (and of science, more broadly) have recently taken up textbooks as useful artifacts for historical inquiry. According to Vicedo (2012, p. 83), before historians of science perceived textbooks as "mere repositories for scientific knowledge." An example for such an approach appears in Kuhn's (2012, pp. 136-137) work, who argued that textbooks address an "already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written". Under Kuhn's (2012, pp. 136-

137) view, textbooks would have only a humble objective: to initiate students in "the vocabulary and syntax of a contemporary scientific language". Hence, they would be relevant only for understanding how science is communicated within classes, having no meaning for studies of how scientists produce new knowledge. This view was recently challenged. Regarding economics in particular, Giraud (2019) made pledge for examining textbooks more attentively: they might provide information about pedagogical and training practices, how scientists shape disciplines and subfields, how epistemological concerns evolve, how scientists dispute priorities, and how external influences (such as religion and politics) affect science. More generally, textbooks might be useful for understanding how economists *do* economics.

Many interesting results followed from studying textbooks and course structures.[5] For instance, Giraud (2014) analyzed Samuelson's *Economics: An Introductory Analysis* and showed that textbooks have a history of their own. Giraud (2014, pp. 135-139) described how several actors participated in what became *Economics*: broader changes in undergraduate education (as American departments lacked an "introductory text that would combine solid theoretical content with some statistical information presented in an appealing way for nonspecialists"), Samuelson's political agenda (of advocating Keynesian policies), publishers' interests, and criticisms it received. This analysis suggests that looking for what is behind textbooks is important. Later, Giraud (2019, pp. 137-138) generalized such point: to make a "thick" narrative, it is important to consider as much contextual information on textbooks as possible; textbooks are not simply shortcuts into a field, as they "are used in certain contexts and sometimes hold a cultural significance that can exceed their sole academic value."

Other inquiries on textbooks produced interesting findings. For instance, Medema (2014) wrote about textbook representations of the Coase's Theorem. While that result first appeared in a textbook in 1966, it was only in the 1970s that it consistently entered several intermediate texts in microeconomics. Still, Medema (2014, pp. 13-14) concluded that Coase's Theorem had no stable meaning: textbooks showed a great heterogeneity regarding "conceptions of the result, the nature of the underlying assumptions, the outcomes of the negotiation process, associated issues of distribution and equity, and the result's relevance". Consequently, what students learned as Coase's Theorem was contingent on what text they followed. The article shows how textbooks help to characterize how particular concepts and results evolve through time and, more importantly, that textbooks have an active role on such a process. Also interesting is Teixeira's (2014, p. 158) account, who argued that while textbooks put "emphasis on certain competences and skills and how textbooks may become oriented toward the internalization of certain methodological and epistemological aspects of a certain

[5] Collier (2019) made a similar case for using course-related materials (such as syllabi and examinations) in the history of economics.

discipline", they might shed some light on how a field's image evolves through time. Teixeira (2014, pp. 163-164) argued that MIT textbooks reflected the department's culture (which emphasized a technical presentation of topics). This culture is related to how different communities of economists felt about different tools and methods as ingredients in producing knowledge. That is, textbooks might be helpful in characterizing how practitioners perceive their discipline. In sum, Medema's (2014) and Teixeira's (2014) works indicate that closely looking at game theory textbooks might yield some new insights.

Making The Survey

Textbooks discussed in Part I, "Survey of Game Theory Textbooks," appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. To find (possibly) all game theory books, two different approaches were useful: searching book titles containing "game*" or "strateg*" (where "*" are search wildcards); and listing books from libraries' category "game theory," if they had one. Naturally, many books which were not precisely about game theory came up in such searches. A typical example regards books on "gaming," such as Richard F. Barton's *A Primer on Simulation and Gaming*, of 1970. In his book, gaming is a method used in experimentation, operations research, and teaching; as Shubik (1975b, pp. 7-8) explained in a book (also about gaming), such a thing is not equal to game theory, although both subjects are often "confused." The selection of what is and what is not game theory proceeded in a case-by-case analysis, following a rule-of-thumb: a book is a "game theory book" if it discusses what game theorists do. That is, a book is a "game theory book" if it considered a model called "game," which is inspired in the *Theory of Games*' underlying principle that people behave strategically, and whose use involves "solving the game" somehow.

Classifying whether items are just "books" or "textbooks" was more complicated. This categorization also followed a case-by-case analysis, and included three components. Textbooks are not vehicles for that part of knowledge which is consensual among researchers, as Kuhn (2012, p. 43) suggested. Especially because game theory was a new subject around 1950, and because it experienced substantial changes between 1960–1975, frequently book authors discussed themes far from general agreement. At least in what concerns game theory, textbooks and research were always entangled. Consequently, textbooks might include "frontier research." That is, if a book contains much of "frontier" subjects and results, in itself such a fact does not constitute a solid basis for calling that book *not* a textbook. However, even if textbooks might bring in points subject to debate, they should do it in an organized way. Textbooks impose an order over subjects of game theory, creating a sense of coherence that might not precisely represent what one finds in research. This is why, for example, that collections of papers are not textbooks: while they may provide a summary of what is important in game theory, their individual pieces lack cohesion among themselves.

Textbooks should also be comprehensive. That is, they should not be exceedingly focused on any one subject of game theory, and they also repeat information which is vastly available elsewhere, as defining what a "game" is, for instance. The only exception here are books which discuss "differential games" (put simply, models based on differential equations) because such a subarea of game theory became huge to an extent that game theory books discussed either games or differential games, never both. Finally, it is also important to consider that textbooks embody pedagogical moves: they don't simply spell out "the" theory of games; they frequently repeat the same idea in different ways, resorting to teaching aids. There are various examples of such aids: textbooks use simplifying assumptions which are not characteristic in published papers; they provide an array of examples, often numerical; they represent mathematical objects using graphics; and frequently they suggest exercises because, after all, textbooks have teaching as their foremost goal.

Not all textbooks are equally important, which implies that in making a survey it is necessary to select which texts merit more attention. To distinguish "how important" a textbook is, two metrics are natural: counting how many book reviews and citations each book received. While helpful, such metrics do not provide a clear answer to what textbooks are deserving of inspection. First, what it meant to be "a largely reviewed book" changed through time. For example, looking at books of 1950–1959, sampled titles received 5.3 reviews on average; looking at 1960–1969, 5.5; at 1970–1979, 2.0; and at 1980–1989, 1.2. Apart from that, some reviews should "count more" than others, depending on which journal they appeared or on who wrote them. Second, as textbooks are artifacts devised for teaching, they are naturally undercited. Exceptions do exist, but when a textbook has a significant amount of citations, it indicates more that it has original insights than anything else. It would be misleading to disregard some textbook because it did not receive many citations, as it could have been influential in ways other than offering some new content.

To select textbooks, apart from paying some attention to its numbers of reviews and citations, another strategy was central. Textbooks have a sort of family resemblance, as if any two textbooks are "parent and descendant," or "siblings," or just "distant relatives." This is so because textbooks are never fully innovative: transmitting knowledge of a given discipline involves, under normal conditions, also communicating information which is already present in existing texts. Newer textbooks "inherit" traits from older textbooks, and reasoning in such a way permits one to think in textbooks as in a genealogy. A way of selecting textbooks in light of that genealogy is working with a mix of backward and forward induction. The goal is to understand how modern game theory textbooks came to be as they are; so it is natural to look back in that textbook genealogical tree for their parents, the parents of their parents, and so on, for instance.

This way of selecting textbooks naturally produces divisions of texts into "generations." Three generations which delimit specific time-frames will be important: 1950–1959; 1968–1970; and 1972–1978. These generations follow from backward-and-forward induction for relatives of modern textbooks—Myerson's (1991), Fudenberg and Tirole's (1991), and Osborne and Rubinstein's (2012) texts; and game theory chapters of microeconomics textbooks (which concern game theory), such as in Kreps's (1990) and Mas-Collel, Whinston and Green's (1995) books—, emphasizing two characteristics they have: first, they focus in presenting non-cooperative game theory as "the" game theory economists should learn; and second, if they include something of cooperative games, it is one or two chapters about several *competing* solution concepts, much differently from chapters on non-cooperative games, in which the Nash equilibrium and its several refinements are everything one learns.

Brief Outline

The dissertation starts with Chapter 1, which is a simple sketch of some concepts of game theory. While non-cooperative games and Nash equilibria are widely known, one cannot say so of cooperative games and its solution concepts, so establishing a common language is useful. Next, Chapter 2 starts surveying game theory textbooks, paying attention to titles which appeared in 1950–1959. Most books from such a period were born at RAND Corporation, but one exception is much relevant: R. Duncan Luce and Howard Raiffa's classic *Games and Decisions*. There are two main points here. First, textbooks were "opening up" the *Theory of Games* to a larger audience. It goes without saying that von Neumann and Morgenstern's work was heavily mathematical, being impenetrable for the economists of 1944. Textbook authors sought both a "popular expression" of game theory (which anyone could read), and also slimmer volumes to train other mathematicians into game theory. Second, textbooks suggest that the *Theory of Games* was becoming sort of "dated" already by 1950–1959. Even if game theory was not a popular subject among economists, and even if many mathematicians had scorn for it (for not being "pure" mathematics), game theory accumulated a huge amount of research in such years, to an extent that some parts of the *Theory of Games* were simply not reflective of what game theorists used in their research.

The whole period of 1950–1959 was part of a two-person era: n-person games, which would be more interesting for social scientists, remained out of the radar. Chapter 3 explores the textbooks of 1968–1970, which were the first to switch to n-person games. Making a textbook organization of n-person (cooperative) game theory was far from easy.

Practitioners were not fully satisfied with von Neumann and Morgenstern's characteristic form and stable set solution, and despite it, they reigned as "the" components of cooperative games. Many alternatives existed, both concerning how to define a game as well as how to solve it, but texts focused on presenting different solution concepts. A crucial question for solution was existence, and textbooks started to discuss n-person games more or less when William F. Lucas found out that not all games admit a von Neumann and Morgenstern solution. This negative result possibly explains how similar textbooks as those of 1968–1970 would become scarce, only reappearing from around 1985 onward. Put simply, game theory needed time to reorganize itself.

These textbooks were "agonistic," meaning they covered game theory as a subject in itself, without many references to other disciplines (such as economics). After 1970, such a type of textbook became rare, and books applying game theory to different knowledge areas, including economics, boomed. Chapter 4 discusses what might be the first books effectively merging game theory and industrial organization, dated of 1972–1978. This period is particularly interesting because economists used both approaches to game theory in industrial organization: von Neumann and Morgenstern's cooperative games *and* Nash's non-cooperative games. Perhaps during that time, non-cooperative games gained an edge over their cooperative siblings. The cooperative texts were fairly ambitious, but seemingly did not please most economists. Differently, James W. Friedman brought in his presentation a mix of elements which eventually became dear to economics—extensive form games, sub-game perfect Nash equilibria, and a Folk Theorem—and, although it is difficult to measure his success, not much later he would publish two new textbooks, and non-cooperative game theory would be prospering, which sort of testify to his first book's success.

Chapters 2–4 provide a broad view of what happened in game theory. To balance them, Chapters 5 and 6 look into some details of how textbooks represented the Nash equilibrium and the core, respectively.[6] In particular, textbooks seem to add a good deal of information on the Nash equilibrium's history. The texts show that, back in 1957, practitioners interpreted the Nash equilibrium more as a generalization of von Neumann's Minimax Theorem than as a distinguished proposal in how to approach games and solve them; and later, as cooperative games were in vogue, they suggest that mathematicians did not pick up Nash's research lead because it was uninteresting from a mathematical point of view—its existence theorem was simply too easy. Chapter 6 also

[6] A note is in order here. Picking the Nash equilibrium as a focus is natural, given its prominence in modern economics. The core, however, does not enjoy a similar prestige. Initially, it would be interesting to emphasize some solution concept of cooperative game theory. A reasonable choice would be to discuss L. S. Shapley's value concept. The "Shapley value" appeared in 1953 and had nice mathematical properties any theorist longs for (for instance, of existence and uniqueness). However, in a preliminary survey, it did not seem that textbooks would provide much insight about its history. In that note, this text presents a history for the core instead.

shows some features of the core's history, in especial, showing that it became a solution concept in textbooks, whereas it was not thought to be one when it first appeared. Unfortunately, it is difficult to explain how modern game theory textbooks came to present the core the way they do (only rarely and mostly disconnected from economic models). Apparently, where the core succeeded, it lost its game theoretical content (as in general equilibrium theory); and where it remained connected to cooperative games, it did not thrive (as in industrial organization). Chapter 7 is—at it is usual—a short and simple conclusion.

1 A Quick Refresher On Game Theory

This short chapter fixes a notation and quickly introduces some notions of game theory which appear in later chapters. In modern textbooks, a primary way of "dividing" game theory consists of separating strategic-form and extensive-form games. In historical perspective, it is more meaningful to divide them into two-person and n-person games. The following subsections define what games are and different solution concepts, also indicating some of their properties.

1.1 Notation

There are three main types of game forms: strategic-, extensive-, and coalitional-form. To properly define strategic-form games, it is necessary to fix a basic notation. Consider any positive integer n, indicating how many players are in a game. The set of players in a game is $N \doteq \{1, \cdots, n\}$. Each player $i \in N$ has at his disposal a set of (pure) strategies, represented by S_i. If i is playing rock-paper-scissors, for instance, $S_i = \{$"play rock", "play paper", "play scissors"$\}$. Also, i might play a mixed strategy, choosing one of his options at random. Then, i would pick a probability distribution over his set of strategies (for notational simplicity, i would select an element of a set of his mixed strategies, Σ_i).[1] If he picks rock, papers, and scissors with equal likelihood, his mixed strategy would be $(1/3, 1/3, 1/3)$.

Sometimes it is useful to write degenerate mixed strategies (when a player chooses his move randomly, but putting probability 1 for some move); to do so, simply put brackets between a pure strategy. For instance, while "play rock" is a pure strategy, ["play rock"] is equivalent to a mixed strategy which always turns out to "play rock". Finally—as just for ease of notation—, write $S = \bigtimes_{i \in N} S_i$ and $\Sigma = \bigtimes_{i \in N} \Sigma_i$. The elements of such sets are vectors with, respectively, strategies and mixed strategies for all players. Also, it is useful to adopt a shorthand: in comparing two strategy profiles which differ in just one coordinate, as $(s_1, \cdots, s_i, \cdots, s_n)$ and $(s_1, \cdots, s_i^*, \cdots, s_n)$, it is easier to simply write (s_i, s_{-i}) and (s_i^*, s_{-i}). These notations might look cumbersome, but they are here mostly for reference—they appear in one footnote or another.

1.2 Definition of a "Game"

Strategic- and extensive-form games are part of any economist's training in game theory. The definition of extensive-forms bumps into graph theory, what makes it

[1] For a formal definition of mixed strategy, check Maschler, Solan and Zamir's (2013, p. 146) textbook.

somewhat intricate, but it is not important here.[2] Strategic-form (or normal-form) games come up frequently, so it is worthwhile to say that a strategic form-game is simply a triplet including: a set of players, N; sets of pure strategies for all players, $(S_i)_{i\in N}$; and a payoff function specifying what each player gets, $u : S \to \mathbb{R}^n$. Examples of strategic-form games follow below, in Figure 1.

The coalitional-form (or characteristic function-form) is not so well-known, so it merits an extra attention. When the *Theory of Games* studied two-person games, it represented them in strategic-form. But in considering larger games, it brought up a new definition of game which emphasized coalitions of players. These coalitions are simply subsets of players. For instance, if some game has three players ($N = \{1, 2, 3\}$), $\{1, 2\}$ is a coalition between 1 and 2, whereas $\{3\}$ is a one-player coalition. A coalitional-form game is an ordered pair consisting of a set of players, N, and a function $v : 2^N \to \mathbb{R}$.[3] This function—called *characteristic function*—says how much each coalition of players is worth. For example, if $v(\{1, 2\}) = 7$, it means that if 1 and 2 act together, they earn 7 units of payoff, independently of what other players do. In that vein, values for single-player coalitions, such as $v(\{1\})$, represent what players can secure themselves if they end up alone. Observe that v does not specify how coalitions of players distribute their winnings among themselves; it only says what amount they may distribute. Also note that, unlike strategic- and extensive-form games, coalitional-form games do not include "strategies" in any way: this form is all about picking partners and not moves, and supposedly everyone behaves optimally in the background. Examples of coalitional-form games follow in a subsequent section (see Tables 1 and 2).

1.3 Two-Person Games

There is an important way of categorizing two-person games: they might or might not be zero-sum. A zero-sum game implies that whatever Player 1 earns, Player 2 loses. Figure 1a has one example of game which is non-zero-sum: observe how in some situations both players win (for instance, if 1 plays α_1 and 2 picks β_2, they both get a positive outcome). The game in Figure 1b, in opposition, is zero-sum: if Player 1 earns 2 units of payoff, for instance, it implies Player 2 is losing 2 units.[4] Two solution concepts are most relevant for two-person zero-sum games: J. von Neumann's minimax and J. F. Nash's equilibrium point (which also applies for non-zero-sum games and larger games). Put simply, in an n-person game (for $n = 2$ or not, zero-sum or not), a strategy vector $(s_1^*, \cdots, s_n^*)$ is a (pure-strategy) Nash equilibrium if:

[2] See Maschler, Solan and Zamir's (2013, p. 43) formal definition, for instance.

[3] The characteristic function v is such that $v(\varnothing) = 0$.

[4] Note that if a game is zero-sum, its payoff function might be a real-valued function (instead of having $\mathbb{R}^2$ as its codomain). For instance, consider Figure 1b. Instead of writing $u(\alpha_1, \beta_1) = (-2, 2)$, it is simpler to write $u(\alpha_1, \beta_1) = -2$—no information is lost. This convention was a standard in past textbooks.

Figure 1 – Non-Zero-Sum and Zero-Sum Games

Player 1 \ Player 2	β_1	β_2
α_1	$-2, \underline{3}$	$1, 2$
α_2	$\underline{-1}, 0$	$\underline{2}, \underline{1}$

(a) A Non-Zero-Sum Game

Player 1 \ Player 2	β_1	β_2
α_1	$-2, \underline{2}$	$1, -1$
α_2	$\underline{-1}, \underline{1}$	$\underline{2}, -2$

(b) A Zero-Sum Game

Source: The author.

$$\forall i \in N, \forall s_i \in S_i \quad u_i(s^*) \geq s_i(s_i, s^*_{-i}) \tag{1.1}$$

This characterization means that any player's strategy in a Nash equilibrium is a best-response choice in light of what other players are choosing in equilibrium (because he is maximizing his payoff). To specify what a minimax solution is, it is necessary to first define two "values" of a given two-person zero-sum game:

$$v_1 \doteq \max_{s_1 \in S_1} \min_{s_2 \in S_2} u(s_1, s_2) \tag{1.2}$$

and

$$v_2 \doteq \min_{s_2 \in S_2} \max_{s_1 \in S_1} u(s_1, s_2) \tag{1.3}$$

Both values only differ by what comes first, either a max or a min operator. The values v_1 and v_2 are called "maxmin value" and "minmax value" of a game. Intuitively, Player 1 cannot earn less than v_1, whereas Player 2 cannot win more than v_2. All two-person zero-sum games have values which match (that is, they are such that $v_1 = v_2$), and their corresponding strategies are a game's minimax solution.

Both concepts might become more clear through an example, so revisit Figure 1b. An easy procedure to identify pure-strategy Nash equilibria consists of underlining best-response payoffs. For instance, in Figure 1b, if Player 2 plays β_1, it is a best-response for Player 1 to pick α_2; to "mark" this, it is usual to underline Player 1's payoff in such a case, which is -1. Matrix cells whose payoffs are all underlined reveal all pure-strategy Nash equilibria. In Figure 1b, only (α_2, β_1) is a Nash equilibrium. There is also a simple algorithm to identify pure minimax strategies. To do so, only consider payoffs written in yellow in Figure 1b (that is, payoffs for Player 1). If Player 1 picks α_1, he gets -2 in a worst-case scenario; and if he chooses α_2, his minimum possible payoff would be -1.

This rationale identifies row minima payoff: Figure 1b's row minima payoffs are -2 and -1. Similarly (and still looking at values in yellow), column maxima are -1 and 2—they represent best-case scenarios for Player 1. The values which "match" in row minimax and column maxima determine minimax strategies: since there is a -1 in both row minima and column maxima, Figure 1b has one minimax solution: (α_2, β_1) (whose payoff for Player 1 is -1). This "coincidence" between Figure 1b's Nash equilibrium and minimax solution is no coincident at all: in any two-person zero-sum game such equality happens.[5]

1.4 n-Person Games

To analyze coalitional-form n-person games, it is necessary to define *imputation*, which is simply a reasonable payoff distribution (that is, an element of $\mathbb{R}^n$ satisfying some rationality constraints). More precisely, a *coalitional structure* is any partition of N (to put it in words, an exhaustive arrangement of all players into coalitions). A payoff vector $x \doteq (x_1, \cdots, x_n) \in \mathbb{R}^n$ is an imputation (for some coalitional structure $\mathcal{B}$) of a game $(N; v)$ if:

$$\forall i \in N \quad x_i \geq v(\{i\}) \tag{1.4}$$

and

$$\forall S \in \mathcal{B} \quad \sum_{i \in S} x_i = v(S) \tag{1.5}$$

Respectively, both properties mean that players should earn at least what they supposedly get by playing alone, and that no coalition is leaving some payoffs undistributed. Frequently research in n-person cooperative games assume $\mathcal{B}$ equals N. Intuitively, they suppose that players of two "smaller" coalitions cannot earn less if they form a "bigger" coalition, so it always makes sense for players to form "the grand coalition" of all players. For a simple illustration, consider a three-person game in which coalitions including Player 1 and at least another player earn a unit of payoff, and other setups yield nothing. Table 1 represents such a game. An example of imputation is $(0, 0, 0)$: it might represent situations in which Player 1 does not join other players in coalitions (for example, in coalitional structure $\{\{1\}, \{2, 3\}\}$). Another imputation is $(1, 0, 0)$, as if Player 1 joins some coalition, but reap all its gains for himself. For notational ease, $X(N; v)$ will denote a set containing all imputations of a game made of a set of players N and a characteristic function v. Having discussed imputations, it is now possible to introduce solution concepts.

[5] See Maschler, Solan and Zamir's (2013, p. 115) "Theorem 4.44" for a formal statement.

Table 1 – A First Example of Cooperative n-Person Game

Coalitions (S)	Value ($v(S)$)
$\{\varnothing\}$, $\{1\}$; $\{2\}$; $\{3\}$; $\{2,3\}$	0
$\{1,2\}$; $\{1,3\}$; $\{1,2,3\}$	1

Table 2 – A Second Example of Cooperative n-Person Game

Coalitions (S)	Value ($v(S)$)
$\{\varnothing\}$, $\{1\}$; $\{2\}$; $\{3\}$	0
$\{1,2\}$; $\{1,3\}$; $\{2,3\}$; $\{1,2,3\}$	1

Two solution concepts for n-person cooperative games are central here. The core of a coalitional-form game $(N;v)$ (written as $\mathcal{C}(N;v)$) is a collection of imputations satisfying a sort of coalitional rationality:

$$\mathcal{C}(N;v) \doteq \left\{(x_1, \cdots, x_n) \in X(N;v) : \forall S \subseteq N \ \sum_{i \in S} x_i \geq v(S)\right\} \tag{1.6}$$

To put it in words, some allocation $x \in X(N;v)$ is *not* a core allocation if a subset of players perceives that x gives them a total payoff which falls short of what they could get if they formed a coalition among themselves. The game in Table 1 has only one imputation in its core: $(1,0,0)$. To see why, consider another imputation, some in which either Player 2 or 3 earn something—for instance, $(9/10, 1/10, 0)$. This is not a core allocation because payoffs of Players 1 and 3 sum to $9/10$, but their coalition is worth more than that since $v(\{1,3\}) = 1$. Importantly, not all games have a non-empty core. To exemplify it, consider a slightly different game, as represented in Table 2. The difference between both examples is that now coalition $\{2,3\}$ does not earn 0, as if Player 1 lost that sort of "edge" he had. No imputation is a core allocation here: for instance, $(1,0,0)$ is not because Players 2 and 3 are receiving zero while their coalition is worth 1; $(9/10, 1/10, 0)$ is not because now Players 2 and 3 earn $1/10$, still not getting what they are worth; and not even a symmetric distribution $(1/3, 1/3, 1/3)$ is a core allocation because, for example, Players 1 and 2 get $2/3$ and their coalition could get 1.[6] In fact, many games have empty cores, although a result provides a necessary and sufficient condition for a game to have a non-empty core.[7]

[6] In Table 2's game, for an imputation (x_1, x_2, x_3) to be a core allocation, it would be necessary that:

$$x_1 + x_2 \geq 1 \ \wedge \ x_1 + x_3 \geq 1 \ \wedge \ x_2 + x_3 \geq 1 \tag{1.7}$$

what cannot happen because, for (x_1, x_2, x_3) to be an imputation, it should satisfy $x_1 + x_2 + x_3 = 1$.

[7] This result is the Bondareva-Shapley Theorem. There is a statement of it in Maschler, Solan and Zamir's (2013, p. 697) textbook.

Apart from the core, a second important solution concept is that of the *Theory of Games*, called stable set. To define it, it is necessary to first specify what *domination* is. If $x \doteq (x_1, \cdots, x_n)$ and $y \doteq (y_1, \cdots, y_n)$ are imputations of a given game, and if S is a non-empty coalition of players, x dominates y via S (written $x \text{ dom}_S\ y$) if:

$$\forall i \in S \quad x_i > y_i \tag{1.8}$$

and

$$\sum_{i \in S} x_i \leq v(S) \tag{1.9}$$

That is, for x to dominate y via S it is necessary that a group of players, S, perceives there is a way to divide their coalition's worth among themselves in a way that they all would be better off in comparison with their situation in y. More broadly, x dominates y (without a "via S") if there is *some* coalition S for which $x \text{ dom}_S\ y$. This apparatus is enough for defining stable sets. The stable set solution is any set $\mathcal{K}$ for which, if x and y are elements of $\mathcal{K}$, then it is not true that x dominates y or vice-versa; and for any imputation z outside of $\mathcal{K}$, there is an imputation in $\mathcal{K}$ which dominates z. These two characterizations are called "internal" and "external stability" of stable sets. Again, it might be better to show how stable sets work with an example.

Revisit Table 2's game, which has a simple solution. One stable set for it includes only three imputations: $(1/2, 1/2, 0)$, $(1/2, 0, 1/2)$, and $(0, 1/2, 1/2)$. To see how they make up a solution, observe that none of such imputations dominate one another. For instance, while Player 2 is better off in $(1/2, 1/2, 0)$ than he is in $(1/2, 0, 1/2)$, he does not have a way to "secure" he should get $1/2$ alone; he can only achieve $(1/2, 1/2, 0)$ by teaming up with Player 1. But Player 1 is not strictly better off in any of those two imputations, so it is not possible to say $(1/2, 1/2, 0)$ dominates $(1/2, 0, 1/2)$ via $\{1, 2\}$. An analogous argument applies for other comparisons. Yet, it is also necessary to show all other imputations are dominated by some of its elements. For example, consider an imputation such as $(3/4, 1/4, 0)$. Here, $(0, 1/2, 1/2)$ dominates it via coalition $\{2, 3\}$: both Players 2 and 3 achieve a better situation in $(0, 1/2, 1/2)$ and they can team up against Player 1 to secure that imputation which they think is best.

Two notes about stable sets are important: not necessarily they exist, and often games yield *many* stable sets, even if they have few players. For instance, Table 1's game has *infinitely* many stable sets. Any set of imputations in which Player 1 takes a payoff $x_1 \in [0, 1]$ and in which Players 2 and 3 split what is left in a fixed proportion is a stable set. Observe that, in any of such arrangements, $(1, 0, 0)$ is a stable set imputation, what leads to another key property of stable sets. The core of such a game is $(1, 0, 0)$: any game's core is in all of its stable set solutions.

Part I

Survey of Game Theory Textbooks

2 The First Game Theory Textbooks, 1950–1959

After John von Neumann and Oskar Morgenstern ([1944] 2007) launched their *Theory of Games and Economic Behavior*, a few years passed before new books in game theory started to appear. Table 3 catalogs game theory books dated from 1950–1959, also pointing how many book reviews they received (column "Rev.") and indicating which resemble textbooks (column "Textb."). Until 1950, readers had access to game theory through the *Theory of Games* itself (published in 1944 and re-edited in 1947 and 1953), its book reviews, and some technical papers, such as Wald's (1945).[1,2,3] These books in Table 3 offered alternative introductions to game theory, apart from reading the *Theory of Games* directly, which was demanding on its readers, or reading its book reviews, naturally superficial given their limitations (of space, for example). While such books offered an alternative presentation of game theory, they did it differently from one another. To illustrate, John D. Williams's (1966) *Compleat Strategyst* was an effort of scientific diffusion among laymen, without mathematical requisites so as Barber (1954, pp. 453-454), one of its reviewers, could casually recommend it to his physician during an appointment.[4] In opposition, John C. McKinsey's (1952) *Introduction to the Theory of Games* and Samuel Karlin's (1959a, 1959b) *Mathematical Methods and Theory in Games, Programming, and Economics* were texts written by mathematicians and for mathematicians—they emphasized technical aspects of game theory, being more interested in proving statements than exploring everyday applications for games (Barber's (1954) physician would likely find them unreadable). The most distant relatives of modern textbooks are Williams's (1966), McKinsey's (1952), and Karlin's (1959a, 1959b) texts, however different they are.

Two observations are in place before proceeding. First, Blackwell and Girshick (1954, p. vii) composed "a textbook in decision theory for first-year graduate students in statistics." Still, it included two chapters on game theory—"Games in Normal Form" and "Values and Optimal Strategies in Games"—because having some knowledge of

1 Apart from adding an appendix with an axiomatic statement of expected utility theory in 1947, newer editions von Neumann and Morgenstern ([1944] 2007, pp. v-vi) mostly corrected misprints. These changes are not relevant for this survey.

2 Wald's (1945) article concerns a generalization of von Neumann's minimax theorem, extending its statement to two-person zero-sum games in which agents have infinite-dimensional strategy spaces.

3 Knowingly, RAND Corporation published a series of research papers, reports, and memoranda in game theory as early as 1948; see, for instance, a report from Bohnenblust *et al.* (1948). These were arguably less disseminated given their internal nature, even in academia.

4 Barber (1954, p. 454) recommended example 20 in the *Compleat Strategyst*, "an application of game-theory technique to the problem of selecting appropriate medication for a given set of disease symptoms."

Table 3 – List of Game Theory Books, 1950–1959

Author	Title	Year	Rev.	Textb.
J. McDonald	*Strategy in Poker, Business & War*	1950	2	✓
H. W. Kuhn and A. W. Tucker (Eds.)	*Contributions to the Theory of Games (Vol. I)*	1950	3	
J. C. C. McKinsey	*Introduction to the Theory of Games*	1952	7	✓
H. W. Kuhn and A. W. Tucker (Eds.)	*Contributions to the Theory of Games (Vol. II)*	1953	4	
D. Blackwell and M. A. Girshick	*Theory of Games and Statistical Decisions*	1954	11	✓
M. Shubik	*Readings in Game Theory and Political Behavior*	1954	2	
R. D. Luce and H. Raiffa	*A Survey of the Theory of Games*	1954	0	
J. D. Williams	*Compleat Strategyst: Being a Primer on the Theory of Games of Strategy*	1954	9	✓
S. Vajda	*The Theory of Games and Linear Programming*	1956	10	✓
R. D. Luce and H. Raiffa	*Games and Decisions*	1957	16	✓
D. Gale	*The Theory of Matrix Games and Linear Economic Models*	1957	0	✓
G. L. Thompson	*Lectures on Game Theory, Markov Chains, and Related Topics*	1958	0	✓
M. Dresher, A. W. Tucker, and P. Wolfe (Eds.)	*Contributions to the Theory of Games (Vol. III)*	1958	0	
S. Karlin	*Methods and Theory and Economics (Vol. I and II)*	1959	9	✓
A. W. Tucker and R. D. Luce	*Contributions to the Theory of Games (Vol. IV)*	1959	4	
M. Shubik	*Strategy and Market Structure*	1959	7	

Source: Each listed book appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. The number of book reviews comes from JSTOR's database and Google Scholar searches.

it was necessary insofar "Wald's mathematical model for decision theory is a special case of [. . .] game theory" (here, they referred to Wald's (1945, 1950) research on a game theoretical approach to statistical inference).[5] Likely such chapters served as a mini-textbook for statisticians, justifying why Blackwell and Girshick's (1954) functioned as a game theory textbook. Second—and more importantly—, in 1951, 1953, 1958 and 1959, the *Annals of Mathematics Studies* published volumes of a collection named *Contributions to the Theory of Games*. These books assembled original papers (not published elsewhere) of different authors researching game theory. Princeton University Press printed them, so they possibly were of easier access than internal discussion texts (for instance, as papers from the RAND Corporation). These volumes are interesting here because game theory had no specialized journal by then, so such books were central in constituting a place for new research to appear and to keep game theorists abreast of one another's work.[6] For example, in volume IV, Thompson and Thompson (1959, p. 407) compiled a bibliography list of 985 items of all "substantial" papers in game theory scattered around, excluding those in "dittoed and mimeographed form" except if they were easily accessible at larger libraries. The sheer need for such a list airs how research in game theory was dispersed: communication was essential for a new-born discipline, and textbooks played a role in substantiating it.

Textbooks in Table 3 display stark contrasts, such as that of how readable Williams's (1966) text was in face of how technical McKinsey's (1952) and Karlin's (1959a, 1959b) expositions were. However different, they fit in a context in which game theory arouse much interest in different areas, but still only a handful of people could actually read the *Theory of Games*; textbooks would recast game theory in different ways, sampling contents from von Neumann and Morgenstern's ([1944] 2007) original treatise and shaping them for particular purposes. These textbooks would become substitutes for the *Theory of Games*: there would be no need to struggle with von Neumann's reasoning any longer to partake of what game theory accomplished. Curiously, while von Neumann's mathematics played a major role in shielding the *Theory of Games* from a general audience, mathematicians would think about making game theory a subject known among non-mathematicians. Their efforts would make a persistent effect in game theory textbooks. The champion of their generation would be R. Duncan Luce and Howard Raiffa's (1957) textbook, which attracted much attention at its time and quickly became a "classic" in game theory; an artifact of its enduring relevancy is in Tower's (1990, pp. 110-112) collection of syllabi: James W. Friedman used it in his graduate

[5] Wald's (1945, 1950) research was central in the early days of game theory and decision theory (after the *Theory of Games* appeared). However, textbooks are not particularly telling about his role in both subjects.

[6] For example, the *International Journal of Game Theory*, *Games and Economic Behavior*, the *International Game Theory Review* appeared, respectively, in 1971, 1989, and 1999. Before 1971, some journals not specialized in game theory offered a considerable space for game theoretical research, such as the *Journal of Conflict Resolution*, born in 1959.

course of game theory at the University of North Carolina in 1989, more than thirty years of its first edition (which never receive an update).[7] In any case, a point is certain: to better understand game theory textbooks of 1950–1959, it is of primary importance to recognize who they addressed and how they adapted the *Theory of Games*.

2.1 Writing For Specific Audiences

The early history of game theory crosses that of World War II. Mathematicians readily started to develop game theory after the *Theory of Games* was out. As Leonard (2010, pp. 293-343) documented, at RAND Corporation game theorists privileged applications of it to military problems. In his investigation, he found game theory at first appeared to be a technical tool capable of providing accurate answers for the Cold War problems, but failing in doing so, it remained being only a reasoning framework; instead of specifically saying what militaries should do, it highlighted strategic aspects worth considering when making decisions (e.g., thinking of issues of threats and credibility). Still, amidst efforts to apply game theory for ongoing conflicts, new theoretical results emerged. Well-known examples include Nash's (1950b) equilibrium point or, to mention a contribution in cooperative games, Lloyd S. Shapley's (1953c) value concept. There are piles of contributions to game theory dating from 1944–1959, many of which appearing in RAND Corporation internal discussion documents. Besides military applied and purely theoretical work, some exercises mixing both also existed. An example is Rufus Isaacs's differential games, which played around boundaries of game theory and dynamic programming, through theorems and practical problems. What matters here is that game theory was out of von Neumann and Morgenstern's hands—newcomers could take it to any direction they wanted.

Importantly, the *Theory of Games* reads more as a project for a new discipline than as a fully realized discipline. In particular, von Neumann and Morgenstern's ([1944] 2007) work has 12 chapters which may divide the *Theory of Games* into three distinct moments. First, chapters 1 and 2 define what game theory is and lay down a proposal of revolutionizing economics. Second, chapters 3 and 4 explore von Neumann's (1928) early work on two-person games. Finally, chapters 5 onward are about n-person games. But von Neumann and Morgenstern's ([1944] 2007) work on n-person games was more about specific values of n than a widely applicable theory for all possible values of n. Their work included, for instance, chapters specialized on 3- and 4-person games, and also smaller discussions about higher values of n up to 7. This is why the *Theory of Games* is more a project: it proposed a problem for which von Neumann didn't have an answer—precisely, how should one solve n-person games—, and also suggested

[7] Friedman is a central character in later sections; he researched oligopoly theory using Nash's (1951) non-cooperative framework.

Table 4 – Thematic Distribution in *Contributions to the Theory of Games*

Volume	Year	Theme	Chapters
I	1950	Finite Games	11
		Infinite Games	4
II	1953	Finite Zero-Sum Two-Person Games	5
		Infinite Zero-Sum Two-Person Games	5
		Games in Extensive Form	6
		General n-Person Games	5
III	1958	Moves as Plays of Other Games	5
		Games with Imperfect Information	6
		Games with Partial Information	4
		Games with a Continuum of Strategies	5
		Games with a Continuum of Moves	3

Source: Kuhn and Tucker (1950, 1953) and Dresher, Tucker and Wolfe (1958a).

how should others try to solve it. Reading the *Theory of Games* from cover to cover, one feels von Neumann wanted future game theorists to tackle games for $n = 4$, then $n = 5$, and so on, as if game theory for up to three players was complete. Mathematicians who worked over von Neumann's foundations would not follow such lead right away.

The *Contributions to the Theory of Games* series exemplify what theoretical topics in game theory mathematicians pursued. Each of its volumes presented a thematic division, except for Volume IV. Table 4 documents how many chapters (which were self-contained papers) in each subject one finds in those books. How proportions varied suggests a general context of what was going on in game theory. Judging by Table 4, around 1950–1953 two-person games received more attention than n-person games. While subjects for Volume III of 1958 might look unclear, a closer inspection shows two-person theory remained championing. Only Volume IV, of 1959, would finally devote itself to "the part of his theory which probably most interested the late John von Neumann," Tucker and Luce (1959b, vii) contended in introducing that volume. This preeminence of two-person games for almost 15 years since the *Theory of Games* appeared was natural considering Wald's (1945, 1950) application of games in statistical inference, which demanded infinite strategy spaces, and military applications, in which problems are usually of one against another, without other players willing to collude with either side.[8] This characteristic of game theory—how better well-developed two-person theory was and what directions new research privileged—echoed through textbooks.

[8] Wald (1950, p. v) explained in opening his book: "The second chapter deals with a generalization of von Neumann's theory of zero sum two-person games, which is then used in Chapter 3 for the development of the theory of statistical decision functions."

2.1.1 The Starting Place: The RAND Corporation

Before delving into game theory textbooks, it is worth briefly refreshing the early history of Project RAND because some of its staff members became important textbook authors. Project RAND outgrew from Douglas Aircraft Company of Santa Monica, California, in 1945, becoming an independent, nonprofit organization in 1948. RAND, which sounds as a contraction of "research and development," sought answers to military planning and decision problems in science during the Cold War years. The organization had a branch of social sciences, mostly concerned over "individual behaviour and social integrity," Leonard (2010, p. 297) pointed. This division had an "applied" goal: as Leonard (2010, pp. 293-343) analyzed, RAND initially sponsored research on game theory hoping it would fruit into military applications—an expectation games failed to fulfill.[9] However, mathematicians also sought developing game theory in itself, in a "pure," non-applied way. This dual concern over developing von Neumann's mathematical theory of games and finding ways of taking it to real problems appears in RAND reports. On September 3, 1948, RAND issued a report summarizing zero-sum two-person game theory, drawing from unpublished texts of its staff members. This early internal document shows RAND members sustained a concern of helping others to catch up with von Neumann and Morgenstern's ([1944] 2007) theory of games as a necessary step for them to develop it further. After all, game theory was a brand-new subject: not only no RAND member would have learned about it during their student days, but no comprehensive introduction to game theory existed besides the *Theory of Games* itself.

> The contents of this report are drawn from published works and Project RAND internal memoranda. The report presents a summary of zero-sum two-person games with a finite number of strategies as developed by von Neumann. [. . .]
>
> <u>Although several examples and solutions of games have been included, they were selected in order to demonstrate the techniques involved rather than to solve practical problems related to national security</u>. It is hoped that the theory of mathematical games of strategy is now sufficiently developed to justify an attempt at application to major problems of national security. Efforts in this direction are now being undertaken. (BOHNENBLUST *et al.*, 1948, p. 2; underlining added)

This quote suggests an urgent need for developing game theory was transmitting its "techniques." A central character in spreading such tools was John D. Williams, astronomer and mathematician who got involved with Douglas Aircraft in 1946, becoming

[9] In Leonard's (2010, pp. 293-343) work, one finds a historical account of how game theory became present in "an array of social scientific practices," including, for instance, social experiments. The focus here is not how game theory crossed ways with other practices; instead, it regards how those who developed game theory—mostly mathematicians—transmitted their knowledge to one another.

head of the Mathematics Department of RAND Corporation in 1948.[10] Researchers at RAND believed game theory should spread beyond Santa Monica, possibly through a new book. The idea of writing his *Compleat Strategyst*, Williams (1966, p. ix) stated, came "during a discussion among a group of persons [at RAND] who have been concerned for some years with the dual problems of the development and application of the Theory of Games of Strategy."[11] Until then, only a restricted group of developers of game theory were knowledgeble about it. RAND staff believed Williams should write it because he was a "complete ignoramus" of highly technical papers on games and would "probably not learn enough of these while writing the book to contaminate seriously the message that should be transmitted," Williams (1966, p. ix) documented. Above all, that book should be accessible:

> The sense of the discussion was that the activity, Game Theory, would benefit from having more persons informed regarding its nature; and that the knowledge would benefit the persons, of course. At the present time, this knowledge is mostly held by the tight professional group which has been developing the subject. Another, and larger, group has heard of it and comprehends, often dimly, its scope and character; the members of this group must usually accept, or reject, the ideas on the basis of insufficient knowledge. So it was felt to be worth while to try to bridge the gap between the priestly mathematical activity of the professional scientist and the necessarily blind reaction of the intelligent layman who happens not to have acquired a mathematical vocabulary. (WILLIAMS, 1966, p. ix)

The *Compleat Strategyst* was born as a home-study book, fulfilling that mission of communicating game theory to laymen. Reading it, Williams (1966, p. x) claimed, students would learn to formulate and solve everyday problems by applying game theory, provided they met a minimal level of mathematical knowledge amounting to simple, daily arithmetic computations. Originally, it had five chapters: "Introduction," "Two-Strategy Games," "Three-Strategy Games," "Four-Strategy Games and Larger Ones," and "Miscellany." In 1966, Williams (1966, p. vii) appended a sixth chapter, "General Method of Solving Games," drawing on research from Tucker (1960), who "developed a combinatorial linear algebra of great generality and power" for solving games. This chapter included an eight-step procedure students could follow to solve a two-person game based on advances of the simplex method. What matters here is what chapter titles hint: *The Compleat Strategyst* concerned only elementary games, avoiding abstract theory; it discussed two-person games alone, for strategy spaces of finite (and small) dimension. Book reviewers saw advantages and disadvantages in Williams's

[10] Williams studied at the University of Arizona, the University of Pennsylvania, and Princeton University. Leonard (2010, p. 285) provides a short biographical account of Williams's career.

[11] The title *Compleat Strategyst* intrigued one of its reviewers. Barber (1954, p. 452) reached his "trusty *Oxford English Dictionary*, 1933" finding out "compleat" was only a different spelling of "complete" and "gyst" is a form of "joist," assuming Williams had "merely combined 'Strate' with 'gyst.'"

(1966) simplified approach. As Hammersley (1955, p. 251) remarked, his treatment had a scope restriction implied by its target audience of laymen; in a sense, it comes across as a book about "the general philosophy of game theory." Regardless, Kieffer (1954, p. 153) felt "No one could possibly read *The Compleat Strategyst* without learning considerable about the *Theory of Games* [of von Neumann and Morgenstern]."

The distinguishing feature of *The Compleat Strategyst* was its accessibility. While it made it possible for laymen to grasp what game theory was about, it would make it an ineffective tool for practitioners of game theory—*The Compleat Strategyst* avoided too many important topics. Two other RAND members worked on books for training new generations of game theorists: McKinsey, who participated in RAND Corporation since its Douglas Aircraft days, joining it in 1947 and leaving it by 1951;[12] and Karlin, who was also an early member, whose engagement dates from 1949–1952 and 1954.[13] To discuss game theory in a way that readers would become apt for *doing* game theory, McKinsey and Karlin need to rely on reading requisites. McKinsey (1952, p. v) explained he devised his text for "upper division and graduate students," so he could apply without further instruction a list of mathematical concepts (namely, "convergence, continuity, derivatives, (Riemann) integrals, greatest lower and least upper bounds, and maxima and minima"). Karlin (1959a) did not warn his readers on what tools they should know before picking up his text. A quick glance over his text, however, shows Karlin (1959a, pp. 15-23) promptly employed concepts from general topology and optimization, besides applying suprema and infima. That is, his exposition relied on his reader's knowledge just as McKinsey's (1952) did.[14] This reliance on what their readers already knew permits a direct comparison with von Neumann and Morgenstern's ([1944] 2007) original and its known history of dissemination.

The *Theory of Games* was knowingly a complex book: having over 600 pages, historians documented how it relied on secondary sources for its dissemination. For instance, its book reviews were critical in explaining for economists what game theory was and how some parts of it functioned.[15] However, the *Theory of Games* was also knowingly self-contained: while von Neumann applied modern mathematics in building

[12] McKinsey was fired from RAND in 1951 for his homosexuality and, at RAND, "homosexuals were thought to be high-risk employees, the rationale being that they were more likely to divulge secrets under the threat of having their private lives revealed" (FEFERMAN; FEFERMAN, 2004, p. 161). After RAND, McKinsey became part of the Stanford University's faculty in the Philosophy Department, where he worked until his suicide in 1953. There, he dropped his agenda on games, working on axiomatizing classical mechanics of physics (alongside Patrick C. Suppes).

[13] The years which Karlin spent at RAND were deduced from his research output there. Among Karlin's advisees, one finds John W. Pratt, whose name economists readily recognize from the Arrow-Pratt measure of risk aversion.

[14] Comparing both titles, possibly some readers could find McKinsey (1952) less demanding since Karlin (1959a, p. 33) recommended McKinsey (1952) as an introductory mathematical exposition in game theory (others were Gale's (1957) and Vajda's (1956c) texts).

[15] I pursued such a point in my Master's Dissertation.

game theory, he constructed every needed tool from scratch. For example, von Neumann and Morgenstern ([1944] 2007, pp. 60-66) provided a short introduction to set theory because it was necessary to mathematically define the games studied. Looking at textbooks, such as McKinsey's (1952) and Karlin's (1959a, 1959b), things are different. In a way, they simplified the *Theory of Games*: they provided shorter accounts, often leaving out some parts of game theory that would be overly complex for newcomers. However, they were not building any tools from scratch. While the *Theory of Games* at least provided a fair shot for non-mathematicians, such readers would need some mathematical training before picking up McKinsey's (1952) and Karlin's (1959a, 1959b) texts. The game theory textbook market, which was born at RAND, was providing too simplistic and too complex alternatives.

The background at RAND explains how textbooks ended up being "extreme," either addressing laymen or mathematicians. Being RAND members, McKinsey and Karlin were subject to that context Williams (1966, p. ix) described: RAND members thought game theory needed a wider dissemination, which could happen through new books. This need was not just a caprice. Leonard (2010, p. 298) documented how "At the early stages, [at RAND] game-theoretic models were thought likely to be useful in solving tactical military problems to be encountered in a war with the USSR." Enthusiasts of game theory, such as von Neumann himself, participated in stimulating research in game theory at RAND. But, regardless of his efforts, quickly RAND members realized it would not provide direct application. Nonetheless, it became crucial in psychological experiments others conduced at RAND. Here it is possible to make some sense of Williams's (1966, p. ix) discussion about producing and easy-to-read textbook: it could inform other scientists working on experiments but who were not mathematicians (and who could not read the *Theory of Games* or technical papers). The knowledge they needed was of a specific sort: to apply game theory in experiments, it is not necessary to discuss its mathematical content; it sufficed get its gist, being familiar with what would be expected from strategic situations in light of what mathematicians discussed. It comes as no surprise that many citations which Williams (1966) received came from interdisciplinary journals (such as *Science*), or journals related to behavioral sciences and operations research.[16]

The *Compleat Strategyst* served, beyond laymen, scientists who just wanted to use game theory, not develop it. Thus, whoever desired to do game theorist's work would have to inform himself elsewhere. Here is where McKinsey's (1952) and Karlin's (1959a, 1959b) fit. Around 1950–1959, game theory was still a new subfield *of mathematics*. A search for papers in game theory of 1950–1959 will show up nearly just mathematics papers. Yet, being a *new* subfield is different from being simply a subfield. Save for

[16] This point relies on tracking citations to Williams's (1966) work until 1960 using Google Scholar.

few exceptions (as Princeton University and the University of Michigan), courses on game theory were far from widespread in universities around the 1950s.[17] Incoming mathematicians to RAND would be newcomers to game theory: they would have to learn a new craft to participate in its research agenda. McKinsey and Karlin went through that: they came from other areas of mathematics, learning game theory at RAND.[18] Being trained with the *Theory of Games* itself entailed two problems. First, not everything on von Neumann and Morgenstern's ([1944] 2007) was important for RAND's goals; in fact, only a small slice of its 625 pages was, which mostly refers to von Neumann's (1928) early work on his minimax. Second, research in game theory was active, so the *Theory of Games* would be an incomplete reading as it did not contain new results about two-person games dating from 1944 onward. Just as Williams's (1966) acted to bring in non-mathematicians, McKinsey's (1952) and Karlin's (1959a, 1959b) functioned as a door for mathematicians.

While who did game theory were mathematicians, RAND did not foster game theory for its own sake; it envisioned applications. This purpose is manifest in textbooks of McKinsey (1952) and Karlin (1959a, 1959b). For starters, both emphasized two-person zero-sum games. As Karlin (1959a, p. 6) argued, "the techniques and concepts of the subject [of n-person games] are relatively undeveloped." That is, by 1959, n-person games were still not sufficiently coherent for a textbook-like presentation—and creating an organization is mandatory in making a textbook. Remember, the *Theory of Games* did not fully characterize solutions for games of $n \geq 4$, as doing so involved convoluted calculations. In opposition, two-person games yielded themselves more easily to applications—no wonder why Wald (1945) could employ such games in studying statistical inference as soon as the *Theory of Games* was out. This ease of use explains how come two-person games were in vogue during 1952–1959, even if the *Theory of Games* prescribed research on n-person problems for its readers. While both

[17] Shubik (1992, p. 161) identified Princeton University, the RAND Corporation, and the University of Michigan as places in which game theory was flourishing. There is no evidence that other universities were also "strongholds" of game theory around that time.

[18] Both McKinsey and Karlin came from different subareas of mathematics and started working in games once they became involved with RAND. McKinsey obtained hid Ph.D. in the University of California, Berkeley, in 1936, with a dissertation titled as "On Boolean Functions of Many Variables," written under Benjamin A. Bernstein. Initially, McKinsey's interest remained in mathematical logic: after earning his doctoral degree, he spent short stays at the New York University and the Montana State College, when he published papers in such a field (most of his research appeared in the *Journal of Symbolic Logic*, in whose editing he participated since 1941). After joining RAND in 1947, a year later he issued a RAND Memorandum named "Ville's Example of a Game Without a Strategic Saddle-Point:" it delimits his entry into game theory (in coauthorship with Melvin Dresher). In turn, Karlin got his Ph.D. in mathematics from Princeton University in 1947 under Salomon Bochner, with a dissertation titled "Independent Functions," of which he obtained his first published paper ("Orthogonal Properties of Independent Functions," dated of 1949). In 1949, Karlin published a RAND Paper (coauthored by L. S. Shapley) named "Some Applications of a Theorem of Convex Function"—a work they developed together with Henri F. Bohnenblust, and which versed on Helly's Theorem, which has applications in game theory. Still in 1949, he distributed his first RAND manuscript directly related to games, "Solutions of Discrete, Two-Person Games" (alongside Shapley and Bohnenblust).

McKinsey (1952) and Karlin (1959a, 1959b) favored two-person games, they envisioned different applications of it.

Comparatively, McKinsey (1952) favored a wider and more mathematical survey of important tools and mathematical applications (in statistics and linear programming). To illustrate this, after his discussions on two-person games, some of his chapter titles were: "Games with Infinitely Many Strategies," "Distribution Functions, "Stieltjes Integrals," "Applications to Statistical Inference," and "Linear Programming."[19] These were concise chapters, no longer than 20 pages. What matters is that they connected game theory with other mathematical areas; there was no application fulfilling RAND's goals. In a way, Karlin (1959a) was not applying game theory to military problems, as it could be expected. While Karlin (1959a) also included a special chapter on linear programming, just as McKinsey (1952) did, what distinguishes his book are his two last chapters equally named "Mathematical Methods in the Study of Economic Models."[20] As their names suggest, he attempted to bridge game theory and economics.

> In the past two decades, impressive progress has been made in the mathematical analysis of economic models. It has been necessary to develop new methods to deal with such models, since they typically involve nondifferentiable functions and variables subject to inequality constraints, which cannot be handled by the classical calculus. Broadly speaking, the methods needed are those used in game theory—i.e., the theory of convex sets, topological fixed-point theorems, the theory of positive matrices, etc. In this chapter [Mathematical Methods in the Study of Economic Models] and the next [Mathematical Methods in the Study of Economic Models (Continued)], we explore the use of these techniques in the context of mathematical economics.
>
> In this chapter we shall examine elements of <u>production theory, consumption theory, and equilibrium theory</u>. In Chapter 9 we shall consider <u>welfare economics, models of the dynamic theory of balanced growth, and certain problems of stability theory associated with equilibrium prices</u>. [. . .] (KARLIN, 1959a, p. 243; underlining added)

This quote could suggest Karlin (1959a, 1959b) successfully blended game theory and economics. This would be an achievement: Karlin (1959a, p. v) envisioned his text as an "attempt at a preliminary synthesis of the concepts of game theory and programming theory, together with the concepts and techniques of mathematical economics, into a single systematic theory." However, his synthesis was much more about linear programming than game theory.[21] Karlin's (1959a) attempt produced mixed feelings among book reviewers. For example, while Contini (1963, p. 165) felt that he

[19] As Beckmann (1953, p. 619) explained, McKinsey (1952) gave infinite games a substantial portion of his book. This subtopic within game theory depended heavily on distribution functions and the Riemann-Stieltjes integral, justifying some of his chapter coverage choices.

[20] More precisely, Karlin named these last chapters as "Mathematical Methods in the Study of Economic Models" and "Mathematical Methods in the Study of Economic Models (Continued)."

[21] A similar thread run in Gale's (1960) book, and similarly to Karlin (1959a), he used more linear programming than game theory itself in his bridging effort.

succeeded in meshing economics and game theory (in particular, for applying linear programming in "theory of choice and resource-allocation"), Morgenstern (1961a, p. 407) regretted Karlin's (1959a, 1959b) emphasis on two-person zero-sum games and his concurrent avoidance of n-person games. Morgenstern's (1961a, p. 407) negative reaction to Karlin's (1959a) treatment of general equilibrium was more incisive.

Morgenstern (1961a, p. 407) contended "Karlin discusses primarily methods, but appear to consider the standard equilibrium theories also to be faithful representations of reality." Remember, von Neumann and Morgenstern ([1944] 2007, p. 13-15) thought "the classical conditions of 'free competition'" was "the starting point of much of what is best in economic theory," but economics got it wrong: instead of assuming "free competition," it should obtain it (as a result) from a study of economics of few agents. Applying game theory to economics, as the *Theory of Games* heralded, was more than a simple application of a new tool to old problems; it also meant a reversal of how economists usually approach economic problems. The "true economic problem," Morgenstern (1961a, p. 407) concluded, "is better described as an n-person game." In light of Morgenstern's (1961a) criticism, Karlin's (1959a) chapters on economics should not be read as evidence of a successful incorporation of game theory by economics, especially because of his focus on programming instead of game theory.

A full use of game theory in economics would only thrive years later. It is hard to put a date on it, but economists would start applying game theory in models of few agents around 1968, and such research would gain momentum within ten years (Part II covers such a phenomenon). However, textbooks of 1950–1959 should not be downplayed. The context of RAND's goals of seeking a popular expression of game theory, whereas it also needed to transform mathematicians into game theorists, shaped the first game theory textbooks. Textbooks from a period frequently recover elements of their predecessors, as it would follow from Kuhn's (1963) reasoning that textbooks of a subject share their "substance or conceptual structure," and it would be no different for game theory. However, if one searches for "the" textbook published between 1950–1959, he has to look elsewhere. Far away from RAND Corporation, whithin a similar context of applying game theory (to social and psychological studies), another textbook—perhaps the most iconic of 1950–1959—emerged. This book was *Games and Decisions*, of R. Duncan Luce and Howard Raiffa.

2.1.2 An Alternative Place: The Behavioral Models Project

While in Santa Monica opposing books on game theory appeared—Williams (1966), on one side, and McKinsey (1952) and Karlin (1959a, 1959b) on another—a middle-ground text would appear in another place: Columbia University, in New York, NY. The book is a famous one, known even by modern practitioners: *Games and*

Decisions: Introduction and Critical Survey, by Luce and Raiffa (1957).[22] In its opening pages, one reads: "A study of the Behavioral Models Project, Bureau of Applied Social Research." The Bureau of Applied Social Research originated from another project, named Office of Radio Research at Princeton University (1937–1940), when it moved to Columbia. Sociologist Paul F. Lazarsfeld headed the Bureau.[23] O'Rand (1992, p. 192) described the Bureau's purpose: "The research program of this operation centered on the analysis of attitudes and behaviors of populations, though no coherent theoretical agenda developed successfully." Nonetheless, they were prone to empirical research. The Bureau first drafted Raiffa "to participate in a program called the Behavioral Models Project which was established to introduce sociologists to such new approaches as Markov chains and game theory," O'Rand (1992, p. 194) reported. In 1953, Luce joined it. *Games and Decisions* was born inside the Bureau.[24] As recounted by Luce and Raiffa (1957, p. x), "A number of studies have been completed, most of which have been distributed as technical reports to a limited audience." The first to receive a "wider distribution," Luce and Raiffa (1957, p. x), was *Games and Decisions*.[25]

Three years before *Games and Decisions* was out, Luce and Raiffa coauthored *A Survey of the Theory of Games*, Technical Report No. 5 of the Behavioral Models Project. It was a draft of *Games and Decisions*. Luce and Raiffa (1954, unnumbered page) explained they wrote for expository purposes, considering two audiences: "social scientists with some, but limited mathematical training [. . .] who have neither the interest nor the mathematical sophistication to follow detailed formal proofs" and "mathematicians interested in the mathematical applications in the social sciences." Later, Luce and Raiffa (1957, p. vii) spoke simply of communicating "the central ideas and results of game theory and related decision-making models unencumbered by their technical mathematical details." Regardless, *Games and Decisions* served both social scientists and mathematicians accordingly, as its preliminary version of 1954 suggested, because of two central characteristics of it. First, Luce and Raiffa (1957) required a limited mathematical

[22] The Massachusetts Institute of Technology awarded Luce his Ph.D. in mathematics in 1950. Until 1953, Luce acted as co-director of the Group Networks Laboratory at MIT. In turn, Raiffa received his Ph.D. in mathematics from the University of Michigan in 1951. Arthur C. Copeland, one of the *Theory of Games'* book reviewers, was his advisor. Raiffa (1992, p. 166) affirmed that he became interested in game theory before he "knew about the minimax theory of von Neumann," while he worked on a part-time position of research assistant; more precisely, in conferences "in which applied mathematical problems were discussed by our clients in the Department of Defense and to formulate meaningful, tractable, mathematical problems." This happened in 1948; also in that year, Copeland started a seminar series in which Raiffa and others would work through the *Theory of Games*, studying von Neumann and Morgenstern's ([1944] 2007) new theory.

[23] Lazarsfeld obtained his Ph.D. in applied mathematics at the University of Vienna, in 1925. He soon turned his interests to psychology, moving to the United States under a Rockefeller Foundation grant. Lazarsfeld stood at Columbia until his retirement, in 1970.

[24] *Games and Decisions* was a landmark for decision theory but, as previously stated, such a discussion is far from reach here.

[25] There is no comprehensive account about the history behind the Behavioral Models Project besides O'Rand's (1992) work.

training of their readers, differently than McKinsey (1952) and Karlin (1959a, 1959b), who wrote for trained mathematicians:

> Still one may ask: what exactly are the prerequisites? It is not easy to say. Certainly neither the calculus nor matrix algebra as such are required, but neither will hinder, for probably the most important pre-requisite is that ill-defined quality: mathematical sophistication. (LUCE; RAIFFA, 1957, p. viii)

As often happens in mathematical textbooks, statements on prerequisites might be misleading—von Neumann and Morgenstern ([1944] 2007, Technical Note) contended their mathematics was "elementary," for instance, contrarying what readers thought. Nerlove (1958, p. 550), who reviews *Games and Decisions*, pointed that "Luce and Raiffa claim that their book requires little or no mathematical background (the publisher claims somewhat more on the dust jacket). As is typical of books that make this claim, it is not quite justified." A specific example backed Nerlove's (1958, p. 550) argument: in discussing n-person games, Luce and Raiffa (1957) used "set theory and the notion of a set function [. . .] without adequate explanation." To prevent any issues, Simon (1958, p. 342) suggested preliminary readings for mathematically naive readers: first, they should read "John McDonald's popular but generally accurate little book, *Strategy in Poker, Business, and War*; next, John Williams' equally entertaining, but slightly more technical, *The Compleat Strategyst*." After such preparation, Simon (1958, p. 342) continued, "the student is ready for Luce and Raiffa."[26] He also provided a reading list for further study, suggesting that after finishing Luce and Raiffa's (1957) book, if some reader felt unsatiated, he should read McKinsey's (1952) and von Neumann and Morgenstern's ([1944] 2007) books. Notwithstanding, Nerlove (1958, p. 501) sustained that while Luce and Raiffa (1957) attempted to make an easy exposition, they imprinted a major deficiency in *Games and Decisions*: it became a misleading text. Its readers would think they learned game theory without having to put any hard work on it. The new theory of games was a mathematical theory, and communicating it should involve conveying its rigor as well:

> If the theory of games is to be valuable primarily because it is the first example of an elaborate mathematical development centered in the social sciences, it must be presented in such a way that the principles

[26] Similarly to Simon (1958), Peston (1960, p. 185) could not recommend *Games and Decisions* as a first reading in game theory: "I would not recommend this book [Luce and Raiffa (1957)] as a first introduction to game theory. Some combination of Williams, Mckinsey, Kuhn's lectures, and selected parts of chapters I, II, III and IV of von Neumann and Morgenstern would be preferable." For reference, chapters I–IV from the *Theory of Games* included von Neumann and Morgenstern's critique to economics, two-person zero-sum theory, and a chapter of examples. Kuhn's lectures notes refereed to a course he taught in the mathematics department at Princeton University in 1952 (for upper level students). Kuhn intended to publish them, but he did not back then. However, in a modern print, Kuhn (2003, p. vii) explained "I withdrew the manuscript for alterations, primarily hoping to add something on the rapidly developing theory of n-person games. The revisions were never made, and the lectures were never published."

> of proof, the art of constructing long chains of logical reasoning, and the technique of abstraction are brought home to the reader. It cannot be said that Luce and Raiffa accomplish this; in their favor it might be added that they did not intend to. (NERLOVE, 1958, p. 551; underlining added)

Even if *Games and Decisions* lacked what could be called "the language" of game theory, there is a second reason why it would be an important reading for social scientists *and* mathematicians. The organization of *Games and Decisions* resembled that of the *Theory of Games*: Luce and Raiffa (1957) started from expected utility theory, proceeding to two-person zero-sum games, and subsequently relaxing hypotheses of being "zero-sum" and of being "two-person," just as the *Theory of Games* did. However, Luce and Raiffa (1957, vii-viii) argued that while they mimicked von Neumann and Morgenstern ([1944] 2007) in "overall outline," details were different as they attempted to incorporate contributions from "the decade since the second edition" (of 1947) and emphasized concepts of game theory instead of searching solutions for specific games.[27] By distancing themselves from the *Theory of Games*, Luce and Raiffa (1957) became closer to research posterior to von Neumann and Morgenstern's ([1944] 2007) book. This move made up *Games and Decisions*' trademark. The most distinguishing feature of it was its "critical tone."

> Our aim [with the critical tone] is to warn and to challenge the reader at just those points where the theory is conceptually weak. [. . .] If we have not failed completely, then there should be something of interest here for a wide group of scholars: economists concerned with economic theory, political scientists and sociologists having a methodological bent or a theoretical concern with conflict of interest, experimental psychologists studying decision making, management scientists interested in theories of "rational" choice and organization, philosophers intrigued with the axiomatization of portions of human behavior, statisticians and other professionally practicing decision makers, and finally mathematicians—those whose work, for the most part, we are reporting. (LUCE; RAIFFA, 1957, p. viii)

This "critical tone," allied with Luce and Raiffa's (1957) eased mathematics, put *Games and Decisions* apart from other books. Textbooks originated at RAND showed a concern over dissemination, but they had only a limited potential in connecting their readers and research in game theory. Readers of McKinsey's (1952) introduction would get a feeling of game theory, but would not acquire sufficient expertise to use it. In turn, who studied McKinsey's (1952) or Karlin's (1959a, 1959b) volumes would surely become a proficient game theorist—but only if he was already a mathematician. The

[27] Titles from Chapters 2–7 illustrate how Luce and Raiffa (1957) followed the *Theory of Games* regarding order: "Utility Theory," "Extensive and Normal Forms," "Two-Person Zero-Sum Games," "Two-Person Non-Zero-Sum Non-Cooperative Games," "Two-Person Cooperative Games," and "Theories of n-Person Games in Normal Form."

audience of Luce and Raiffa's (1957) would understand what researchers achieved after von Neumann and Morgenstern ([1944] 2007) published their work, what questions remained open, and even some suggested paths for addressing them, without needing a major in mathematics beforehand. *Games and Decisions* readily established a considerable influence, which extended itself beyond mathematics. Within only three years of its publication, it received many citations in journals of economics, psychology, law, among others.[28]

2.2 A Classic Becomes Dated

So far, textbooks of 1950–1959 could be seen as adapting the *Theory of Games*' presentation for different audiences by adjusting its mathematical rigor—depending on how much mathematical expertise they had—or slicing its content, only showing what mattered in von Neumann and Morgenstern's ([1944] 2007) original—what could be a focus on two-person (especially zero-sum) games. Textbooks did more: they also updated the *Theory of Games*. Institutions such as the RAND Corporation and the Office of Naval Research (in particular, its Logistic Project) financed research in game theory seeking workable models for war issues; while game theory failed in fulfilling such a need, it accomplished much in a mathematical sense. The 1950s and 1960s were years of huge theoretical accomplishments, as practitioners recognize (for instance, see Aumann's (1987, pp. 467-476) and Kuhn's (1997a, pp. xi-xii) comments). Kuhn (1997b, pp. ix-x), in his collection *Classics of Game Theory*, thought of incorporating "some 'prehistoric' excerpts (say, by Montmort, Zermelo, and von Neumann)" to enrich classic papers, which were "the basic building blocks on which the current edifice of game theory is built." The keyword here is *prehistoric*: to a certain extent, past research had lost its direct usefulness. On one side, von Neumann's (1928) early work and the *Theory of Games* were pivotal in establishing game theory, but on another, newer research was supplanting some of their achievements. Among all eighteen so-called "classic" contributions Kuhn (1997b) selected, half are from 1950–1957, including Nash's (1950b, 1950a, 1951) contributions regarding equilibrium points and bargaining, and Shapley's (1953c) value (which became an alternative solution concept of cooperative game theory, finding applications early on in political science).[29] Game theory was growing beyond what the *Theory of Games* accomplished, out of von Neumann's hands; in keeping abreast with it, reading the *Theory of Games* was simply not enough.

Some developments were not much critical, but others substantially and permanently changed game theory—including even what a *game* is. The *Theory of Games*' second chapter was about defining games usefully. As von Neumann and Morgenstern

[28] This point relies on a search using Clarivate's *Web of Knowledge* database.
[29] Other 8 papers were from 1960–1969, and just one was from 1975.

([1944] 2007, p. 48) explained, they would obtain “a rather complicated but exhaustive and mathematically precise scheme,” only to “replace this scheme by a vast simpler one, which is nevertheless fully and rigorously equivalent to it.” What permitted moving from a “complicated” to a “simpler” definition of “strategy,” understood as a complete plan of action which would not pose any hindrances on player’s actions.[30] Instead of making decisions as they become necessary, players would reason about every possible contingency he or she might face; while strategies impose an intellectual burden on players, von Neumann and Morgenstern ([1944] 2007, p. 79) argued, it boiled down to “an innocuous assumption within the confines of a mathematical analysis.” A few pages later, von Neumann and Morgenstern ([1944] 2007, p. 85) baptized each of those two approaches, all-inclusive and simplified, as “the extensive and the normalized form of the game,” respectively.[31] However, von Neumann and Morgenstern’s ([1944] 2007) definition of extensive form games was not definitive, especially because it was not amenable to discussions of what information each player has. The first volume of *Contributions to the Theory of Games* contained a list of 14 urgent problems in game theory, among which one reads:

> Covering the rapidly expanding frontier of the theory of games as they do, the contributions of this study provide signposts to present and future trends in research. Some of the outstanding problems which are indicated admit an explicit formulation while many other lie in zones of the theory which need further clarification and restatement above all. Until now, the base of the theory has been the theory of the finite zero-sum two-person game which has received the most intensive development and whose results will influence the direction of research in the other branches. [. . .]
>
> [. . .]
>
> It is in the n-person theory that we find the zone of twilight and problems which await clear delineation. Often these problems have their roots in an undeveloped part of the two-person theory. A prime example is the extensive form of a game.
>
> (12) To develop a comprehensive theory of games in extensive form with which to analyze the role of information—i.e., the effect of changes in the pattern of information. At present, equivalence of information patters can only be defined for games with a value, thus excluding most n-person games. (KUHN; TUCKER, 1951, pp. x, xii; underlining added)

That is, von Neumann and Morgenstern’s ([1944] 2007, pp. 73-74) formulation of extensive form games yielded some issues: it was simply too complicated. Kuhn

[30] Another simplifying device von Neumann and Morgenstern ([1944] 2007, p. 83) resorted to was that of “mathematical expectation” to “get rid even of the chance move.”

[31] Each form had different uses, von Neumann and Morgenstern ([1944] 2007, p. 85) suggested: “Actually the normalized form is better suited for the derivation of general theorems, while the extensive form is preferable for the analysis of special cases; i.e., the former can be used advantageously to establish properties which are common to all games, while the latter brings out characteristic differences of games and the decisive structural features which determine these differences.”

(1953) himself would tackle such a problem. While von Neumann and Morgenstern ([1944] 2007, pp. 67-71, 81-84) used extensive form games as an intermediate step in constructing strategic form games (which were easier to analyze), Kuhn (1953, p. 193) thought: "Since all games are found in extensive form, while it is practical to normalize but a few, it seems desirable to attack the completion of a general theory of games in extensive form." Extensive form games should play a bigger role than that of transitional phase between a realistic description and a game theory model. As von Neumann and Morgenstern ([1944] 2007, pp. 77-78) did, Kuhn (1953, p. 194) defined *extensive form game* to be a "game tree" appended with a list of "specifications." These included: a partition indicating when each player acted; an information partition, delimiting what past moves each player know at each game node; a probability distribution for nature-played moves; and, as usual, a payoff function. Contrarily to the *Theory of Games*, he was not committed to a perfect information assumption. This was an important generalization; to draw a connection between both approaches, Kuhn (1953, p. 199) even stated a "Theorem of Categorization" specifying when "von Neumann games" would be equivalent to those he defined. Importantly here, Kuhn (1953) worked on a frontier problem and, despite his accomplishment of getting rid of perfect information, to a large extent n-person games remained unyielding to analysis. Regardless, textbooks incorporated Kuhn's (1953) definition—it replaced von Neumann and Morgenstern's ([1944] 2007) original.[32]

The influence of Kuhn's (1953) was quick to come: in Volume II of *Contributions to the Theory of Games*, there is a section of papers about "Games in Extensive Form." Of its five papers, four used Kuhn's (1953) definition (and another proceeded without reference to either von Neumann and Morgenstern ([1944] 2007) or Kuhn (1953)). When textbooks presented extensive form games, they opted for Kuhn's (1953) updated definition.[33] In particular, Luce and Raiffa (1957, p. 48) and McKinsey (1952, p. 106) did so. They were not verbal about why they did so, however, only saying they did it. Luce and Raiffa (1957, 48) pointed "The original description of a game in extensive form [. . .] differs somewhat from and is less compact than this one, which we have paraphrased from the formal definition given by Kuhn." In turn, McKinsey (1952, p. 106) stated: "A detailed account of games in extensive form is given in von Neumann and Morgenstern [1]. We

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32 Modern textbooks cite Kuhn (1953) in acknowledgement of its relevance for formulating extensive form games. For example, see Fudenberg and Tirole (1991, p. 77), Myerson (1991, p. 37), and Osborne and Rubinstein (2012, p. 144). Kuhn (1953, p. 210) is also important insofar he defined behavioral strategies as they are known today, defended as "another natural method of randomization." For a recent citation in that regard, see Fudenberg and Tirole (1991, p. 89).

33 Karlin (1959a, 1959b) neglected extensive form games for a specific reason: they were not central to what game theorists discussed by then; most researchers still privileged two-person games in strategic form. In particular, Karlin (1959a, 19) supported his choice of not discussing extensive games saying "a theorem is proved to the effect that any game in extensive form may be in fact reduced to an equivalent game in normal form" and "our definition of a game as a triplet [. . .] is flexible and general enough to encompass all forms of finite game theory, including in particular the structure of games in extensive form."

have based our discussion, however, on the formulation to be found in Kuhn [2]."[34] Regardless of McKinsey's (1952) and Luce and Raiffa's (1957) reasons, what they did has one implication to the role the *Theory of Games* played after game theory textbooks started to appear.

Defining *game* is fundamental step in game theory: it delimits what kinds of problems one might tackle, as well as how can one solve them. The first game theory textbooks brought up a new definition for extensive form games, which replaced von Neumann and Morgenstern's ([1944] 2007), reflecting what happened in research. Kuhn's (1953) case functions as an example here; such updating changes were more broad in content.[35] In such a sense, the *Theory of Games* was becoming dated: to participate in game theory research, one should not look for how it defined extensive form games. Or, if he did, it would be necessary to unlearn what the *Theory of Games* taught. This does not mean von Neumann and Morgenstern's ([1944] 2007) work was no longer influential or worth reading; for many years, game theorists would follow leads von Neumann and Morgenstern's ([1944] 2007) left. For instance, when research in game theory started to concentrate efforts on n-person games, papers would focus on von Neumann and Morgenstern's ([1944] 2007) solution concept, and not any other available alternative. The *Theory of Games* remained widely cited, but increasingly more for historical reasons than for actual use of its content. The research flow was changing game theory down to its roots, and to learn and use game theory, reading *Theory of Games* would not be enough. But textbooks of 1950–1959 were not simply providing easier and updated accounts of game theory. More than that, they had a role in organizing a newborn community.

2.3 Brief Concluding Remarks

The *Theory of Games* was a challenging book, and it comes as no surprise some capable readers would write gentler introductions to game theory. Naturally, some of such textbooks sought to broaden game theory's audience. These efforts would not concentrate on non-mathematicians only, however; even for a trained mathematician, von Neumann's 625-page presentation demanded a significant reading effort. Thus, game theory textbooks of 1950–1959 let non-mathematicians to better understand what game theory was about, without providing them enough tools to *do* game theory; and they also organized game theory in a way to introduce mathematicians to that new area which they never studied before. The making of textbooks involved two central moves.

[34] McKinsey (1952, p. 106) cited a working paper version of Kuhn's (1953) work, dated of 1950.

[35] Another interesting case concerns algorithms to find minimax solutions of games with large strategy spaces. The examples of the *Theory of Games* were not amenable for a general procedure of solution calculation. RAND researchers Brown (1951) and Robinson (1951) were important in proposing a usable algorithm, which was widely available in textbooks of 1950–1959.

First, it was necessary to slice the *Theory of Games*, selecting its most relevant parts, which was mostly two-person games (because of their usefulness in Cold War-related problems). Except by *Games and Decisions*, textbooks were not discussing n-person games, which would be much more interesting for economists than two-person games. Second, textbooks needed to update the content of the *Theory of Games*. While research in game theory was concentrated on few institutions, such as the RAND Corporation, a considerable large amount of research stocked up until 1959. This research not only filled in details which the *Theory of Games* left behind, but also substituted some of its propositions. But perhaps most importantly, textbooks were not simply passive receptors of what was consensual in game theory: they actively sought tying together a loose community.

Textbooks published during 1950–1959 provided a sort of "socialization" of game theorists. They transmitted a message saying what it meant to "do" game theory and, besides, *how* should researchers do it—mostly answering mathematical problems. The logic of game theory mimicked how mathematicians did mathematics: while inspiration for a problem could come from elsewhere—say, from a military problem—, its statement and resolution assumed a theorem-and-proof form. This type of reasoning came up even in textbooks such as *Games and Decisions*, which sought a more popular exposition of game theory. This does not mean that game theory was aloof from other disciplines; around 1950–1959, war issues guided much of its research questions, and in later years, other disciplines—such as economics—would also have their influence. But textbooks show that, however fluid game theory was as a field, it should not be downplayed how deeply mathematicians imprinted their own practices into the field of game theory.

While textbooks showed a conception of what game theory was, it should also be noted they reflected its most pressing questions. In particular, it is remarkable that few textbooks discussed n-person games while in the *Theory of Games* they were what was most important to make game theory usable in economics and other social sciences. The textbooks make it visible that mathematicians did not know how should they do n-person theory. The subject was not in many textbooks, and those which covered it did it differently from one another. They shared a common ground by presenting the *Theory of Games'* way—its definition of n-person (cooperative) game and solution—and providing some criticism of it. This criticism involved switching focus to non-cooperative games, but textbooks were skeptical about such path. In fact, a new "generation" of textbooks would only come when game theory chose a route into n-person game theory.

3 The Rise and Halt of Cooperative Games, 1968–1970

When von Neumann and Morgenstern ([1944] 2007) launched their theory of games, they successfully solved two-person zero-sum games: von Neumann's (1928) Minimax Theorem guaranteed solution existence, and it also had a certain "mathematical elegance," as Luce and Raiffa (1957, pp. 56-57) described it, which lured mathematicians into game theory. This "elegance" mostly refers to connections between game theory and other subfields of mathematics (such as linear programming). The Cold War demands, allied with such a mathematical attractiveness, explains how come mathematicians focused on two-person games for years since the *Theory of Games* was out. Besides, there was a dissatisfaction with how von Neumann and Morgenstern ([1944] 2007) tackled n-person games. In explaining why mathematicians firstly preferred two-person games, Dresher, Tucker and Wolfe (1958b, pp. 1-2) pointed three major complaints against the *Theory of Games*' n-person theory. Two regarded particular technicalities of games, and their third criticism concerned a growing feeling that n-person games lacked "elegance." Yet, with time, mathematicians switched their focus to n-person theory.

That switching was anything but simple. Knowingly, adding a third player introduced a substantial change in how the *Theory of Games* analyzed games: by including a third participant, any two players could join forces against their common rival by forming a *coalition*.[1] This single featured added a thick layer of complexity to game theory, and so n-person theory deviated from 2-person theory in fundamental ways. To mention a few, strategic form games gave space to characteristic functions;[2] strategies disappeared, and solutions became sets of payoffs; and the *Theory of Games* even suggested a "fictitious player" should become instrumental in organizing an attack plan for solving n-person games: by solving any zero-sum n-person game, such resource provided a way to study non-zero-sum $(n-1)$-person games.[3] Even if n-person game theory was much different from 2-person game theory, it should possess an

[1] A coalition is simply a subset of players (that is, a subset of N).

[2] Given a set of players N, a characteristic function is any function $v : 2^N \to \mathbb{R}$ (satisfying $v(\varnothing) = 0$). For any coalition $S \subset N$, $v(S)$ says how much S earns by playing. Formally, a "coalitional game" or "characteristic function form game" is simply a tuple (N, v).

[3] Put simply, a "fictitious player" is a player who earns precisely what is needed to make a game zero-sum. Using fictitious players implied some issues, however. For von Neumann and Morgenstern ([1944] 2007, p. 507), their fictitious player was "no player at all, but only a formal device for a formal purpose." The fictitious player was a player nonetheless, as rational and eager for payoffs as any other and, consequently, could interfere with compensation schemes of coalitions it participated in. This caveat led the *Theory of Games* into a study of what solutions from zero-sum n-person games were legitimate as solutions of non-zero-sum $(n-1)$-person games. Effectively, fictitious players did not become crucial for game theorists—such a concept quickly vanished from textbooks.

essential feature: in their quest for characterizing solutions for all games, von Neumann and Morgenstern ([1944] 2007) were adamant concerning solution existence: it was a mandatory property of their theory.

> There can be, of course, no concessions as regards existence. If it should turn out that our requirements concerning a solution *S* are, in any special case, unfulfillable,—this would certainly necessitate a fundamental change in the theory. This a general proof of the existence of solutions *S* for all particular cases is most desirable. It will appear from our subsequent investigations that this proof has not yet been carried out in full generality but that in all cases considered so far solutions were found.
>
> As regards uniqueness the situation is altogether different. [. . .] (VON NEUMANN; MORGENSTERN, [1944] 2007, p. 42; underlining added)

Differently than solution nonexistence, solution multiplicity was not problematic considering how von Neumann and Morgenstern ([1944] 2007, pp. 42-43) interpreted it: they believed societies could accept differing yet stable social configurations, and each one of them would correspond to a game solution.[4] Critically here, von Neumann found at least one solution for every game he studied. By itself, such a fact does not count as an existence theorem, but it quickly fostered a conjecture. An early evidence is in Wald's (1947, p. 50) book review of the *Theory of Games*, in which he stated: "In all cases investigated so far it was found that there exists at least one solution. There seems to be hardly any doubt that every game has a solution but no general existence theorem has yet been established." His words sounded almost as a call, inviting mathematicians to attempt to prove all n-person games admit a solution in the *Theory of Games'* sense (whose name eventually became *stable set*).[5] Doing so was clearly not an easy task, as even von Neumann was unsuccessful in such a quest. Regardless, even if he couldn't prove it in the *Theory of Games*, his approach of investigation for increasingly large games became a staple in how other mathematicians gave their shots at n-person games.

The *Theory of Games* provided full solutions for games of 2 and 3 players. But it offered more, as it proposed game theoreticians could seek solutions for larger games by slicing them into smaller bits. To analyze 4-person problems, for instance, von Neumann and Morgenstern ([1944] 2007, p. 295) designed a way to graphically represent them using a cube. By studying subsets of its eight corners, they arrived at solutions for

[4] Naturally, solution multiplicity posed as a problem for some. Dresher, Tucker and Wolfe (1958b, 11) felt it was "one of the more telling criticisms of solution theory."

[5] At some point, von Neumann and Morgenstern's ([1944] 2007) solution concept for 3-person and larger games became known as *stable sets*, although the *Theory of Games* referred to it simply as "solution." Nowhere a history of how "stable sets" came to be thus labeled exists. However, it is possible to advance a conjecture: Shapley (1952, p. 2) firstly characterized von Neumann and Morgenstern's ([1944] 2007) solution as "the class of A-stable sets." Just for clarity, in his notation A denotes a set of payoff distributions satisfying individual rationality (in which no player accepts a payoff he could earn by playing alone). Possibly, the suffix "A-" fell off use with time.

particular types of 4-person games. For example, in investigating what they labeled "corner *I*," von Neumann and Morgenstern ([1944] 2007, p. 420) defined a whole class of games, which they called "simple games"—because in such a class any coalition earns either 0 or 1 in payoffs; or, to put it differently, there is a clear sense of winning and losing coalitions, and thus games are "simple." This is how mathematicians would seek solutions, as general n-person games proved to be of exceptional difficult treatment: they would define classes of games, endowed with particular characteristics which would make them solvable.[6] Such a strategy of studying classes of games and solutions attained a substantial popularity.[7] Yet, it let game theorists down a dead-end street.

There was hope that, by working on n-person games bit by bit, eventually a general existence result would emerge. This effort would meet a dead-end because, in reality, that conjecture saying all n-person games have at least one stable set is false. The pivotal counter-example is due to William F. Lucas (1967, 1968), a University of Michigan-trained mathematician.[8] His case in point was intricate: it had ten players, and while he shared his result in a RAND Memorandum and in a communication to the *Bulletin of the American Mathematical Society*, only later Lucas (1969) published a full argument showing his 10-person game admitted no stable set.[9] This result would bring about a reorientation of cooperative game theory, since it showed a major research agenda of game theory could not fulfill its ambitions.

These events concerning what happened with n-person game theory until circa 1967–1969 had their reflections on textbooks. Table 5 catalogs game theory books of 1966–1970. The textbooks in Table 5 differ from their predecessors precisely because they started to cover n-person games. Remember, textbooks from 1950–1959 emphasized two-person games, whereas texts of 1960–1965 highlighted linear programming (a point Appendix B addresses). This does not mean all game theory textbooks from 1966 onward covered n-person games, nor that they did so homogeneously. To mention a singular

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6 For a specific illustration, Bott (1953) studied a class of games parametrized by an integer k (such that $n/2 < k < n$): for any coalition $S \subset N$, its earning $v(S)$ would equal $n - |S|$ if $|S| \geq k$ and $-|S|$ otherwise—in words, a coalition of players would earn a payoff comparable to its size, given its players are numbered enough to meet a cutoff rule. Naturally, he named this class as of "majority games"—for a coalition to win a majority game, it is imperative it gathers enough players. Not only did Bott (1953) focus in a class of n-person games, he also targeted a particular form of solution, that of symmetric solutions (so index permutations of an imputation in a solution is also part of such a solution).

7 As Aumann (1987, p. 13) recollected, game theorists' main research trend consisted of, after exhausting two-person games, "investigating various classes of games and describing their stable sets."

8 Lucas obtained his Ph.D. from the University of Michigan in 1963 under Robert M. Thrall, a mathematician who became deeply involved in the development of operations research. Thrall possibly drove Luce into game theory, as Lucas's early papers (of 1963–1967) are all about cooperative games, including one with collaboration with Thrall (named *n-Person Games in Partition Function Form*, which appeared in the *Naval Research Logistics Quarterly* journal, in 1963).

9 Lucas's (1967, 1968) counter-example was of a game with transferable utility—meaning players could transfer payoffs from one another, making side-payments. Previously, Richard E. Stearns obtained negative result for solution existence in games without transferable utility (in an unpublished paper; see Aumann (1987, p. 29) for a comment).

case, Davis (1970, pp. 134-196) included a chapter on "The n-Person Game" in his textbook, but it mostly concerned 3-person games—which is reasonable considering he sought a nontechnical presentation of game theory, and discussing games for a general n was not straightforward. The greatest curiosity about such generation of textbooks concerns its timing: it started to discuss n-person games in a moment such a class of games received its harshest blow.

Anatol Rapoport's *Two-Person Game Theory: The Essential Ideas* and *N-Person Game Theory: Concepts and Applications*, and Guillermo Owen's *Game Theory* are particularly noteworthy textbooks.[10] Rapoport is not a widely known character of the history of game theory, but he is not a compleat stranger either: Erickson (2015, pp. 166-203) brought up Rapoport as a main character in his account of experiments of the Mental Health Research Institute, at the University of Michigan. In particular, Erickson (2015, p. 182) contended Rapoport's (1966, 1970) textbooks would diffuse "Rapoport's conception of games and game theory—as a tool for investigating the psychology of conflict and cooperation" among "a new generation of social and behavioral scientists." In turn, Guillermo Owen never received as much attention from historians, but Giocoli (2003, p. 297) characterized his book as "widely-used"—indeed, it received four editions (in 1968, 1982, 1995, and 2013) and many translations (to German, Russian, Japanese, Romanian, Polish, and Italian at least).[11] Apart from that, it is also notable how commentators of game theory mentioned his textbook as a suggested reading alongside Luce and Raiffa's (1957) classic.[12] But perhaps more relevantly, Rapoport's (1966, 1970) and Owen's (1968) textbooks presented n-person game theory in a way that would remain more or less unchanged until recent years, at least in what concerns cooperative games.

3.1 An Odd Arrangement

3.1.1 The Projects of A. Rapoport and G. Owen

Rapoport emigrated from Lozovaya, a village in Ukraine, in 1928, at only 17 years of age. After growing up—and showing an interest for being a pianist—, he eventually became a mathematician. The University of Chicago awarded him a Ph.D. in 1941, with a thesis he titled "Construction of Non-Abelian Fields with Prescribed Arithmetic," advised by Otto F. G. Schilling and Abraham A. Albert. While Rapoport took off his career working in abstract algebra, he eventually got into game theory, but before it, his interests wandered through biophysics. After serving in the World War II, in 1947, he

[10] Whereas Rapoport's (1966, 1970) and Owen's (1968) books received fewer reviews than did Kaufmann's (1967), Bartos's (1967), or Borch's (1968) textbooks, these had only *some* chapters dedicated to game theory; their main subjects were other subdisciplines of either mathematics or economics.

[11] This information comes from a simple WorldCat search.

[12] Check Lucas's (1971, p. 4) and Schotter and Schwödiauer's (1980, p. 483) comments, for instance.

Table 5 – List of Game Theory Books, 1966–1970

Author	Title	Year	Rev.	Textb.
A. Rapoport	*Two-Person Game Theory: The Essential Ideas*	1966	4	✓
T. Schelling	*Arms and Influence*	1966	11	
A. Kaufmann	*Graphs, Dynamic Programming and Finite Games*	1967	6	✓
O. J. Bartos	*Simple Models of Group Behavior*	1967	5	✓
G. Owen	*Game Theory*	1968	4	✓
K. H. Borch	*The Economics of Uncertainty*	1968	8	✓
I. R. Buchler and H. G. Nutini	*Game Theory in the Behavioral Sciences*	1969	5	
E. Goffman	*Strategic Interaction*	1969	7	
A. Blaquière, F. Gérard and G. Leitmann (Eds.)	*Quantitative and Qualitative Games*	1969	1	✓
M. D. Davis	*Game Theory: A Nontechnical Introduction*	1970	2	✓
A. Rapoport	*N-Person Game Theory: Concepts and Applications*	1970	4	✓
R. F. Barton	*A Primer on Simulation and Gaming*	1970	2	✓

Source: Each listed book appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. The number of book reviews comes from JSTOR's database and Google Scholar searches.

joined the Committee of Mathematical Biology at his alma matter. In March and June of that year, he published parts of his larger project *Mathematical Theory of Motivation Interactions of Two Individuals*. These "parts" were papers concerning a model in which two agents had to produce some output for their satisfaction, what required effort as input. But, as cooperative game theory would suggest, they could produce more by working together. Rapoport (1947a, 1947b) did not cite von Neumann and Morgenstern ([1944] 2007) or game theory however, and discussed that model using biological terms of symbiosis and parasitism. In 1954, he took a year leave to Princeton's Center for Advanced Studies in the Behavioral Sciences, then, in 1955, joined the University of Michigan, Ann Arbor. Around such time his work would turn to conflict resolution (his first book reviews and notes about games date from 1957). At some point in 1954–1955, Rapoport met R. Duncan Luce, who presented him the Prisoner's Dilemma, which would push him toward game theory, Erickson (2015, p. 175) suggested.

The years Rapoport spent at Ann Arbor shaped his relationship with game theory.[13] Much similarly to RAND, the Mental Health Research Institute was an interdisciplinary organization who brought game theory and other tools together to meet Cold War demands. Erickson (2015, p. 165) described its research by its "emphasizing a combination of theory-building and virtuosic experimental technique," which served a goal of creating "a framework for the unified study of living 'systems' at all levels of organization, from neural networks to societies." These "systems" involved a myriad of possible situations the War could materialize, such as that of finding an effective communication mean in case broadcast media became unusable.[14] The distinguishing feature of such an agenda was its deep reliance in experimentation, which contrasts with RAND's dominantly pen-and-paper mathematical approach (even if some experimentation took place at RAND). Importantly here, if Michigan's and RAND's ways were different, Rapoport's (1966, 1970) and Owen's (1968) textbooks shows so: while Rapoport adhered to Michigander practices, Owen got into game theory in Princeton's tradition, which resembles that of RAND Corporation.[15,16]

[13] Rapoport remained in the University of Michigan until 1970, when he moved to the University of Toronto, where he acted as a Professor of Psychology and Mathematics and of Peace and Conflict Studies.

[14] See Erickson's (2015, p. 177) description of such a problem.

[15] Shubik (1992, p. 161) provided an account of what Princeton University's game theory tradition was like during 1949–1955, documenting that a significant cross-over of people existed between that department and RAND. While game theory at Princeton was arguably less subject to the Cold War demands, what matters is what it meant "to do game theory" in Princeton: it revolved around proving theorems, and not conducting experiments.

[16] As Shubik (1992, 161) recollected, Princeton University and RAND were equally important in fostering game theory, and stated: "Although few of us at Princeton appreciated it, there was considerable activity at Michigan at the time as well." It is unclear what precisely about Michigan's research was difficult to sympathize with for someone at Princeton or RAND. Naturally, one could go down a road of theory *versus* experimentalism. This lead seems fruitless, however; important names of non-experimental game theory, such as H. Raiffa, Robert M. Thrall, and William F. Lucas, came from the University of Michigan.

Owen was one of H. W. Kuhn's advisees at Princeton, receiving his Ph.D. in 1962 with a thesis titled "The Analysis of Large N-Person Games by Means of Decomposition into Smaller Games."[17] This thesis in itself already distinguishes Owen from a previous generation of researchers in game theory: he focused on n-person games since his graduate training, not paying much attention to two-person games.[18] Before finishing his textbook, Owen published papers in game theory and wrote articles for encyclopedias about games (in *New Catholic Encyclopedia* and *American People's Encyclopedia*, in 1966 and 1967, respectively). Since 1961 he assumed a professorship at Fordham University, where he would stay until 1969, when he moved to Rice University. In 1978, he became a professor in the University of the Andes, at his home country, where he worked until 1982. No historical account puts Fordham University as a "center" for game theory: while Owen worked as a professor there when he wrote his textbook, he possibly was not locally connected with other people doing game theory.[19]

Rapoport (1966, 1970) and Owen (1968) came from different backgrounds and made different uses of game theory. Perhaps naturally, their textbooks are also significantly different from one another. A simple skim through prefaces shows it. Owen (1968, p. v) wrote his manual believing "For several years there has been a definite need for a text which covers comprehensively the salient aspects of both two-person and n-person game theory from a mathematical point of view." Until then, only Luce and Raiffa (1957) provided an introduction to n-person games, but they did not design it thinking of mathematicians (not *only* mathematicians, at least). The game theory textbook market had a gap, and Owen (1968) sought to fill it. Rapoport (1966, pp. 6-7) sustained a similar justification for his project: he wrote his textbooks because a "widespread" interest in game theory "in our age of competition, strategy, and gamesmanship" had, in combination with a "lack of sufficient acquaintance with the essential ideas," led to "regrettable misunderstandings and confusion." Here he thought of non-mathematicians, in opposition to Owen (1968). This affirmation possibly reflects his experience at Michigan, in which scientists from different areas applied games in Cold War issues. Still, he was aware of existing texts providing adequate readings of game theory for non-mathematicians. But something in them was missing.

The missing piece was n-person theory. However, putting it into textbook form

[17] Before Owen, Kuhn advised at least four students into game theory (thesis names and years in parentheses): Herbert M. Gurk ("Extreme Games, Simple Games, and Finite Solutions," 1956), James H. Griesmer ("On Extreme Games", 1958), David L. Yarmush ("Preliminaries to a Dynamic Theory of Zero-Sum Two-Person Games", 1959), and Richard E. Stearns ("Three Person Cooperative Games Without Side Payments", 1961).

[18] Although he worked mainly on n-person games, it is noteworthy Owen (1967) obtained a simple, one-page long proof for von Neumann's (1928) Minimax Theorem. Binmore (2004, p. 19) published full a paper paying homage to its simplicity and elegance.

[19] Owen was born in Colombia, and he got his undergraduate study at Fordham University (earning his B.S. in 1958).

was a different task for Rapoport (1966, 1970) and Owen (1968) because that theory was produced mostly by mathematicians. In such a sense, Rapoport's (1966, 1970) project required a simplification effort to a larger extent than Owen's (1968) did. Rapoport (1966, p. 7) believed non-mathematicians had two main issues in consuming game theory texts: "a lack of experience with mathematical ideas, and a lack of experience with mathematical *notation*."[20] While Luce and Raiffa (1957) provided a "vast simplification over the original entirely uninhibited notation of the fundamental treatise [von Neumann and Morgenstern ([1944] 2007)]," it was still heavy on its notation, Rapoport (1966, p. 8) argued. The previous decade had an example of successful adaptation of game theory: Williams's (1966) book. Still, it provided a shortsighted view of games because of its restricted attention to two-person zero-sum games, similarly to its contemporaries. Rapoport (1966, p. 8) thought "one cannot get an idea of a building by examining just the foundation." This is why he aimed at writing an accessible textbook covering n-person games, but until then Rapoport (1966, p. 8) admitted having failed "in making an acceptable 'translation' of the N-person game theory." This is why he published *Two-Person Game Theory: The Essential Ideas* first, a book which—as its title suggests—avoided n-person games altogether. Interestingly, initially Rapoport (1966, p. 9) planned to publish *N-Person Game Theory* "when and if" he solved "the notation problem." Four years later, Rapoport (1970, p. 5) had accepted n-person game theory could not "be presented with a minimum of (mostly familiar) mathematical notation," thus he published *N-Person Game Theory: Concepts and Applications* without fully accomplishing his goal.

While *whom* textbooks of 1966–1970 addressed is a distinguishing feature between them, what differentiates from textbooks of 1950–1959 is *what* they addressed, n-person games. The inclusion of a new, vast subject of game theory calls for a discussion of how textbook authors organized (or reorganized) game theory. Regarding two-person games, almost nothing changed: Rapoport (1966, 1970) and Owen (1968) organized two-person games similarly, mimicking past texts such as McKinsey's (1952) and Karlin's (1959a) textbooks. Newer textbooks differed mainly on how much importance they gave to two-person problems vis-à-vis other topics. Rapoport (1966) followed the *Theory of Games* more closely, starting from foundations—including, for example, expected utility theory—and moved slowly. While it took him more than 100 pages to close his inquiry into two-person zero-sum games and start exploring related subjects, for Owen (1968) it took a little more than 30, who quickly moved to linear programming and variations of

[20] Rapoport (1966, p. 7) explained what hindered non-mathematicians in studying game theory was not a lack of a particular training—say, learning linear algebra—, but lack of a byproduct of mathematical training, which he called "certain habits of thought:" "[. . .] it is not the ability to play an instrument that is required in order to follow the development of a musical thought, say in a symphony, but rather 'musicality,' certain habits of listening. The ability to think mathematically is like the ability to listen musically. Some of this ability may be inborn; some may be acquired without technical training; and much of it comes with technical training."

elementary two-person games (infinite and multistage games, for example). Despite the page difference in their treatment of two-person games, the organization of their books was much similar in content.

Surprisingly, presentations of n-person games were also homogeneous in that regard. In particular, the organization of n-person games in Owen's (1968) and Rapoport's (1970) textbooks followed a four-step recipe (which made them similar). First, they redefined games in an n-person context. In moving from 2- to n-person games, von Neumann and Morgenstern ([1944] 2007, pp. 238-241) crafted what they called *characteristic function*. Put simply, instead of characterizing games by specifying players, what strategies they have, when they play, what information they have and so on, it was enough for a cooperative study to point who was playing and how much each possible coalition could obtain if all its members behaved optimally. This line of inquiry—of games in coalitional or characteristic function form—became *the* way game theorists investigated n-person problems. Second, they presented von Neumann and Morgenstern's ([1944] 2007) stable sets as a central solution concept of n-person games. While stable sets were subject to some criticism, they remained widely researched. Until here, things remained closely tied with the *Theory of Games*, but such a characteristic would change in subsequent steps. Third, textbook authors explored alternative solution concepts to stable sets. Fourth, they introduced ways of formalizing a cooperative game other than through characteristic functions. These last steps reflect changes n-person game theory underwent since 1944.

3.1.2 How Textbooks Labeled and Presented Solutions

Before 1966–1970, Luce and Raiffa's (1957) textbook already discussed n-person games, even if around 1957 most research still focused on two-person problems. *Games and Decisions*'s chapters 7–12 covered n-person theory: "Theories of n-Person Games in Normal Form," "Characteristic Functions," "Solutions," "ψ-Stability," "Reasonable Outcomes and Value," and "Applications of n-Person Theory." These chapters provided an organization of n-person game theory which slightly differed from what one finds in textbooks of 1966–1970. The first meaningful difference concerns how textbook authors labeled "solutions." Back in *Games and Decisions*, chapter "Solutions" concerned a specific solution concept: that of the *Theory of Games*. Interestingly, that textbook included a presentation of Nash's (1951) work, which Luce and Raiffa (1957, pp. 170-173) explained as discussing "equilibrium points," and not as *solving* a specific type of games. By then, n-person game theory was at its infancy, and notions of "solution" or "equilibrium" were under formation—they did not have a solid, stabilized meaning.[21]

[21] Curiously, von Neumann and Morgenstern ([1944] 2007) did not label their minimax solution as an "equilibrium," but some publications from the 1950s—including textbooks of 1952–1959—sometimes did so (for instance, Luce and Raiffa (1957, pp. 71-72) did it, but McKinsey (1952) and Karlin (1959a)

Games and Decisions was not crystal clear on how it decided to call some objects "solution" (and others "equilibrium," for instance). Chapter "Two-Person Cooperative Games" discussed how different authors meant different things when they spoke of solutions. At some length, Luce and Raiffa (1957, p. 120) questioned: "What then is the point and interpretation of a solution? Our views are given in the next section where we discuss arbitration schemes." For Luce and Raiffa (1957, p. 121), an *arbitration scheme* was a rule an arbiter could apply in solving a conflict among two players, who would both agree on that arbiter's decision. Importantly, such a rule should specify what each player would earn, leaving no room for indeterminateness.[22] Translating that rough notion, which referrer to two-person problems, into n-person games would be problematic, as n-person problems often suffer from having way too many solutions (in the *Theory of Games* sense). *Games and Decisions* did not say why stable sets were the only solutions, but hinted about it. The only apparent reason why von Neumann and Morgenstern's stable sets figured alone in chapter "Solutions" was because "In the published literature of n-person games one definition, based on characteristic functions and imputations, [it] has received primary attention," Luce and Raiffa (1957, p. 199) disclosed. The *Theory of Games* called its solution simply as "solution," and so such label stuck among game theorists around 1957.

This correspondence between "solution" and "the *Theory of Games*' solution" apparently grew out of habit, but it weakened a decade after Luce and Raiffa (1957) published their text. Rapoport (1970, p. 93) had a chapter called "The Von Neumann-Morgenstern Solution" which, in conjunction with its subsequent chapters, implied other "solutions" existed, whereas Owen (1968, p. 179) had a chapter whose title directly mentioned "Other Solution Concepts for n-Person Games." These mentions of "alternatives" do not only reflect research produced new solution concepts; a change was going on. Take, for instance, Shapley's (1953c, p. 307) contribution, in which he explored how "At the foundation of the theory of games is the assumption that the players of a game can evaluate, in their utility scales, every 'prospect' that might arise as a result of a play" to obtain an evaluation (called *value*) of how much worth is in playing a game. This value reads as an expected utility of joining a game, considering all possible coalitions that can emerge using a uniform distribution. Shapley (1953c) did not portray his value as a solution concept, nor did *Games and Decisions*; but textbooks of 1968–1970 did. Thus, Shapley's (1953c) value *became* a solution concept, and textbooks reflect so. Unfortunately, textbook authors did not precisely define what makes something a "solution," but textbooks suggest what they thought of it was broadening through

did not.)

[22] This rule should possess some properties, Luce and Raiffa (1957, p. 123) continued: it should provide each player at least what he would get by playing non-cooperatively; it should be independent of player labels; it should be independent of utility scales; it should not be oversensitive to small perturbations; and it should reflect each player's capabilities.

time.[23]

Apart differently labeling some objects to be "solutions," there is another significant difference between how Luce and Raiffa (1957), on one side, and Owen (1968) and Rapoport (1970), on the other, organized n-person game theory. As stated above, *Games and Decisions* had a chapter named "Theories of n-Person Games in Normal Form," which may sound reasonable to modern readers, but which was unexpected given the context at the time. Luce and Raiffa (1957, pp. 155-157) discussed strategic-form n-person games because of difficulties they had with the *Theory of Games'* way of studying such problems—collusion often involved *ad hoc* assumptions, and it became formidably more difficult to study games as one focused on larger values of n. Textbooks of 1968–1970, oppositely to *Games and Decisions*, did not discuss strategic-form n-person games. Owen (1968, p. 158) explained why strategic forms would not have a good use beyond two-person problems.[24] Mixed strategies were crucial for analyzing two-person games; randomization was "the essence" of two-person conflicts. However, as a third player joined a game, what was critical was not randomization, but coalition formation; and characteristic forms allowed highlighting coalitions, assuming players were optimally randomizing their moves in the background. While one could think of strategic-form n-person games, as Nash (1951) did, doing it would be missing an important point of game theory: practitioners valued studying coalitions. This identification of coalitions as "the essence" of n-person games has a crucial implication: it erased any roles non-cooperative game theory could play.

If n-person games have nothing to do with strategies, it would not make much sense to discuss non-cooperative games and their equilibria in a textbook presentation. This does not mean textbooks of 1968–1970 fully neglected Nash's (1951) work. While Rapoport (1970) skipped it completely, Owen (1968) brought it to his presentation of game theory—even if *very* briefly. To be more specific, Owen (1968, pp. 155-156) discussed Nash's (1951) contribution in an n-person context in a single page, followed by two lines in another (around 200 words in total). For Owen (1968, p. 155) "the principal question is the existence of equilibrium n-tuples" in non-cooperative games, and such a question already had an answer. Besides, even if he considered Nash's (1951) work valuable, Owen (1968, p. 155) felt "all the difficulties which were observed for equilibrium points of bimatrix games are also present here [in n-person games]." Thus, it seems textbooks neglected Nash's (1951) paper for some reasons, one of them being that non-cooperative games apparently did not pose interesting mathematical problems for mathematicians to solve. In opposition, solutions of characteristic function form

[23] Differently than in *Games and Decisions*, determinateness played no role in labeling objects as solutions as of 1968–1970, apparently: Rapoport (1970, p. 96) contended "Solutions which single out *sets* of outcomes rather than unique 'outcomes' are common in mathematics," exemplifying his point using a quadratic equation.

[24] Rapoport (1970, pp. 63-66) forwarded a similar reasoning.

games provided a wealth of problems and disputes. Interestingly, some of such issues played a role in how textbook authors organized solution concepts.

Taking a step back, Luce and Raiffa (1957, pp. 203-204) presented von Neumann and Morgenstern's ([1944] 2007) stable set solution for its historical relevance and its prominence in research and, subsequently, they addressed Shapley's and Donald B. Gillies's (1953) work, which crafted a notion of *core* in game theory (a set of feasible allocations, unimprovable by any coalition of players).[25] In Luce and Raiffa's (1957, 215-218) textbook, Gillies and Shapley's core sounded as an extension of von Neumann and Morgenstern's ([1944] 2007) approach, what is consistent with how Gillies (1953, p. 6) presented his contribution (a tool which should aid in finding stable sets). However, the textbooks of 1968-1970 reversed things in their presentation: Gillies and Shapley's core would come first, and von Neumann and Morgenstern's ([1944] 2007) solution would become a response to it. The organizing question regarded existence. Owen (1968, p. 163) said given a domination relation as von Neumann and Morgenstern ([1944] 2007, p. 264) defined, "An obvious first idea is to study the undominated imputations." Gillies and Shapley's core could comprise multiple imputations, but such a characteristic was not problematic. For Owen (1968, p. 164), a "greater difficulty" was that "it need not exist (i.e., it might be empty)." Because of its possible emptiness, Owen (1968, p. 165) argued "Inasmuch as the core is often empty, it becomes necessary to seek for some other solution concept." Rapoport (1970, pp. 90-91) went over a similar reasoning, showing all constant-sum n-person games yield empty cores, thus making "the core unsuitable [. . .] as a 'prescription to rational players,'" concluding: "We must therefore seek other bases for constructing 'solutions' of such games." Observe how lack of existence—which here translates as emptiness—could make a solution concept "unsuitable," making it "necessary" to look for an alternative. The alternative was von Neumann and Morgenstern's ([1944] 2007) solution.

The driving force running underneath expositions of solutions and alternative solutions concerned existence and uniqueness. As mentioned before, Lucas (1967, 1968) found a game having no stable set solution. Newer textbooks only partially absorbed such result. Owen (1968, p. 166) remarked that the *Theory of Games* solution had not yielded a general existence theorem. While uniqueness was certainly not possible—known games of multiple stable sets abounded—, Owen (1968, p. 166) stated: "no n-person game has been given which does not possess a stable set." In a footnote, Owen (1968, p. 166) added: "A 10-person game which has no stable set solutions has recently been constructed," giving a reference to Lucas's (1967) work (a RAND report). These statements contradict each other. It is not clear if Owen didn't have time to chew Lucas's (1967) argument (which he only outlined in that report), or if editorial constraints made it impractical

[25] Shapley's contribution occurred off-the-record. For more about it, see Chapter 6 (starting on page 129).

for Owen (1968) to make larger changes in the text when he became acquainted with Lucas's (1967) working paper. In turn, Rapoport (1970, p. 96) was fully aware of Lucas's (1967, 1968) 10-person counter-example; in an end note of his textbook, he appraised what it meant:

> Whenever an N-person game has both a Von Neumann-Morgenstern solution and a core, the set of imputations in the core must be contained in the set of imputations in the solution. In general, therefore, the core is the smaller set. However W. F. Lucas (see "A game with no solution" and "The proof that a game may not have a solution") has shown that a game may have a core but not a Von Neumann-Morgenstern solution. This was a surprising result, because it had been widely conjectured among game theoreticians that every N-person game has at least one Von Neumann-Morgenstern solution, and some effort was made to prove this result as a "fundamental theorem of N-person game theory," analogous to the "fundamental theorem of algebra," according to which every polynomial with complex coefficients has at least one complex root (complex numbers in this context include the real numbers). The hope of providing such a "fundamental theorem" for game theory has now been shattered by Lucas' counter-example. The consolation is that new avenues of investigations have been opened in the mathematics of game theory, namely questions concerning conditions for the existence (or non-existence) of solutions. [. . .] (RAPOPORT, 1970, pp. 312-313; underlining added)

The fact that Rapoport (1970, pp. 312-313) left such a consideration for an end note, and that Owen's (1968, p. 166) exposition had contradictory statements, make textbook organizations seem odd.[26] On one side, stable sets enjoyed a certain centrality in game theory, being more popular than other solution concepts and, remember, textbook authors presented stable sets as a more adequate solution notion vis-à-vis Gillies and Shapley's core (which could be empty). On another, Lucas (1967, 1968) made it clear that stable sets were not that much adequate for someone longing for an existence property. In fact, that reasoning in which stable sets fared better than cores fell to the ground in light of Lucas's (1967, 1968) research. Given that Owen (1968) and Rapoport (1970) were organizing game theory of a new, non-two-person era, some oddities are natural. Yet, they do point toward change: going back to von Neumann and Morgenstern's ([1944] 2007, p. 42) words quoted before, in which they said game theory would need a "fundamental change" in case some games did not have stable sets, one could expect textbook presentations of cooperative n-person game theory to substantially change once authors could fully grasp Lucas's (1967, 1968) counter-example. However, it seems cooperative game theory came to a halt.

[26] Instead of existence, other factors justified presentations of other solution concepts. For example, Owen (1968, p. 185) brought up Aumann and Maschler's (1964) *bargaining set* concept because it, unlike stable sets, could suggest what "may actually take place during a play of the game" (it highlighted a mechanism of threats players could use). Other similar stories backed presentations of other solution concepts, and they make alternative solutions seem like complements to a theory of stable sets.

Looking at later textbooks of 1985–1988, whose authors were mathematicians and which concerned mainly cooperative games, things are not much different from Owen's (1968) and Rapoport's (1970) texts.[27] Titles from Szép and Forgó (1985), Driessen (1988), and Jianhua (1988) have a similar structure to that which Owen (1968) and Rapoport (1970) provided: they presented cooperative games as a sequence comprising characteristic functions, stable sets, and alternative solutions. What changed, most notably, is a matter of order. Only Jianhua's (1988, pp. 117-117, 127) put stable sets in a position of "advantage" against cores (because of that emptiness) problem, but he added "The notion of stable sets is obviously an unsatisfactory concept of solution," citing Lucas (1969); Szép and Forgó's (1985) and Driessen's (1988) did not present cores before stable sets.[28] They make it look as if stable sets lost that centrality they held around 1968–1970.

Effectively, it seems cooperative game theory came to a halt. Not a halt meaning it lost its appeal; mathematicians continued to dig deeper, but seemingly it did not reach a "new era," as it happened when interests moved from two- to n-person games. Later textbooks would be successors of Owen's (1968) and Rapoport's (1970) manuals, updating them with new results, but they suggest education in cooperative game theory would go through similar lines in either 1968–1970 and 1985–1988. In such a view, textbooks addressing cooperative n-person games are not closely related to modern textbooks addressing economists. But there is a caveat: Rapoport (1970) divided his book in two parts. The survey so far concerned his Part I, "Basic Concepts," which addressed games and their solution in a mathematical sense. Part II, "Applications," diverged from past textbooks (and that of Owen (1968) as well). It points toward applications of cooperative game theory to other disciplines, including economics.

3.2 M. Shubik: The Cooperative Link

While Owen's (1968) textbook emphasized game *theory* from cover to cover, Rapoport (1970) divided his book in two parts: "Basic Concepts," which discussed n-person games and solution concepts emphasizing their theory, and "Applications," bringing up models which used game theory. This last part comprised chapter 12–20, respectively named as: "A Small Market," "Large Markets," "Simple Games and Legislatures," "Symmetric and Quota Games," "Coalitions and Power," "Experiments Suggested by N-Person Game Theory," and "'So Long Sucker': A Do-it-Yourself Ex-

[27] The point of looking at textbooks "whose authors were mathematicians and which concerned mainly cooperative games" is observing comparable objects; around 1985–1988, game theory would have penetrated other disciplines already. The cited textbooks here concern mainly cooperative n-person games.

[28] Szép and Forgó (1985) presented stable sets before cores, and Driessen (1988) started from Shapley's value.

periment" (and two chapters of a more reflexive nature, "The Behavioral Scientist's View" and "Concluding Remarks"). As names suggest, they addressed three types of applications: of markets, as in microeconomic theory; of voting models; and of experimentation.[29] This small number of applications of game theory to problems outside of mathematics reflected a scientific stage of underdevelopment. Rapoport (1970, p. 184) explained "the logical structure of strategic conflicts is indeed the prime and, at least at present, the only achievable objective of game theory." The best game theory could do, besides establishing new theorems, was "translating the purely mathematical concepts into highly simplified and idealized, but nonetheless imaginable, social situations," Rapoport (1970, pp. 184-185) continued.[30] However immature applications of game theory were, it is remarkable that by 1970 textbooks already started to discuss an intersection between game theory and microeconomics—even before non-cooperative games became influential.

Around 1961–1962, game theory yielded applications to different problems, some in economics, others not. Two mentions here are noteworthy. First, Gale and Shapley (1962, p. 9) studied matching problems of student college admissions and related problems (as those of marriage and roommate matching). Gale and Shapley (1962, pp. 13-14) proposed a "'deferred-acceptance' procedure" for their main problem, which determined a unique solution they believed to be optimal for students.[31] Second, Lewontin (1961, pp. 382-383) proposed studying population genetics using game theory, what implied rethinking game theory's language, adapting it to biology. This paper precedes Smith's (1982) work, which more famously brought the Nash equilibrium concept into biology (see Aumann's (1987, pp. 476-477) comment, for instance).[32] These two examples suggest game theory was reaching other fields—such as mechanism design and biology, even if at a slow pace. However, such applications were not central for Rapoport's (1970) exposition. It is unsurprising he discussed experiments—as they relate to his research agenda at the Mental Health Research Institute—, and even applications in voting, which were around since 1954.[33] What could cause startlement is that his textbook privileged studying markets. And more than that: even Owen (1968), whose focus was game *theory* proper and not applications, discussed markets as well, in a small section of his study of n-person games.

The link between markets and Owen's (1968) and Rapoport's (1970) textbooks

[29] Von Neumann and Morgesntern ([1944] 2007, pp. 564-573) and Shapley and Shubik (1954) discussed small markets of one seller and two buyers and voting games, respectively.

[30] Owen (1968, pp. v-vi) portrayed game theory as a "mathematical description of sociological phenomena," and judged his textbook would be "poor" if it did not relate both. But, he warned, all he provided were heuristic interpretations of mathematical concepts (i.e., intuition).

[31] This research is related to why Shapley received a Nobel prize in 2012 (alongside Alvin E. Roth).

[32] Dimand (2000, p. 203) listed a series of early application of game theory to a variety of disciplines, including Lewontin (1961) as a "precursor" of Smith (1982).

[33] The key reference here is Shapley and Shubik's (1954) work.

comes from Shubik's (1959a) work linking Francis Y. Edgeworth's exchange model and game theory, in what he called "Edgeworth market games." Shubik (1959a, p. 267) argued that while mathematicians investigated solutions for n-person games, they paid little attention to economic analysis, so he would extend "a bargaining or bilateral monopoly problem studied by Edgeworth" using games. This problem in point concerns exchange between two agents. Shubik (1959a, pp. 268-269) thought its solution in economics—known as *contract curve*—was "very similar" to von Neumann and Morgenstern's stable sets because both were "a set of distributions which do not dominate each other but dominate all other distributions." However, there was a point of divergence between both: Edgeworth's contract curve did not allow for side-payments, whereas stable sets did. Among others, Shubik (1959a, pp. 271-272) formally explored differences and similarities about both solutions, and also studied what would happen to them as more consumers joined such a market. More importantly, Shubik (1959a) made game theory step into microeconomics 15 years after the *Theory of Games* was around.

There is a caveat, however: research papers citing Shubik's (1959a) work provide a different picture of its reception than what one finds in textbooks. Effectively, Shubik's (1959a) research repercussed in economics through general equilibrium theory. In particular, it stimulated models using Gillies and Shapley's core as a tool of general equilibrium, useful for characterizing Walrasian equilibria, as one finds in Debreu and Scarf's (1963) work—indeed, 9 of their 12 cited references came from game theory. This connection between microeconomics and Shubik's (1959a) market games quickly lost its game theory content, however. The *core* notion was already around economics before, and references to game theory in this literature became mostly historical—using it in general equilibrium models involved nothing of strategic nature.[34] Textbooks, in opposition, provided a faithful presentation of Shubik (1959a) work. Owen (1968, p. 172) covered "Edgeworth Market Games" as an application of n-person game theory to economics, in a sort of exception given that until then his exposition remained strictly interested in "mathematical ideas." At first, Owen (1968, p. 172) commented Edgeworth's original work and von Neumann and Morgenstern's ([1944] 2007) book provided "quite analogous" solutions to that exchange problem. However, Owen (1968, pp. 172-174) argued that while Edgeworth's approach suggested a shrinking of contract curves would occur in largely populated markets (when infinitely many players participated in it), stable sets did not. This collapsing tendency was something Shubik (1959a, p. 271) found out (Edgeworth only assumed it would happen). Rapoport's (1970, pp. 202-204) rested on a similar reasoning. Thus, Shubik's (1959a) market game provided an environment in which cores had an edge over stable sets: while in game *theory* proper the *Theory of*

[34] For example, Hildenbrand (1968, p. 443) cited Shubik (1959a) only saying "The connection between Edgeworth's contract curve and the core of a game was pointed out by M. Shubik."

Games' solution was dominant, becoming even a response to a fault in cores, matters changed in economic analysis.[35]

Still, textbook presentations did not get much beyond what one finds in Shubik's (1959a) paper, what could suggest not much research followed from it (outside of general equilibrium theory, naturally). Citations to Shubik (1959a) corroborate such suggestion. Until 1970, his work received only one mention outside general equilibrium.[36] This citation came from Farrell (1970), who applied the core in an oligopoly model. Still, when cooperative game theory reached a watershed moment—with Lucas's (1967, 1968) counter-example—, textbooks suggested applications of games in economics should also benefit from not focusing on stable sets. This lead would become fruitful not much after Owen (1968) and Rapoport (1970) published their texts. In his acknowledgements, Farrell (1970) said he was specially thankful to James W. Friedman, an economist who obtained his Ph.D. in 1963 and was still establishing himself professionally. Friedman, who would publish a game theory *and* economics textbook, is a pivotal character in bringing Nash's (1951) contribution in non-cooperative games to textbooks.

3.3 Brief Concluding Remarks

To distinguish G. Owen's and A. Rapoport's textbooks as belonging to a new generation of textbooks is easy. Until 1968, game theory textbooks were offspring of a two-person world: they focused on that part of the *Theory of Games* which dominated research agendas, personal and institutional, often spinning off to linear programming. The new generation put out a radically different type of textbook, which focused on n-person games. This part of game theory was unfinished: open (and critical) questions abounded. This rough state says what was most important for new presentations of game theory. What characterized textbooks of 1952–1959 was an effort of making game theory adequately readable by different audiences. While readability was still a concern (more easily seen in Rapoport's frustrated project), textbooks sought an organization of n-person game theory. By 1968–1970, several alternative solution concepts would had emerged, and authors had to make sense of them.

The very nature of textbooks impose a task of organizing subjects of a discipline—in a shallow level, saying what comes before and what comes next. But in linking matters in a sequence, textbook authors frequently have to justify why they do so. In particular, Owen and Rapoport had to fit D. B. Gillies and L. S. Shapley's core somewhere, and

[35] Remember, cores can be empty, what would be problematic in solving a market games. However, Shubik (1959a, pp. 273-274) established sufficient conditions for a particular imputation to always be in the game's core. Later, Bondareva (1963) and Shapley (1967) would provide more general conditions for a n-person game to have a non-empty core. This result became known as "the Bondareva-Shapley theorem."

[36] This point rests on Clarivate's *Web of Knowledge* database.

they also could not refrain from presenting J. von Neumann and O. Morgenstern's stable sets, perhaps what one have closest to a "main" solution concept. To do so, authors could faithfully follow what one finds in research; in Gillies's dissertation, the core is just an instrument, something devised for supporting mathematicians in finding stable sets. Textbooks could replicate that description, but instead, they build up Gillies and Shapley's core as a natural way of solving games, yet defective, which motivated one to study von Neumann and Morgenstern's stable sets. What drove textbook presentations was a quest for finding a satisfactory solution concept, mirroring what happened in research papers. What made some solution "satisfactory" was not only mathematical properties—if it was, Shapley's value concept would have triumphed, as it has compelling properties of existence *and* uniqueness. Textbook authors (and theorists) wanted more; by looking at other solution concepts coming up in textbooks, one could suggest a more descriptive solution concept, saying specifically how do players bargain in joining coalitions and splitting group payoffs, was something they desired. A final, decisive answer to how to solve a game would never come, however.

The game theory textbook would come to a sort of halt after Owen and Rapoport published their texts: other textbooks emphasizing n-person games would only reappear around 1985 onwards, but even then without substantial changes. Textbook presentations of n-person cooperative game theory of 1985 forwards paralleled what Owen and Rapoport proposed years before. Instead of theory-oriented texts, what seemingly characterizes game theory textbooks published after 1970 is specialization. Newer books would either specialize in a mathematical sense, covering details of specific topics in game theory, or they would select parts of game theory and apply them to other disciplines. Some examples of such areas are social science, political science, linguistics, and theology. At that moment in which textbooks branched out to several fields, manuals bridging game theory and economics also started to appear, what frames another "generation" of game theory textbooks.

4 The Slow Rise of Non-Cooperative Games, 1972–1978

After Rapoport (1970) published his volume on n-person games and before 1978 (a period which marks another generation of textbooks), few new game theory textbooks appeared. In Tables 6 and 7 below, only Davis (1970) and Vorobyov (1977) published what could be called "descendants" of Owen's (1968) and Rapoport's (1970) texts, providing an agnostic and ample coverage of game theory, from 2 to n players. Most books of 1972–1978 concerned either some specialty within game theory, as those about differential games, or an application of games to another field, such as political science. But as Part II of Rapoport's (1970) manual showed, cooperative game theory had started to accrue applications in economics by 1970. Weintraub's (1975, unnumbered page) textbook reinforces such a suggestion, considering it addressed "undergraduate students of economics and their teachers:" he said game theory had been "used with increasing frequency to illuminate and synthesize various problems formerly treated by the calculus," providing "useful insights about economic processes."[1] What Rapoport's (1970) book organization and Weintraub's (1975) comment suggest is that game theory, after the *Theory of Games* had been in shelves for nearly three decades, was finally permeating economics.

This entry of game theory into economics did not happen through a single channel; textbooks oriented toward economists (such as Weintraub's (1975) and Bacharach's (1977)) discussed exchange economies, oligopolies, externalities, labor market bargaining, and voting. But among different applications of game theory in economics, one ruled all others: industrial organization. Here, "industrial organization" designates models of non-competitive markets.[2] While it is well-known that Tirole's (1988) textbook of game theoretical industrial organization is a landmark in game theory's history, it was not *the first* book to put industrial organization in game theory clothes: during 1972–1978, economists James W. Friedman and Lester G. Telser crafted textbooks of industrial organization using game theory as their chief tool.[3] Unlike Tirole's (1988) exposition, which focused in Nash's (1951) equilibrium concept and its refinements developed

1 Weintraub's (1975, unnumbered page) hoped his introductory text would persuade students to pursue further instruction with Rapoport's (1966, 1970) sequence on 2- and n-person games. Furthermore, Weintraub (1975) was not alone in his quest of introducing game theory to economists: Bacharach (1977, p. viii), a British economist, held a similar objective.

2 For instance, see Schmalensee's (2018, p. 6325) definition, who stated "industrial organization" encompasses "the field of economics concerned with markets that cannot easily be analysed using the standard textbook competitive model."

3 For an example of such acknowledgement concerning Tirole's (1988) textbook, see Dimand's (2000, 202-203) comments.

Table 6 – List of Game Theory Books, 1971–1974

Author	Title	Year	Rev.	Textb.
M. D. Davis	*Game Theory: A Nontechnical Introduction*	1970	2	✓
A. Rapoport	*N-Person Game Theory: Concepts and Applications*	1970	4	✓
R. F. Barton	*A Primer on Simulation and Gaming*	1970	2	✓
A. Friedman	*Differential Games*	1971	3	✓
T. Parthasarathy and T. E. S. Raghavan	*Some Topics in Two-Person Games*	1971	2	✓
M. Nicholson	*Conflict Analysis*	1971	4	✓
M. Nicholson	*Oligopoly and Conflict: A Dynamic Approach*	1972	5	
L. G. Telser	*Competition, Collusion, and Game Theory*	1972	5	✓
L. S. Wrightsman Jr., J. O'Connor, and N. J. Baker	*Cooperation and Competition: Readings on Mixed-Motive Games*	1972	0	
T. C. Schelling	*Zero-Sum Games*	1973	0	✓
T. J. Fararo	*Mathematical Sociology: An Introduction to Fundamentals*	1973	3	✓
A. Blaquière (Ed.)	*Topics in Differential Games*	1973	1	
R. R. Singleton and W. F. Tyndall	*Games and Programs: Mathematics for Modeling*	1974	3	✓
A. Rapoport (Ed.)	*Game Theory as a Theory of Conflict Resolution*	1974	0	
R. J. Aumann and L. S. Shapley	*Values of Non-Atomic Games*	1974	2	

Source: Each listed book appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. The number of book reviews comes from JSTOR's database and Google Scholar searches.

Table 7 – List of Game Theory Books, 1975–1978

Author	Title	Year	Rev.	Textb.
A. Zauberman	*Differential Games and Other Game-Theoretic Topics in Soviet Literature: A Survey*	1975	1	
E. R. Weintraub	*Conflict and Co-Operation in Economics*	1975	1	✓
M. Shubik	*The Uses and Methods of Gaming*	1975	4	✓
S. J. Brams	*Game Theory and Politics*	1975	3	✓
M. Shubik	*Games for Society, Business, and War: Towards a Theory of Gaming*	1975	4	✓
J. McDonald	*The Game of Business*	1975	0	
A. Rapoport, M. J. Guyer, and D. G. Gordon	*The 2x2 Game*	1976	1	
J. Rosenmuller	*Extreme Games and Their Solutions*	1977	0	
N. N. Vorobev	*Applications of Mathematics, Volume 7: Game Theory*	1977	1	✓
M. Bacharach	*Economics and the Theory of Games*	1977	0	✓
J. W. Friedman	*Oligopoly and the Theory of Games*	1977	3	✓
J. C. Harsanyi	*Rational Behavior and Bargaining Equilibrium in Games and Social Situations*	1977	4	
R. Henn and O. Moeschlin (Eds.)	*Mathematical Economics and Game Theory: Essays in Honor of Oskar Morgenstern*	1977	1	
K. C. Bowen	*Research Games: An Approach to the Study of Decision Processes*	1978	1	
L. G. Telser	*Economic Theory and the Core*	1978	4	✓
P. C. Ordeshook (Ed.)	*Game Theory and Political Science*	1978	1	

Source: Each listed book appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. The number of book reviews comes from JSTOR's database and Google Scholar searches.

mostly 1975 onward, textbooks of 1972–1978 focused on either one side of game theory, cooperative or non-cooperative; it was not clear which would win economists. In one of his textbooks, Telser (1972) provided a short historical account telling how things reached such a state:

> Although the nature of a market and what happens there is surely a proper subject of economic analysis, the student will search the literature in vain for more than passing mention of this fundamental topic prior to 1881 when Edgeworth published his profound analysis of markets. The next important contribution did not appear until a decade later in Bohm-Bawerk's celebrated study of a horse market containing the first rigorous constructions of market supply and demand schedules. This paucity of early analysis is all the more surprising when we recall that in the 1870s economics embarked on its modem rigorous course with the contributions of Jevons, Menger, and Walras. After Bohm-Bawerk a half century passed before the next major contribution, the publication of von Neumann and Morgenstern's *Theory of Games* (1944). But the reviewers found in game theory little of relevance for economics, <u>and it was not until 1959 that Martin Shubik pointed out that Edgeworth's theory could be married to game theory to produce a formidable new approach to the study of competition. This approach is now known as the theory of the core. Nor is this all. In 1838 Cournot developed a mathematical theory of competition generally condemned and misunderstood in most textbooks, which turned out to be the forerunner of the minimax theorem of game theory as applied to nonzero-sum games. This became clear shortly after J. F. Nash in 1950 published his work on equilibrium points, and economists became aware of their connection to Cournot's theory</u>. Rigorous research into competition has been growing lustily only since 1959. This curious tale may engage the attention of the historian of economic thought, but it is not our further concern. (TELSER, 1972, p. xiii; underlining added)

As Telser's (1972, p. xiii) historical recap points out, game theory had two contact points with economics: one remembering Edgeworth (1881), which refers to Gillies and Shapley's core and cooperative games, and another recalling Cournot (1938), related with Nash's (1951) equilibrium and non-cooperative games. These connections were around before 1960, and yet they would only flourish almost two decades later. Textbooks show early attempts in bridging game theory and economics, possibly working as "parents" of Tirole's (1988) classic. It is interesting that both approaches to games, cooperative *and* non-cooperative game theory, ended serving industrial organization around 1972–1978. They *coexisted* in economics, in a situation much different from what would expect given that modern industrial organization, as Tirole (1988) presents, has no connections with cooperative games. Textbooks of 1972–1978 clarify how such process succeeded.

4.1 The Aftermath of Cooperative Game Theory

When Owen (1968) and Rapoport (1970) published their textbooks, n-person game theory was reaching a stalemate. For decades researchers sough an existence result for von Neumann and Morgenstern's ([1944] 2007) stables sets, and Lucas (1967, 1968) showed such a quest was vain. As argued before, Owen (1968) and Rapoport (1970) published their texts almost simultaneously with Lucas (1967, 1968), what explains some oddities they display. In particular, they presented stable sets as championing over other solution concepts, including Gillies and Shapley's core, even if von Neumann and Morgenstern's ([1944] 2007) solution was inadequate (it could be non-existent just as cores could be empty). However, one could expect that, with time, game theory would reorganize itself, disposing off any ambiguities. Examining such process relying on textbooks is particularly challenging since, as Tables 6 and 7 show, most new textbooks were not precisely successors of Owen's (1968) and Rapoport's (1970) volumes. Newer expositions concerned applications of game theory in different areas, or specialized subareas of game theory. In practice, there is no textbook fully permitting such an analysis. However, two proxies are available: Vorobyov's (1977) textbook, a sort of translation from a Russian book; and lectures notes from Aumann (1971, 1976) and Maschler (1973) (of courses they offered in Stanford University), available on Alvin E. Roth's archival material available at the Economists' Papers Archive of Duke University.[4]

4.1.1 Foreign Traditions of Game Theory

Before Lucas (1968, 1969) published his counter-example showing the *Theory of Games* solution for n-person games would never produce an existence theorem, game theorists already sought alternative solution concepts for games. The oldest examples date from 1950–1953, with works from Nash (1950b, 1951)—who crafted the notion of non-cooperative games—, and Shapley (1952, 1953c) and Gillies (1953)—who thought of concepts which later became solutions (namely, the Shapley value and the core). These and newer alternatives figured in textbooks of 1968–1970: while Owen (1968) organized subsections dedicated to "The Shapley Value," "The Bargaining Set," and "ψ-Stability," Rapoport (1970) wrote chapters for "The Shapley Value," "The Bargaining Set," and "The Kernel." Bargaining sets and kernels emerged outside of American game theory. By 1970, foreign game theory traditions certainly existed in Israel, Russia, and possibly also in France and Germany.[5] The history behind them is not known and difficult to penetrate into; yet, it is undeniable they exerted a significant degree of influence over American game theory. For illustration, consider Rapoport's (1970, p. 7) acknowledgement of Israeli game theory:

> The reader will note that the authors cited are predominantly American and Israeli. This reflects the continued interest in the United States and in Israel in the application potential of game theoretic ideas to social

[4] Aumann's (1976) lecture notes became a book in 1989. Aumann (1989, unnumbered page) did not change his original lecture notes, except by minor corrections.

[5] Nessah, Tazdaït and Vahabi (2021) provided an account of game theory in France around the 1950s.

> science. There is also a large Russian literature; but, to the extent that I have examined it, it is of interest only to the mathematical specialist, and so falls outside the scope of this book. (RAPOPORT, 1970, p. 7; underlining added)

The aforementioned Israeli tradition of game theory seemingly originated from Robert J. Aumann's work. Born in Germany in an orthodox Jewish family, Aumann emigrated to the United Space as Nazism grew in is home country. He obtained a Ph.D. from the Massachusetts Institute of Technology's department of mathematics under George W. Whitehead with a thesis in knot theory—a subfield of mathematical topology—named "Asphericity of Alternating Linkages." After finishing his graduate education in 1955, Aumann started at a job at the Analytical Research Group, a consulting firm affiliated with Princeton University. There, Aumann (2005, no page) recollected, he studied a war-related problem of "defending a city from attack by a squadron of aircraft, a few of which are carrying nuclear weapons, but most of which are decoys." This case remembered him of Nash, who told Aumann (2005, no page) "a little about game theory" in his Ph.D. years: "I [. . .] figured that game theory had to be the right tool to attack this problem. So I studied some game theory—just enough for this problem—and then the subject started attracting me in its own right. The rest is history, as the saying goes." Back in 1948, with the establishment of the state of Israel, Aumann made plans to leave the United States; in 1956, he took a position at the Hebrew University of Jerusalem, where he would stay his whole career. There he would supervise Ph.D. students in mathematics, among which one finds, for example, Bezalel Peleg, who got his degree in 1964 (and whose Ph.D. dissertation was about game theory).[6] Assuming an appointment of assistant professor as early as 1962, Peleg would also make his career researching game theory at the Hebrew University. Aumann's (2005, no page) influence ran beyond students: he brought already established Israeli mathematicians into game theory as well—importantly here, including Michael Maschler.

Maschler was a mathematician who studied and worked at the Hebrew University. While his original interests lied on complex analysis, his research agenda shifted after he met Aumann in Jerusalem. In late the late fifties, Aumann (2008, pp. 355-356) presented a colloquium on von Neumann and Morgenstern's ([1944] 2007) solution concept; Maschler attended it and later "asked several questions that challenged its appropriateness." They continued their discussion privately for months, and its outcome was *The Bargaining Set for Cooperative Games*—a paper with a new solution concept which would figure in American textbooks of 1968–1970. Most notably here, Aumann, Maschler, and other game theorists from the Hebrew University would establish a connection with research centers in the United States, so somehow they ended giving

[6] Among Aumann's Ph.D. students who wrote thesis in game theory one finds, among others, Sergiu Hart, Elon Kohlberg, Abraham Neyman, David Schmeidler, and Shmuel Zamir.

lectures on Stanford University.

The story behind Aumann and Maschler work and its influence has a significant amount of gaps. Yet, things are even more obscure when one looks at Russian game theory. Nikolai N. Vorobyov, a "key textbook author" here, is a main character of Soviet game theory. Trained as a mathematician, his early interests lied in abstract algebra, mathematical logic, and probability theory, but around 1955 game theory grabbed his attention.[7,8] His book *Game Theory: Lectures for Economists and Systems Scientists* reads as a translation of a textbook—and not a "textbook proper," written by Vorobyov himself. Kotz (1977, p. v), a mathematician from Temple University (who obtained his Ph.D. from Cornell University in 1960), "used lecture notes based on the Russian original of this book [Vorobyov's] during the academic year 1975-1976 at Temple University." Vorobyov's (1977, p. vii) original was, in turn, based on "a number of lectures given frequently by the author to third year students of the Department of Economics at Leningrad State University who specialize in economical cybernetics." Mixing teachings and writings of different authors in different countries, *Game Theory: Lectures for Economists and Systems Scientists* possibly is not a faithful representation of both. Regardless, what such book shows goes hand-in-hand with Aumann's (1971, 1976) and Maschler's (1973) lecture notes, suggesting changes going on in game theory were not restricted to any particular place.[9] They show a similar picture of game theory in a post Lucas (1967, 1968) world.

4.1.2 What Happened to Stable Sets

The crucial question is how game theorists reorganized—if they did it—their field in light of Lucas's (1967, 1968) counter-example. From Maschler's (1973) and Aumann's (1976) notes, it seems von Neumann and Morgenstern's ([1944] 2007) stable sets were losing their dominance over other solution concepts of n-person game theory around 1973–1976. Truly, Maschler (1973) emphasized what he and his colleges of the Hebrew University researched: bargaining sets, kernels, and nuclei. Remember, Aumann (2008, p. 355) said Maschler started in game theory by challenging stable sets, and Maschler participated in crafting bargaining sets and kernels.[10] More importantly, Maschler (1973, pp. 4-6) skipped on presenting stable sets altogether in his lectures, choosing Gillies and Shapley's core as a starting point for solving games, and then proceeding to topics of

7 The Russian Academy of Sciences had a laudatory webpage dedicated to Vorobyov's work, available at the *Wayback Machine – Internet Archive* website (see Russian Academy of Sciences (2022)).

8 Vorobyov's advisor during his Ph.D. was Andrey A. Markov, who worked on stochastic processes (and from whom the name "Markov chain" comes from).

9 Maschler's (1973) lecture notes are not dated. However, using comments from Zamir (2008, p. 386), is it possible to deduce they are from 1973, since he said Maschler' lecture notes "were published at the Hebrew University (1970), at the IMSSS [Institute for Mathematical Studies in the Social Sciences] at Stanford University (1973), and at the Institute for Advanced Studies in Vienna (1978)."

10 While Schmeidler (1969) developed nuclei, these are closely related to Davis and Maschler's (1965) kernels

his research agenda. Moreover, it is remarkable how Aumann (1976) presented Gillies and Shapley's core and Shubik's (1959a) "Edgeworth Market Games" without reference to the *Theory of Games* solution concept, deviating from what textbooks did until then. This switch in emphasis signals a deeper change in how mathematicians approached stable sets, and game theory more broadly. To start appraising what such a shift meant, consider a passage from Lucas's (1971) survey on game theory, titled "Some Recent Developments in *n*-Person Game Theory:"

> It may be of interest to speculate about the importance of the nonexistence of vN-M stable sets for some games (in characteristic function form). [...] Many people have made contributions to research on stable sets in the past, and there are still many interesting problems to be solved for this model. One may guess that the recent results would cause a renewed interest in the vN-M theory and a temporary increase in the amount of research on these problems in the near future. However, if this work is not quite productive, then it is likely that research on their model may slacken in a short time. On the other hand, if a general existence theorem were to have been proved, then much more work on stable sets would have been expected as well as attempts to relate them to other mathematical concepts. With the discovery of a counterexample, however, it now appears that the primary importance assigned to the question of existence may not have been justified at least from the mathematical point of view. (LUCAS, 1971, pp. 516-517; underlining added)

This "question of existence" in Lucas's (1971) quote governed how textbook authors explained von Neumann and Morgenstern's ([1944] 2007) stable sets in 1968–1970: they introduced stable sets as an answer to Gillies and Shapley's core, which could be empty. Presentations were different by 1976–1977: another problematic feature of the core substituted that "question of existence." As in Lucas's (1971, pp. 516-517) words above, it seems mathematicians were rethinking how important existence should be. This is clearly seen in Vorobyov's (1970) textbook and Aumann's (1976) lecture notes. While Aumann (1976, pp. 40, 56) discussed a game of empty core, his introduction to stable sets initially rested on a historical *raison d'être*, since they were "the first solution concept" and because they were "extensively studied" by von Neumann and Morgenstern ([1944] 2007) and others—and not because cores may be empty. In his exposition, Aumann (1976, pp. 56-74) did not mention some games do not admit stable set solutions. When Aumann (1976, p. 58) defined stable sets, he positioned them as providing an improvement over cores, but unlike past textbooks, such an improvement did not concern existence:

> The stability concept underlying the definition of the core could be criticized as being too strong. It does not seem natural to exclude as unstable a dominated payoff vector when the dominating payoff vector is itself not stable. This suggests we should focus our attention on domination by stable imputations. (AUMANN, 1976, p. 58; underlining added)

That is, again Gillies and Shapley's core served as a first yet defective solution, which motivated a study of stable sets. But authors changed what needed correction: it was not existence any longer, but its underlying rationale. A similar reasoning permeated Vorobyov's (1970) exposition. Vorobyov (1970, pp. 140-141) explained that Gillies and Shapley's core consisted of "imputations that are stable but in a somewhat negative or passive sense" because "the properties of imputations in the core contain no recommendation that these imputations should be used, and moreover these properties do not help us to set them off against other suggested (or recommended) imputations." Stable sets worked as an improvement over cores because they brought with them an optimality principle which Gillies and Shapley's core did not. Such a principle was not ideal, however: Vorobyov (1970, p. 142) argued "the optimality principle inherited in a vN-M solution is not universally realized," meaning that, although stable sets could not exist, it would be desirable to know under which conditions they do. Moreover, its optimality principle was not complete because it did not yield a single payoff distribution scheme; that is, stable sets had a severe multiplicity, which he thought was undesirable. The takeaway lesson is that, much like Lucas's (1971, pp. 516-517) suggested, existence was receiving a new meaning in game theory, at least from mathematicians.

This little twist in how Aumann (1976) and Vorobyov (1977) approached stable sets is remarkable. Surely, von Neumann and Morgenstern's ([1944] 2007) solution for n-person games produced discontent since the *Theory of Games* was out but, in spite of any quarrels, who kept doing game theory until circa 1970 were mathematicians. Possibly because of their disciplinary image, mathematical properties took precedence over any defects stable sets had (for instance, of not explicitly modeling how agents behave in a game, leaving any mentions to "strategies" behind; or of its serious multiplicity, which rendered stable sets unsuitable for prediction). But in making sense of Lucas's (1967, 1968) negative result, mathematicians decided to not "throw the baby out with the bathwater"—instead of criticizing stable sets for possibly being non-existent, as they did previously with Gillies and Shapley's core, they reevaluated how central existence should be. But there is more to it. As it could be expected, game theorists also looked at other parts of their discipline which had received less attention until then, in a natural reaction to Lucas's (1967, 1968) counter-example. One of such parts is Nash's (1951) non-cooperative approach.

4.1.3 The Iconic Non-Cooperative Examples

Past textbooks frequently left out Nash's (1951) contribution of their presentations. The most notable exception until 1972 was still Luce and Raiffa's (1957) volume. It dedicated a four-page section to Nash's (1951) non-cooperative games, and it also brought up equilibrium points in other parts of its exposition, especially because of their equivalence with von Neumann's (1928) minimax: when a two-person game is also a zero-sum, players' interests are diametrically opposed, so they are naturally non-cooperative. Here, equilibrium points and minimax strategies coincide. Luce and Raiffa (1957, pp. 170-173) described such a situation, and then they pointed some problems with equilibrium points (of non-equivalence and non-interchangeability).[11] Things would be different for the texts of 1976–1977. The sheer extent of pages dedicated to Nash's (1951) equilibrium hint some change was in course: Aumann (1976, pp. 7-26) and Vorobyov (1977, pp. 90-115) dedicated *full chapters* to Nash's (1951) games.

Much like Luce and Raiffa (1957, pp. 170-173) did, Vorobyov (1977, pp. 1; 90-91) and Aumann (1976, pp. 7-9) presented n-person non-cooperative games as a natural generalization of two-person zero-sum games. Starting from a two-person game, Aumann (1976, p. 8) argued not all "strictly competitive" games have a minimax value; by then, his analysis concerned pure strategies only. To fill in such gap, Aumann (1976, p. 8-9) presented Nash's (1951) non-cooperative game and equilibrium point, emphasizing a necessary and sufficient condition for a strategy profile to be an equilibrium of a two-person game (put simply, it should satisfy a sort of minimax property). In a sense, Aumann (1976, p. 8-9) presented Nash's (1951) work as a generalization of von Neumann's (1928) Minimax Theorem, and it is quite natural he did so: Nash (1951, p. 286) himself did it (possibly because they were mathematicians and "generalizing" a theorem had a value of its own). However, even if newer presentations still presented Nash's (1951) paper as a generalization, as Luce and Raiffa (1957) did, they did not appraise non-cooperative games in light of two-person non-zero-sum theory—put it more specifically, they did not seek in Nash's (1951) framework properties found for von Neumann and Morgenstern's ([1944] 2007) minimax in two-person zero-sum games, as *Games and Decisions* did. The basis for evaluating Nash's (1951) contributions had changed.

In fact, expositions cared less about *appraising* Nash's (1951) framework, and more about giving it meaning. The "meaning" here is an interpretation. Aumann (1976, pp. 17-18) discussed two ways of interpreting equilibria. Fist, he suggested equilibrium points could be read as self-enforcing agreements; if participants wrote down they would play at an equilibrium (in a sort of commitment), they would not have advantages in doing otherwise. An example of situation in which that interpretation is appealing is that of "international treaties and illegal collusions on constrained trade," Aumann (1976, p. 17) contended. Furthermore, Aumann (1976, p. 18) said when players cannot communicate, equilibria could be "prominent" or "natural" results so "each player has reason to believe that the other one will play in accordance with it." For instance, in

[11] "Interchangeability" means that if players pick strategies of *different* equilibrium points, they will still end up in equilibrium; "equivalence" means players do not prefer one equilibrium over another (they all yield a single payoff distribution).

a game of multiple payoff-equivalent equilibria, a mixed strategy equilibrium would be natural given players cannot agree on either equilibrium point.[12] But as Aumann (1976) and others sought an explanation of what Nash's (1951) equilibrium meant, they ended up putting in new shape old, iconic examples of game theory—the "Prisoner's Dilemma" and the "Battle of the Sexes."

The history behind the Battle is not documented anywhere. Apparently, there are no mentions to it prior to 1957 (when it came up in *Games and Decisions*).[13] Luce and Raiffa (1957, pp. 90-91) mentioned a game for which "Various interpretations are possible, but one seems most familiar; we may call it the 'battle of the sexes.'" Luce and Raiffa (1957, pp. 90-94) did not provide a source or original author for that example, nor did they claim it was an original thought of themselves.[14] Then, two possibilities follow: either someone crafted the Battle and discussed it in unpublished artifacts (such as RAND memoranda or else), and *Games and Decisions* brought it to a "formal" publication; or Luce and Raiffa (1957) created it, but they did not believe it to be significantly important as to explicitly claim its authorship. In any case, Luce and Raiffa (1957, pp. 90-91) presented the Battle because it showcased how much more difficult two-person non-zero-sum games are *vis-à-vis* two-person zero-sum problems. The Prisoner's Dilemma, in opposition, has a well-known history.[15] As Erickson (2015, pp. 129-131) contended, the Dilemma was a device RAND mathematicians thought of to challenge von Neumann and Morgenstern's ([1944] 2007) cooperative and Nash's (1951) non-cooperative approaches. Experimentation should show which solution would win, but results were inconclusive—the Dilemma did not make it clear in which path should game theory develop itself. The interesting point about both examples is that their content shifted through time, as game theory itself experience large changes.

Back in 1957, *Games and Decisions* used the Battle and the Dilemma to question Nash's (1951) non-cooperative approach an equilibrium concept for non-zero-sum games. Consider the Battle in Figure 2a. Luce and Raiffa (1957, p. 104) contented "If there is to be a non-cooperative theory for this game, the least we can expect it to do is to suggest a strategy or class of strategies for each of the players." For them, Nash's (1951) framework did not provide a fully satisfactory answer. The problem is that if Players 1 and 2 should make their decisions simultaneously, without communication, playing an equilibrium point would not be an easy task. They would repeatedly try to outthink one another. Consider Player's 1 viewpoint. To show why Nash's (1951) approach would be

12 Vorobyov (1977, pp. 105-109) provided slightly different discussions. Put simply, he used the Prisoner's Dilemma to illustrate what precisely non-cooperativeness means, and used the Battle of the Sexes to point that fairness and higher payoffs do not always go together.

13 This search refers to querying results from JSTOR and Google Scholar using "Battle of the Sexes" and "Game" as search expressions.

14 Differently, Luce and Raiffa (1957, p. 91) said the Prisoner's Dilemma had a specific creator: "is attributed to A. W. Tucker."

15 Check Poundstone's (1992, pp. 101-131) work, for example.

Figure 2 – The Iconic Examples of Non-Cooperative Games

Player 1 \ Player 2	β_1	β_2
α_1	2, 1	−1, −1
α_2	−1, −1	1, 2

(a) The Battle of the Sexes

Player 1 \ Player 2	β_1	β_2
α_1	0.9, 0.9	0, 1
α_2	1, 0	0.1, 0.1

(b) The Prisoner's Dilemma

Source: Luce and Raiffa (1957, pp. 90, 95).

tricky, they Luce and Raiffa (1957, p. 92) develop a monologue of Player 1's reasoning in picking his move:

> I want (α_1, β_1) and clearly my opponent wants (α_2, β_2), but if I take α_1 and she takes β_2, then we both lose out. Suppose, then, that I give in and take α_2—I still will do pretty well. But player 2 may reason the same way and give in to me, and again we would both lose with the α_2, β_1 pair. Indeed, whatever rationalization I give for either α_1 or α_2 there is, by the symmetry of the situation, a similar rationalization for player 2, and so it seems inevitable that we both lose. (LUCE; RAIFFA, 1957, p. 92; underlining added)

Luce and Raiffa's (1957, p. 92) "monologue" regarded pure strategies, but playing a randomized strategy would be no better. The game in Figure 2a has one Nash equilibrium in mixed strategies.[16] If Player 1 thought of going for that equilibrium, and if Player 2 thought he would do so, it would be better for 2 to react to it by playing a pure strategy (β_2), without randomization—and consequently, it would again make sense for 1 to go back to a pure strategy (α_2). The Battle example served *Games and Decisions* to show how much more complex non-zero-sum games are; equilibria in them did not satisfy a list of four properties equilibria in zero-sum games have, which made them work flawlessly.[17] The criticism using the Dilemma was rather different. As it is usual, what is striking in the Dilemma is how its unique equilibrium is not Pareto optimal: both prisoners could do better if they cooperated with each other, but since defecting is a dominating strategy, they never do what is best for them (in a group perspective). These two examples, the Battle and the Dilemma, could be "the death knell for the equilibrium concept as the principal ingredient of a theory of non-cooperative non-zero-sum games"

[16] In such an equilibrium, Player 1 chooses α_1 and α_2 with probabilities 0.6 and 0.4, respectively. In turn, Player 2 chooses β_1 and β_2 with probabilities 0.4 and 0.6.

[17] These four properties of two-person zero-sum games are: it never pays off to inform your rival what your strategy is; it is never beneficial to communicate an attempt to coordinate a joint plan of action; no equilibrium is better from another, in any player's perspective, and mixing strategies from different equilibria still form a equilibrium; and minimax and equilibrium points always coincide.

if game theory should be a "realistic theory" in Luce and Raiffa's (1957, p. 104) opinion. These examples would reemerge in a different guise decades later.

Instead of serving as tools for criticizing Nash's (1951) approach, the Battle and the Dilemma later became devices for describing features of non-cooperative game theory—without appraising it, either positively or negatively. Aumann (1976, p. 19) thought that in the Battle "the mixed strategy equilibrium is the natural outcome" precisely because such a game represents a situation in which players fail to bargain. That is, in realizing outthinking one another would lead players nowhere, they would simply accept that playing a randomized strategy would be better. Also, Aumann (1976, p. 22) used the Dilemma to explain why game theory does not always assure social optima: "According to the logic of game theory, though $(4, 4)$ [the Pareto optimal outcome; outcome $(0.9, 0.9)$ in Figure 2b] may be a 'good' outcome, it is not self-eforcing and so in a sense not 'stable.'" That is, there was nothing reprimandable in what Nash's (1951) theory implied for the Dilemma; players would act in an inefficient equilibrium because the Dilemma lacks a self-enforcing mechanism of cooperation. The Dilemma's problem was its game rules (and so it does not mean Nash's (1951) equilibrium was questionable because of it).

The changes in the now iconic examples of reflect broader movements within game theory. Around 1957, Nash's (1951) approach was one possible answer to dissatisfactions researchers had with von Neumann and Morgenstern's ([1944] 2007) theory for larger than two-person games. Just as a theory allowing every conceivable coalition to form was exaggerated, so was Nash's (1951) assumption prohibiting any coalitions to emerge. This context explains how Luce and Raiffa (1957) approached the Battle and the Dilemma, using them to show they were not buying Nash's (1951) approach. Fast-forwarding more than ten years, game theory was at a different stage. Even if von Neumann and Morgenstern's ([1944] 2007) way was troublesome, it became a dominant way of working on n-person problems; and with Lucas's (1967, 1968) counter-example, such a dominance came to an end. This blow possibly motivated authors and lecturers to look at alternatives under a different light, what appears on teaching materials of 1976-1977: they had no preferred way of studying n-person games, and so they presented the Battle and the Dilemma not as reasons to discredit Nash's (1951) approach, but simply as means of describing its characteristics.

Even if details of presentations were changing, such as with the Battle and the Dilemma, it is important to emphasize that, in general, "pure" textbooks did not change substantially since 1968–1970, in comparison with Owen's (1968) and Rapoport's (1970) books. In looking for a "genealogy" of modern game theory textbooks (in particular, those which economists use), it does not make much sense to keep investigating more recent descendants of Owen's (1968) and Rapoport's (1970) works. To grasp how the

"Nashfication" of game theory—and of game theory textbooks—happened, it is more profitable to turn to industrial organization, as Tables 6 and 7 suggested.

4.2 Another "New" Industrial Organization

Around 1972–1978, Scherer's (1970) textbook was becoming a staple in graduate industrial organization courses.[18] In nearly all graduate courses in industrial organization Tower (1981) collected, Scherer's (1970) *Industrial Market Structure and Economic Performance* appeared as a main text.[19] The field was under rapid change, incorporating data in its analysis. One of its reviewers, Dewey (1972, p. 1451), explained that "in the early 1950's we had so little statistical data on industrial concentration in the United States that 10 days or so of steady reading in a good library would have sufficed to make any economist an expert on the subject." Things were shifting with what he called a "data revolution." Scherer's (1970) textbook covered a "new" industrial organization in what concerns its relationship with empirical knowledge, and possibly such a feature explains its popularity. Game theory was present in it, but played only a small supporting role in very specific models of oligopoly pricing. Games fully hit *Industrial Market Structure and Economic Performance* only in its third edition, of 1990, and it was not by Scherer's hands; that edition included an additional author, David Ross (from Williams College), who brought to it a little more of game theory—but still it was far from a dominating technique. This characterization—of incorporating data, but not much of games—was not a particularity of Scherer's (1970) work; it was conspicuous of industrial organization in general, as Grether (1970, pp. 85-86) portrayed in his survey of that field. Grether (1970, pp. 85-86) "consulted with twenty-one persons active in the field of industrial organization" to question what weak points existed in industrial organization; among other answers, respondents raised "the lack of use of refined game theoretical models." Change was, however, under way in *another* "new" industrial organization.

In a survey for the *American Economic Review*, Shubik (1975a, p. 280) argued there was a single "skeleton in every economic theorist's closet:" oligopoly theory. He stated every course on microeconomics would cover problems in oligopoly theory for a few hours and their "classical 'solutions.'" Afterward, professors would drop oligopoly theory favoring more well-established segments of microeconomic theory. This circumstance was a consequence of economists not having "a generally accepted adequate theory of oligopoly," Shubik (1975a, p. 280) continued. The area of oligopoly

[18] F. M. Scherer was a Harvard University trained economist, earning his Ph.D. in 1963. Before publishing his textbook on industrial organization, he lectured at Princeton University (1963–1966) and the University of Michigan (1966–1972). His principal research interests has always concerned industrial organization and technological change.

[19] There is also a collection of syllabi in industrial organization published in *American Economist*, which also puts Scherer's book in prominence (see the bibliographic reference for Grether (1975)).

theory had always been populated by mathematically-inclined authors, and Shubik (1975a, p. 280) mentioned its central characters: while authors such as "A. A. Cournot, J. Bertrand, F. Y. Edgeworth, H. Hotelling, T. Bowley and others" cultivated a first generation of models, modern developers included "R. J. Aumann, R. Selten, Shubik, B. Shitovitz, L. Telser, and others." These new theorists have a known background in game theory—except by Telser, an economist of the University of Chicago.[20,21] Telser (1968, p. 315) was a book reviewer of Kuenne's (1966) essay collection in honor of E. H. Chamberlin, a classic Harvard author in monopolistic competition, and he provided a list of key names just as Shubik (1975a, p. 280) did;[22] for him, "Fellner, Georgescu-Roegen, Samuelson, Bain, Heflebower, and H. Johnson" provided traditional readings, whereas:[23]

> [. . .] Students of competition find the [classical] theory of monopolistic competition neither a useful guide nor a source of inspiration for a testable hypothesis. In this area of research, the useful theory is due to Cournot leading to equilibrium points by Nash; to Edgeworth leading to the core and closer study of competition by Shubik, Debreu, Scarf, and Aumann; and to the major contribution in the general area so far in this century which has inspired these new developments, the game theory of von Neumann and Morgenstern.
>
> The theory of competition enjoys a renaissance that owes nothing to the theory of monopolistic competition and much to the core and the equilibrium point, concepts whose full implications are still being developed and which promise healthy progeny from the marriage of game theory to classical economics. (TELSER, 1968, pp. 314-315; underlining added)

This "renaissance" involved two approaches to game theory: while one connected Cournot's (1938) work and non-cooperative games, other united Edgeworth's (1881) and cooperative games. Textbooks of industrial organization of 1972–1978 started to incorporate both ways. Doing so was not a dominant approach—Scherer's (1970) text was more popular than any other—, but such books of 1972–1978 function as parents of Tirole's (1988) classic. In such a sense, two textbook authors are prominent here: Telser, whose course at Chicago figures in Tower's (1981, pp. 167-168) collection of syllabi of industrial organization, which stands out for its emphasis on games (using Luce

[20] Benyamin Shitovitz is a less known name in game theory among economists. Aumann advised him during his Ph.D. at the Hebrew University of Jerusalem, completed in 1974. One of Aumann's (1964) many papers in games concerned market models with a continuum of "small" traders (having many players, such markets would be competitive). Shitovitz (1973, p. 467) followed his advisor's lead, studying markets having a continuum of small players, but also having a few "large" ones.

[21] L. G. Telser is a main character here—more information on him follows below.

[22] For a historical reconstruction of Chamberlin's ([1933] 1969) *magnus opus*, his *The Theory of Monopolistic Competition: A Re-orientation of the Theory of Value*, see Reinwald (1977). Also noteworthy is Aslanbeigui and Oakes's (2011) discussion about its reception.

[23] This book under review was *Monopolistic Competition Theory: Studies in Impact*, which had as its subtitle *Essays in Honor of Edward H. Chamberlin*. Edward H. Chamberlin passed away in 1967. Chamberlin ([1933] 1969) published an influential book, *The Theory of Monopolistic Competition*, which professors of industrial organization of the early 1980s still frequently mentioned (check Tower's (1981) collection, which documents a list o syllabi of industrial organization from courses dating of 1979–1981).

and Raiffa's (1957) as a main text, alongside textbooks of Telser himself), and James W. Friedman (1968, p. 257) who, apparently, was a frontrunner in using Nash's (1951) work in duopoly and oligopoly models.[24] Looking at Tables 6 and 7, one sees Telser published two texts, *Competition, Collusion, and Game Theory* and *Economic Theory and the Core*, whereas Friedman published one, *Oligopoly and the Theory of Games*.

Both Friedman and Telser are not well-known, even among historians of economics, and it is not crystal clear how they got into game theory. Telser received his Ph.D. training at the University of Chicago, earning his title in 1956 (Milton Friedman was his advisor). After a brief period at the Iowa State University, Telser assumed a position at his alma matter in 1958, where he remained until his retirement. His research seemingly moved from commodity markets to advertising, and then to oligopoly theory.[25] His shift toward game theory was specially unusual given his affiliation, as there was no research tradition of game theory at the Chicago University (regardless of it hosting the Cowles Commission during 1939–1955). Telser (1972, p. ix) vised the Cowles Foundation for Research in Economics at Yale University during 1964–1965, which was instrumental for him to learn "the theory of the core," he said in his textbook.[26] Much of what he presented in it came from a course named "Theories of Competition," which he offered five years before he published his textbook. Friedman, who obtained hid Ph.D. from Yale in 1963, was another economist interested in game theory and oligopolies at Cowles during Telser's visit;[27] in fact, a Cowles Research Report says: "James Friedman extended the noncooperative approach to the theory of oligopoly in studies initiated at the Cowles Foundation and completed while on a year's leave at the University of California at Berkeley" (Cowles Foundation for Research in Economics, 1968, p. 7).

During 1962–1964, Friedman worked on oligopoly experiments (Cowles Foundation for Research in Economics, 1964, 32). Friedman (1967, pp. 379-380) argued "At least from the time of Adam Smith, economists may be found who believe that oligopolists will collude rather than behave competitive." While A. A. Cournot and K. Wicksell thought of collusion when studying duopolies, things would only start to get into proper shape after the *Theory of Games* appeared, he continued. Friedman's (1967, pp. 380-381) goal was to "test the conjectures and theories" he alluded to in surveying what economists thought of duopolies in an experimental setting where agents played a game

[24] All mentions hereafter to "Friedman" refer to James W. Friedman, not Milton Friedman.

[25] Paper titles illustrate that. Telser named his first two papers as "The Support Program and the Stability of Cotton Prices" and "Futures Trading and the Storage of Cotton and Wheat" published at the *Journal of Farm Economics* and the *Journal of Political Economy*, respectively, in 1957 and 1958. The subject of his research changed by 1961, when he published "How Much Does It Pay Whom to Advertise?" in the *American Economic Review*.

[26] Telser (1959, 296) said he was already aware of game theory before visiting Cowles, but it was not his tool of choice for his early research.

[27] From 1968 through 1983, Friedman remained at University of Rochester, and in 1985 he joined the University of North Carolina at Chapel Hill, where he stayed until his retirement.

of complete information and could exchange messages—it was an "explicitly cooperative" game. Then, Friedman's articles of 1971–1976 showed a turn to non-cooperative games. For example, he titled some of his papers as: "A Non-cooperative Equilibrium for Supergames," "A Noncooperative View of Oligopoly," "Non-Cooperative Equilibria in Time-Dependent Supergames," and "Reaction Functions as Nash Equilibria"—what clearly points toward Nash's (1951) framework.[28]

This short biographical sketch of Telser and Friedman indicates that something happened at Cowles during their visits, which turned a switch in their research, as they started to look into game theory. Cowles' "Report of Research Activities" always include a brief, thematic survey of what happened there. The report from July 1, 1964–June 30, 1967, had a section about "Competitive Equilibrium, Oligopoly and the Theory of Games." Here, "Competitive Equilibrium" had a connection with game theory: H. Scarf worked on a algorithm for calculating equilibrium price vector by "taking as his point of departure work by C. E. Lemke, in conjunction with J. T. Howson, on the calculation of a Nash-Cournot equilibrium for a two-person non-zero-sum game" (Cowles Foundation for Research in Economics, 1968, p. 4). This report section also described Friedman's work (but not Telser's). In an interview, Telser (2018, p. 118) said: "I did learn directly from Scarf and Aumann about game theory, things that I hadn't known before, and in particular, the most important topic that I got from what Scarf did is on the theory of the core." There is no similar information regarding Friedman. Apparently, Cowles was essential in connecting mathematicians who knew game theory and economists, although it is not clear how things happened there.

Regardless of how influential Cowles was for both Telser and Friedman, their research *and* textbooks have a critical difference. While Friedman's research emphasized a non-cooperative approach, Telser (1972, p. xiv) privileged what he called "core theory," an applied study of cooperative game theory.[29] Importantly, both authors covered both approaches, cooperative and non-cooperative. What varied were proportions. Their justification for using game theory stemmed from a common dissatisfaction of how economists had been addressing market problems. For Telser (1972, p. xiv), a satisfactory theory should define what "competition" means and deduce implications, instead of taking for granted "some of the intrinsic properties of markets and competition without

[28] This does nit mean that all of Friedman's research was on non-cooperative games. During 1971-1976, Friedman also published: "Duality Principles in the Theory of Cost and Production-Revisited," "A Non-cooperative Equilibrium for Supergames: A Correction," "Concavity of Production Functions and Non-Increasing Returns to Scale," "On Reaction Function Equilibria," and "Price Signaling in Experimental Oligopoly." That is, not all his research was about non-cooperative games.

[29] There is not many mentions to "core theory" in published papers preceding Telser's (1972, 1978) and Friedman's (1977) textbooks, and none of them—papers and textbooks—actually explained what "core theory" means. As Telser (1978) shows, core theory applies to a manifold of applications, so a model's subject plays no role in categorizing defining "core theory." Importantly, core theory is not just about market problems.

properly understanding them." For example, assuming that no agent alone can influence prices was a flawed approach—this hypothesis should emerge as a result instead. In turn, Friedman (1977, p. 3) characterized competitive and monopolist markets by "an absence of *strategic elements*"—finding optimal decisions for each participant involves solving "straightforward maximization" problems; in opposition, "*strategic interdependence* is the source of what is called the oligopoly problem." The key point is that oligopoly models are about "fewness," as Fellner (1949, p. v) pointed in his classic treatise: "a few decision-making units shape their policies in view of how they mutually react to each other's moves." Friedman (1977, p. 3) endorsed Fellner's (1949) view, and argued that while it seemingly implied "an endless vicious circle" of each agent thinking about what others will do, "it need not be:" game theory provided a way out; and, because of it, game theory was finally entering economics.

4.2.1 Flashback: M. Shubik's Strategy and Market Structure

In perspective, it would be hard to say some past text (of 1972 backward) was a "parent textbook" for what Telser (1972, 1978) and Friedman (1977) offered. The only predecessor having some resemblance with such textbooks of 1972–1977 is M. Shubik's *Strategy and Market Structure: Competition, Oligopoly, and the Theory of Games*, of 1959. Looking back at game theory books of 1950–1959 (see Table 3), *Strategy and Market Structure* stands out because it was a one-of-a-kind book: no other volume of 1950–1959 was exclusively focussed in applying game theory to economics, following the *Theory of Games'* lead. This book received a significant amount of reviews, getting appraisals in journals as the *American Economic Review* and *Econometrica*, among others in different disciplines. The reviews from economists were not positive, however, and its influence would be non-existent: as Giocoli (2003, p. 345) put it, it was a "fruitless attempt" in making game theory work in economics.[30]

Shubik studied under O. Morgenstern in Princeton University, getting his Ph.D. in 1953, but game theory grabbed his interests before, while he was still a Master student of Political Economy in the University of Toronto.[31] In an interview he gave to INFORMS (Institute for Operations Research and the Management Sciences), Shubik (2017, p. 6) described he had to write a book review for his econometrics class: "And the econometrician said, go to the library, pick out a new book roughly related to econometrics, and write a review of it. I went to the library. I picked up the *Theory*

[30] More specifically, Giocoli (2003, pp. 365-366) argued Shubik's (1959b) transformation of the "Cournot equilibrium into a static fixed-point solution actually constituted a perfect own-goal from the viewpoint of promoting game theory"—such an approach was unfit vis-à-vis what most economists did at that time.

[31] Shubik did not receive much attention from historians of economics so far. Some mentions to Shubik's influence appears in Giocoli's (2003, pp. 332-33, 345) and Erickson's (2015, pp. 244-248) works. However, no account of his contributions as a main character exists.

of Games and Economic Behavior." This event ultimately led him into Princeton and, while he was there, he wrote *Strategy and Market Structure*. His book is a predecessor of the textbooks of 1972-1978 because it integrated game theory into economics. This combination was not present in the earlier books, which discussed game theory as a subject in itself, only eventually suggesting a possible application of it (but not necessarily in economics). The texts of 1972-1978 were different: they present a collection of models using game theory. This basic organization resembles what Shubik (1959b) did. But a quick glance at *Strategy and Market Structure*'s shows Shubik's (1959b) work was not a simple collection of models: he sought a general theory, which would bring coherence industrial organization. The keyword here is "unification."

Shubik (1959b, p. xi) divided his book into two parts. Part I, "The Background to Competition," explored known models and approaches in industrial organization to show how valuable game theory could be; Part II, "The Dynamics of Oligopoly: Mathematical Institutional Economics," started a new dynamic theory. Naturally, in putting game theory to work for duopoly and oligopoly models, Shubik (1959b, p. 18) had to choose between going with von Neumann and Morgenstern's ([1944] 2007) cooperative or Nash's (1951) non-cooperative approaches; he not only used both ways, but also remarked his theory for dynamic models was a third path. In particular, Shubik (1959b, p. 21) explained his theory was "neither co-operative nor non-co-operative, in the sense previously used, but are semi-co-operative to greater or lesser degrees, depending upon technological features, information conditions, and/or the degree of organization present between competitors." *Strategy and Market Structure* was not precisely a success among economists—its book reviews showed mixed feelings, and it did not stimulate further research using game theory.[32] While *Strategy and Market Structure* firstly attempted to make industrial organization more receptive toward game theory, it produced little to no effect and, as Eichner (1978, p. 1020) recognized, between Shubik's (1959a) work and textbooks of 1972-1978, no significant attempts in making industrial organization game theoretical existed. Apparently, even if Shubik's (1959b) work is related to Telser's (1972, 1978) and Friedman's (1977), it is not precisely useful in understanding them.

4.2.2 Cooperative Industrial Organization

While Shubik (1959b) considered cooperative *and* non-cooperative approaches in his study of industrial organization, from a modern perspective, to speak of a "cooperative industrial organization" might sound exotic. Interestingly, such a "cooperative

[32] For instance, while Harsanyi (1961, p. 268) thought *Strategy and Market Structure* was "a valuable pioneering contribution to the economic applications of game theory," he criticized its approach to dynamic problems for not providing determinate, testable propositions, and for relying on *ad hoc* assumptions.

industrial organization" came up in textbooks simultaneously to the more familiar "non-cooperative industrial organization," in a period in which Nash's (1951) contribution would start to penetrate economics. In such a context, it is natural to ask how both approaches related to one another (if they did), and how did economists react to that "cooperative industrial organization," especially considering that it somehow fell into oblivion. Here, it is important to remember that cooperative game theory admits many alternative solutions (as the textbooks of Owen (1968) and Rapoport (1970) showed). In the textbooks of 1972-1978, only two solution concepts of cooperative game theory received attention—and none of them was von Neumann and Morgenstern's ([1944] 2007) stable sets: there was a *value approach*, steering from value functions, and that of *core theory*, based on Gillies and Shapley's core. Among both, just one figured into textbook presentations of industrial organization.

Friedman (1977) covered value-based models in economics, what included Nash's (1950a) bargaining model, Shapley's (1953b) value, and Harsanyi's (1963) bargaining model (which generalizes Nash's (1950a) and Shapley's (1953b) contributions). His presentation of such models was more abstract, however, what contrasts with his textbook as a whole. Most of his chapters concerned non-cooperative games, and they also brought up some applications. However, when it came down to cooperative approaches, Friedman (1977) offered no uses for value functions and Gillies and Shapley's core. In itself, such a fact suggests that value functions were not useable in industrial organization. The situation was different with Gillies and Shapley's core: even if Friedman (1977) did not apply it anywhere, Telser (1972, 1978) did, and in a wide array of models. For illustration, Telser's (1978) second textbook reads as a storefront of core theory, showing how useful it could be to a series of problems, including: externalities, public goods, natural monopolies, "Viner Industries" (a model for an industry of identical firms of a particular kind of cost functions), storage and inventories (models of exchange where traders can withhold their products), competitive markets, and more.

This list of applications of core theory included industrial organization, which Telser (1972) handled in his first textbook. His text was deeply connected with his research agenda, and Telser (1972, pp. xiii-xiv) advocated for a reappraisal of competitive markets; he felt economists took "for granted some of the intrinsic properties of markets and competition without properly understanding them." For example, some of such "intrinsic properties" include saying individuals are powerless in influencing prices, and that prices equal marginal costs. *Competition, Collusion, and Game Theory* proposed to deduce such results through a cooperative game. The model it forwarded was relatively simple, only assuming people have stocks of goods and preferences over what trades they could make. To solve it, it would be necessary to specify a characteristic function and study its underlying core. This set up allowed Telser (1972, p. xv) to tackle several questions. For instance, in studying a model analogous to a simple supply and demand partial equilibrium model, he explained:

> In the first two chapters I focus on a single market where one good is exchanged for money. This model is the analogue of partial equilibrium analysis according to Marshall. Consequently, we can learn much about the structure and performance of markets. Some of the questions considered are as follows: When does a competitive equilibrium exist? This is equivalent to finding when a market has a nonempty core. When is there a set of trades capable of implementing the core constraints? When will there be a common price per unit in the market? Under what conditions will there be Pareto optimality? How do transactions costs affect the equilibrium? What is the role of brokers in a market? How do changes in the number of traders affect the equilibrium? How efficient is random contact among the traders? Can there be an equilibrium if trade is confined to coalitions consisting of pairs of traders? [. . .] (TELSER, 1972, p. xv)

But it was not all: *Competition, Collusion, and Game Theory* also investigated oligopoly theory using that basic framework. In doing so, Telser (1972) was pushing further both game theory *per se* and economics. To name two examples, regarding game theory, Telser (1972, pp. 45-46) criticized the *Theory of Games* attempt to deduce group rationality (using a fictitious players), seeking to obtain it as "a consequence of the core constraints." Concerning economics and industrial organization, he discussed competition *versus* collusion in a sequence of four chapters, addressing questions of why do sellers not always collude, and how do they share profits, among others. In perspective, *Competition, Collusion, and Game Theory* reads as a genuine *tour de force* centered towards making Gillies and Shapley's core useful in industrial organization. This making of a "cooperative industrial organization" is notably different from what Friedman (1977) did with Gillies and Shapley's core.

Friedman's (1977) *Oligopoly and the Theory of Games* two final chapters covered, respectively, what he called "the value approach" and Gillies and Shapley's core—both seen as possible solution concepts for cooperative games, written in characteristic function form. Unlike Telser (1972), Friedman (1977) did not apply such approaches to solve industrial organization models; his presentation was "pure," without reference to economics. But in closing his book, he provided a reflection about such approaches. Friedman (1977, pp. 288-289) argued cooperative game-theoretical approaches were "far more satisfactory" than "the sort of thing which has pervaded oligopoly theory," but they were still not sufficiently developed for oligopoly theory. He exemplified his position by pointing that in a perfectly symmetric industry, core theory would reasonably suggest that firms should collude and split their joint profits. But matters were not so clear in unsymmetrical industries; in such a case, a symmetric payoff distribution would hardly be accepted, and questions of "what side payments are made?" and "what is the final settlement?" would be left unanswered: Gillies and Shapley's core was simply unsuited for responding them.

The contrast between Telser's (1972) and Friedman's (1977) expositions suggest that "core theory" was an unstable subject in the period of 1972–1978. While Gillies and Shapley's core had a figured out place within mathematical game theory, it was still unclear how it could be useful in industrial organization. In reading Friedman's (1977) text of game theory, one would hint that cores were far less fruitful than Nash's (1951) non-cooperative setup and equilibrium, since Friedman (1977) used only them in applying game theory to competition issues. Reading Telser's (1972, 1978) expositions would bring up a different interpretation: it would appear that Gillies and Shapley's core was so powerful as to serve nearly any subarea of economics. This is not surprising: by comparing books in Tables 5, 6, and 7, it seems game theory was just starting to find applications outside of mathematics. Using core theory in industrial organization, in particular, was a brand new approach. While in hindsight it is clear non-cooperative eventually championed over cooperative games, it was not so around 1972–1978. Core theory could have become a dominant approach. To a certain extent, capturing reactions to Telser's (1972, 1978) and Friedman's (1977) textbooks indicates how economists felt about both approaches around 1972–1978. In particular, responses to Telser's (1972, 1978) work assist in characterizing how his "cooperative industrial organization" did not thrive.

The reviews Telser's (1978) textbook receive were particularly assertive in appraising core theory and its value for economics. As Trockel (1980, p. 251) pointed, Telser (1978) wrote a book applying Gillies and Shapley's core to several subareas of economics, making it seem like an "almost omnipotent tool in economic theory." This indiscriminate use arouse feelings that Telser's (1978) textbook made Gillies and Shapley's core appear to achieve more than it actually did. Truly, such solution concept was fundamental in general equilibrium theory—a subarea Telser (1972, 1978) did not discuss—, but such a standing was not true for most other themes in economics. Perhaps what made Telser (1978) use it in assorted models was a characteristic Gale (1980, p. 203) emphasized: Gillies and Shapley's core assumes players are able to communicate and make commitments without specifying how they do so; that is, it is "an attractive concept for the theorist who is unwilling or unable to specify the structure of a game completely." However attractive, such trait naturally brings with it (at least) two disadvantages. First, cores are usually too large, resulting in indeterminate answers concerning what allocations would actually emerge in each model. Second—and much more seriously—, as Gale (1980, p. 203) put it, "Anyone *assuming* that the core is the appropriate solution concept for some economic model should at least direct his attention to these problems and try to justify the assumption." This point is also present in Dixit's (1979, p. 990) review. Importantly here, Telser (1978, pp. 3-4) did not do that; he only said Gillies and

Shapley's core embedded a notion of competition without explaining why it would fit well in his models.[33]

Yet, Telser's (1978) use of cores had a third problem. Related to that lack of discussion of why should anyone use Gillies and Shapley's core as a solution for all of Telser's (1978) models lies yet another problem: cores might be empty. To avoid disconcerting situations, Telser (1978) actively imposed artificial restrictions upon his models, so they would not face a dead end. Reviewers were not appreciative of it. For instance, Dixit (1979, p. 990) pointed such restrictions meant "complicated groupings of buyers and sellers" in oligopoly, "making the use of the core in oligopoly often difficult to accept." To add a second example, in discussing Telser's (1978) constraints over "the location problem," Gale (1980, p. 204) felt: "the emptiness of the core means either that it is not the appropriate way to describe competition or that the model is incompletely specified"—that is, Telser's (1978) approach was simply not convincing. These reactions to *Economic Theory and the Core* suggests core theory was not as successful in economics as reading Telser's (1978) book could suggest. Yet, it could be that it was satisfactory for industrial organization.

To appraise so, reviews of *Competition, Collusion, and Game Theory*, of 1972, are helpful. They indicate economists had mixed feelings for Telser's (1972) cooperative industrial organization. Nearly all reviewers thought Telser's (1972) attempt fell short at specific points. To mention a few examples, Clarke (1973, pp. 250-251) was not fully confident *Competition, Collusion, and Game Theory* obtained group rationality as an implication of "the core constraints" instead of plainly assuming it, as Telser (1972, p. 45) desired to do; Nagatani (1972, p. 573) also showed some doubts about Telser's (1972) discussion about cooperation *versus* competition in repeated games; and finally, while Phillips's (1973, p. 540) review was positive, he asserted "startling new results are notable by their absence" in Telser's (1972) first three chapters. While book reviews went for specific criticisms, in general they imprint an image of doubt: they were not sure Telser's (1972) approach was successful. Telser's (1972) was pushing boundaries, but it was not clear if his chosen direction would bloom someday. To make sense of how Telser's (1972) application of core theory in industrial organization faded away, other textbooks are useful.

Looking at subsequent textbooks (of 1978–1987), it appears cooperative games more broadly did not manage to find a sufficiently persuasive application in economics. In opposition, Nash's (1951) approached not only thrived in industrial organization, but it also won its place due to other insights it provided (for example, through the Prisoners' Dilemma). Later textbooks, such as Shubik's (1982) *Game Theory in the Social Sciences:*

[33] This interpretation of Telser (1978) meant, simply put, that agents sought what was best for them. See Dixit's (1979, p. 989) explanation of it, for instance.

Concepts and Solutions, Ichiishi's (1983) *Game Theory for Economic Analysis*, and Friedman's (1986) *Game Theory with Applications to Economics*, brought up both cooperative and non-cooperative game theory. And while new examples in non-cooperative games popped here and there, the quintessential example of application of Gillies and Shapley's core in economics remained being Shubik's (1959a) "Edgeworth Market Game." This reads as if Telser's (1972) attempt of merging industrial organization and cooperative games led nowhere; none of his points made it through other textbooks.

4.2.3 Non-Cooperative Industrial Organization

While Telser's (1972) textbook pushed forward a merge of cooperative games and industrial organization, another mixing attempt emphasized Nash's (1951) framework. This is unsurprising to some extent, as Tirole (1988, p. xi)—who did a non-cooperative kind of industrial organization—recollected that "Theoretical industrial organization has made substantial progress since the early 1970s, and has become a central element of the culture of microeconomics." Indeed, when Telser (1972) and Friedman (1977) published their textbooks, papers applying non-cooperative game theory to oligopoly were already appearing in major journals of economics. A quick search shows up a considerable amount of work in the *Economic Journal*, the *Review of Economic Studies*, the *American Economic Review*, *Economica*, *Econometrica*, and the *Journal of Political Economy*. By 1970, almost 22% of papers having "duopoly" or "oligopoly" as keywords would also mention Nash (and 39.1% would name Cournot).[34] These figures would rapidly increase later on. While non-cooperative game theory becoming more frequently present in published papers, not everyone felt its results were extraordinary. Schotter and Schwödiauer (1980) composed a survey of game theory's effects over economics; insofar they discussed a bunch of subareas of economics, their evaluation for oligopoly theory was lukewarm at best:[35]

> [. . .] game theory is not doing much more than generalizing the results obtained already by A. A. Cournot (1838), Joseph Bertrand (1883), Wilhelm Launhardt (1885), Edgeworth (1897), Harold Hotelling (1929), Edward Chamberlin (1933), and Heinrich von Stackelberg (1934). For instance, the solution proposed by Launhardt, Hotelling, and Chamberlin for the price setting problem in differentiated oligopolies is equivalent to a Nash noncooperative equilibrium in pure strategies of a game given in normal form (Shubik, 1959a; Wilhelm Krelle, 1961; 1976; Telser, 1972; Friedman, 1977). (SCHOTTER; SCHWöDIAUER, 1980, 512; underlining added)

[34] This point relies on data from JSTOR and Portico's Constellate tool.

[35] Schotter and Schwödiauer (1980, p. 512) mentioned Wilhelm Krelle, a German economist. Most likely, his work was not influential in the United States, since mostly of his writings are in German (and most of his English writings are book reviews).

The comment above, beyond pointing Telser's (1972) and Friedman's (1977) textbooks as references in industrial organization, puts in check how relevant game theory was for industrial organization. Naturally, it concerns an opinion of just two economists, and which might not be representative of what most economists thought. Still, it does raise a questioning. The relationship between Cournot's (1938) model and Nash's (1951) was around for a long time already: for instance, Shubik (1959b, p. 65) had already emphasized it, almost twenty years before Friedman (1977) published his text. Thus, for a non-cooperative, game theoretical textbook discussion of duopolies and oligopolies to be distinguished, it would need something extra. To a good degree, Friedman's (1977) textbook covered charted ground (for instance, how Cournot's (1938) and Nash's (1951) solutions were equal). But is also brought up something that caught up its book reviewers attention:

> As the title indicates, the main topic of this book is oligopoly and the theory of games; but it is as a book on formal oligopolistic models that it is of interest. <u>For the study of these one needs little more in the way of game-theoretic concepts than the basic notion of a Nash equilibrium with which Cournot was already working in 1838</u>. [. . .]
>
> The author remarks, and an informal survey would seem to confirm this, that <u>very many economists seem to believe that oligopolists behave like (single-period) Cournot oligopolists or else they collude. But this is a rather simpleminded view</u>. As the author points out, outright collusion is often very difficult to organize (being, for example, supposedly illegal in the United States), while, on the other hand, the scope for reasonable behaviour when the opportunities for collusion are restricted is very much wider than that described by Cournot. The author illustrates this point by analysing a sequence of formal models, and it is when he is expounding and enlarging upon his own and related research work in this line that the book is at its most interesting (Chapters 8 and 9). [. . .] (BINMORE, 1978, pp. 102-103; underlining added)

Friedman's (1977) book discussed competition *versus* collusion, much as Telser (1972) did. This was a fundamental question in industrial organization. Just as Binmore (1978, pp. 102-103) said, Friedman (1977, p. 11) contended that "many economists seem to believe that oligopolists behave like (single-period) Cournot oligopolists or they collude." That is, normally economists would *assume* players behave either by colluding or by playing according to Cournot's (1938) reasoning, possibly using *ad hoc* justifications for considering one case or another. Departing from such dichotomy was part of Friedman's research agenda. This is seen, for instance, in Friedman's (1971a, p. 1) work on "supergames" (also called "repeated games"), in which he showed "the usual notions of 'threat' which are found in the literature of game theory make no sense in non-cooperative supergames." This short quote rightfully strikes a familiar note for those acquainted with Selten's (1965a, 1965b) sub-game perfect equilibrium concept, and it is what Binmore (1978, p. 103) found "most interesting" in Friedman's (1977) textbook.

What made his textbook distinguished was Friedman's (1971a) work on supergames, because it went beyond what economists already new about non-cooperative game theory and Cournot's (1938) model. In particular, his paper stands out because it included a formalization of the Folk Theorem: a result which game theorists knew since "the late 1950s," but whose authorship was "obscure," Aumann (1987, p. 16) remarked in his *Palgrave* entry. Possibly, Friedman's (1971a) formalization was the first to appear in a published article. In that work, Friedman (1971a, p. 11) discussed an infinite-horizon, dynamic non-cooperative game, for which "A promising area of application" would be "the theory of oligopoly." This is so because, using it, one could discuss whether in a given industry firms would collude or not (what depended, among others, in how they discounted their payoff streams). That, "competition *versus* collusion" would be something one deduces instead of something one assumes. While Friedman's (1971a) paper was about game theory in general, and while the Folk Theorem would be no novelty for a mathematician, it is important to emphasize that Friedman (1971a, 1977) was not precisely following any trends in game theory when he brought it to industrial organization.

Apart from including the Folk Theorem, Friedman's (1977) book distinguishes itself for considering extensive form-games; until then, that form was out of scope for game theory textbooks: they mostly provided short introductions to them, and solved them in different ways. Their focus was on strategic-form two-person games and characteristic function-form n-person (cooperative) games. In such a context, it should be no surprise Nash's (1951) contribution remained tied with two-person games—it simply does not apply for characteristic function form games. And when textbooks mentioned extensive form games, they did not bring up Nash's (1951) equilibrium as a solution approach. For example, Owen (1968, p. 101) simply suggested "It is advisable to solve these games by working backward," without connecting such an approach with Nash's (1951) framework. And perhaps more curiously, Rapoport (1966, pp. 49-53) suggested a sort of minimax reasoning: thinking in forward induction, players should pick moves defensively, choosing what would guarantee them a higher minimum payoff. In light of such, Friedman (1977) was doing more than simply applying Nash's (1951) equilibrium in industrial organization; he gave it a newer image, which went beyond that of being just something equivalent to Cournot's (1938) solution in oligopoly theory, or being just a Minimax equivalent in two-person zero-sum games.

In reading Friedman's (1977) textbook, it could appear it promoted a "Nashfication" of economics, but saying so would be speculative at best. What is remarkably clear is that no other textbook emphasized non-cooperative games as Friedman (1977) did, nor did other textbook bring up Nash's (1951) contribution together with other elements which are deer to non-cooperative games—extensive form games, refinements of Nash equilibria, and the Folk Theorem. Putting Friedman's (1977) work in genealogical perspective, it seems to be a close relative to Tirole's (1988) well-known book. And even before Tirole (1988) published his book on industrial organization, Friedman (1983, 1986) would publish two other books, *Oligopoly Theory* and *Game Theory with Applications to Economics*. This last book was even more similar to modern textbooks, as it brought together a plethora of refinements for Nash's (1951) equilibrium for extensive-form games, as if Friedman (1986) went deeper with his project of 1977. In any case, Nash's (1951) had then started to pervade game theory-oriented economics textbooks.

4.3 Brief Concluding Remarks

Looking at textbooks in perspective, there is a fundamental difference between those published until 1971 and those published afterward. Until 1971, textbooks were agnostic: they discussed game theory in itself, rarely making applications of it in problems of other disciplines. Around 1968–1970, W. F. Lucas's example of a 10-person game having no von Neumann and Morgenstern solution acted as a reorganizing force: until then, researches privileged stable set solutions in spite of their pitfalls, but once they realized that stable sets do not exist for all games, they had to rethink their research priorities. There is a lack of textbooks to appraise how they resolved their issues, but lecture notes (and a lecture notes-based textbook) indicate game theorists reweighed how much value they put in existence, and also that they looked at alternatives—including J. F. Nash's non-cooperative approach—with changed eyes.

From 1972 onward, agnostic textbooks would appear less frequently; and even when a new book of such type appeared, it would closely resemble past texts. What characterize most textbooks of that later period is that they applied game theory. The hall of covered disciplines was large, and it included economics. In particular, game theory and economics textbooks of 1972–1978 regarded industrial organization. They are what modern textbooks have closest of "siblings." Being non-agnostic, textbooks arranged game theory in a different way, much guided by what is useful for economics. The family resemblance does not restrict itself with covering (or emphasizing) non-cooperative game theory; it also comprises a coverage extending itself to other objects of game theory which are dear to economists—extensive form games, refinements of Nash's equilibrium concept, and insightful examples, such as the Prisoners' Dilemma, to mention a few.

One characteristic of economics-oriented modern textbooks which might go unnoticed is that sometimes they present cooperative game theory. These presentations usually include many solution concepts, including Gillies and Shapley's core. Historically, agnostic textbooks presented von Neumann and Morgenstern's stable sets as "better" than Gillies and Shapley's core for one reason or another. However, when it came down to studying markets, Gillies and Shapley's core performed better, especially because of its “shrinking properties” in increasingly more populated markets. This advantage stimulated different subareas of economics, including general equilibrium theory, but what might come as a surprise is that it also fostered a cooperative type industrial organization, unlike anything one finds in modern expositions. L G. Telser researched that different industrial organization and brought it to a textbook presentation in his *Competition, Collusion, and Game Theory*. Although Telser tackled pressing questions for industrial organization, his approach eventually disappeared from textbooks.

Almost simultaneously, non-cooperative game theory also started to penetrate in industrial organization textbooks—majorly through J. W. Friedman’s hands. The relationship between Cournot’s model and Nash’s equilibrium was known for years, so explaining how non-cooperative game theory succeeded in industrial organization is not simply a matter of tracking when that mixing occurred. For non-cooperative games to thrive in industrial organization, more than Nash’s contribution would be necessary. In particular, Friedman’s *Oligopoly and the Theory of Games* used a different game theory from that one finds in agnostic textbooks: he emphasized repeated games, extensive-forms, and brought a first formalization for the Folk Theorem, what allowed him to put an end to a traditional deadlock of industrial organization, which forced economists to assume either that firms compete or that they collude. While it is hard to appraise how influential *Oligopoly and the Theory of Games* was at its time, Friedman surely enjoyed incentives to publish more textbooks—he wrote two more linking economics and game theory. The stage was set for the modern textbooks.

Part II

Textbook Accounts of Selected Concepts

5 The Nash Equilibrium: Cooperation Versus Non-Cooperation

The *Theory of Games and Economic Behavior*, of J. von Neumann and O. Morgenstern, not only founded game theory, but did so by arguing that economics needed to start anew on an axiomatic basis. While that book certainly caused a fuss for some time—some book reviewers bought its promises, whereas others thought game theory would have no use at all—, it took decades for its subject to effectively make its way into economics.[1] However, only a particular type of game theory became palatable for economists: J. F. Nash's non-cooperative games, which are strikingly different from what von Neumann and Morgenstern elaborated in their *Theory of Games*. Possibly, present-day economists would feel astonished in realizing that back then game theory was not about rational players picking strategies in their best interest, but about players choosing in which "teams" (coalitions) they wanted to play. But perhaps they would feel even more astonished if they knew such games, called cooperative, lingered around until very recently, even finding their way into economics more or less simultaneously to Nash's non-cooperative approach.

Existing narratives addressed some aspects of such a "Nashfication" which game theory underwent as economists incorporated it in their canon. Two are particularly remarkable. First, economics changed. The discipline which von Neumann and Morgenstern confronted back in 1944 had a particular disciplinary image, that may be read as incompatible with game theory as known today (put simply, a bag of applications of fixed-point theorems, often showing equilibria exist, but not describing how economic agents might get there). Second, Nash's framework also experienced a great deal of change between its inception around 1950–1951 and its much later entry into economics. These changes are of technical nature and concern J. Harsanyi's formulation of a tractable approach for incomplete information games and R. Selten's invention of a refinement of Nash's equilibrium concept, nowadays referred to as *sub-game perfect (Nash) equilibrium.*[2] Yet, another facet of such history merits attention as "game theory" includes far more

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[1] Giocoli (2009, p. 188) discussed three alternative explanation of how the Nash equilibrium rose in interest after being neglected for decades: "the beginning of the literature on the refinements of Nash equilibrium," "the reaction against Chicago antitrust theory and policy," and "the application of game-theoretic tools to mechanism design problems."

[2] Giocoli (2003) and Erickson (2015), respectively, addressed these two aspects. This dissertation does not include an account of Harsanyi's and Selten's contributions—while they certainly were pivotal in the "Nashfication" of economics, they are beyond reach here. This is so because textbooks up until the late 1970s did not discuss Bayesian games, nor did they focus on refinements of Nash's equilibrium concept. Both contributions would be more important to discuss textbooks from the 1980s onward, something which is not part of this text.

than Nash's framework. For decades researchers privileged studying von Neumann and Morgenstern's cooperative games, and changes in cooperative game theory reflected back on how practitioners viewed Nash's contributions. That is, a broader view of game theory say something about how the Nash equilibrium was "forgotten" to be "rediscovered" later.

Historians have many ways of writing histories, and this applies to the Nash equilibrium. A textbook-story is particularly interesting here. To begin with, game theory's history besides the "Nashfication" is largely unexplored, so it would be rather tough to contextualize what happened in game theory during more or less three full decades (from 1944 until the late-1970s). Textbooks provide a shortcut, even if some care is necessary in extracting contextual information out of them. But possibly more importantly, the Nash equilibrium which populates economists' minds today is not simply "the Nash equilibrium;" or, to put it differently, no object of economic theory stands on its own. Usually, one concept hinges on another, and such relationships are essential for economists to "make sense" of objects individually. For instance, one of such objects which is related to the Nash equilibrium is the Prisoner's Dilemma. The equilibrium says something about the Dilemma—how rational prisoners behave—, and the Dilemma says something about the equilibrium—individual interests not necessarily lead players to socially optimum outcomes. These relationships do not always come up in game theory papers (possibly because who normally write them are mathematically-inclined authors, so their work focus on technical aspects of game theory). Here, textbooks are useful: they have a story to tell about how mathematicians and economists "connected" the Nash equilibrium to other parts of game theory.

5.1 A Known Though Necessary Exegesis

The history behind Nash's acclaimed equilibrium point has been told and retold, by practitioners of game theory and historians of economic thought alike (for example, see Myerson (1999)—a practitioner—, Leonard (1994) and Giocoli (2004)—historians). Yet another visit to some of Nash's articles is appropriate, considering that past writers emphasized questions of how should someone interpret an equilibrium point. These writers have gone back to Nash's Ph.D. dissertation, and discussed how its ninth section, "Motivation and Interpretation," which contains two ways of interpreting his equilibrium concept, is missing in Nash's published record. While giving Nash's equilibrium an interpretation was a decisive step in making it palatable for economists, a textbook-based account of its history demands paying attention to two other aspects of Nash's papers, which have not received as much attention and might pass unnoticed: how equilibrium points relate to von Neumann's (1928) two-person zero-sum games and Nash's focus on strategic-form games.

Nash's (1950a) first published article is his well-known one-page long definition of equilibrium point and its existence theorem.[3] Put it in even fewer words than Nash (1950a), for any fixed player, an n-tuple would *counter* another if it provided him a higher expected gain. For Nash (1950a, p. 49), an equilibrium point was plainly a "self-countering n-tuple." Showing every game admits at least one equilibrium was a quickly solved matter, being needed only a mention to Kakutani's (1941) fixed point theorem. The 1950 paper was not burning any bridges: instead of antagonizing von Neumann and Morgenstern's ([1944] 2007) approach to games, Nash (1950a, p. 49) simply suggested it would be thinkable of defining some games in which "each player has a finite set of pure strategies and in which a definite set of payments to the n players corresponds to each n-tuple of pure strategies, one strategy being taken for each player." That is, he suggested it would be possible to connect his framework with that of the *Theory of Games* (for instance, check Giocoli's (2003, pp. 307-309) account). Defending non-cooperative games as a proper domain of game theory would only happen in his subsequent paper. But more importantly, Nash (1950a, p. 49) closed his paper affirming his equilibrium point and von Neumann and Morgenstern's ([1944] 2007) "main theorem"—the Minimax Theorem—were equivalent for two-person zero-sum games. That is, they offered identical mixed strategy profiles as solutions. While Nash (1950a) submitted his paper in November, 1949, in May, 1950, he defended his Ph.D. His dissertation contains much of what he would publish a year later, in his *Annals of Mathematics* paper of 1951.[4] This 1951 article was not about suggesting an alternative solution concept which could coincide with von Neumann and Morgenstern's ([1944] 2007) for a particular case; it was more ambitious, advocating non-cooperative games could take over game theory, putting down the *Theory of Games*'s cooperative approach.

Differently from his first paper, Nash (1951, p. 286) referred to his subsequent work as "Our theory," distinguishing it from that "very fruitful theory of two-person zero-sum games" of von Neumann and Morgenstern ([1944] 2007)—somewhat disregarding their program concerning n-person games. The capital difference between both approaches is how they moved from 2- to n-person problems. To study n-person games, von Neumann and Morgenstern ([1944] 2007) developed a new form for games—named *coalitional* or *characteristic function form*, being different from strategic and extensive forms—and a new solution, later named *stable set*. This move into n-person games kept back almost

[3] In the beginning of Nash's (1950a) paper one reads "Communicated by S. Lefschetz, November 16, 1949." Solomon Lefschetz was a mathematician remembered, among other contributions, by a fixed-point theorem named after him. As it is well-known, Lefschetz received a recommendation letter in favor of Nash, in which Richard J. Duffin said Princeton University should accept Nash in its Ph.D. program because he was "a mathematical genius."

[4] Nash's (1951) second paper on equilibrium points appeared in the *Annals of Mathematics*, a journal oriented toward mathematicians, unlike the *Proceedings of the National Academy of Sciences of the United States of America*, of general interest. This difference accounts for how "informal" Nash's (1950a) discussion might sound given his background as a mathematician.

nothing from von Neumann's (1928) take on two-person zero-sum games. Nash (1951, p. 286) sold his non-cooperative setup and equilibrium point as "a generalization of the concept of the solution of a two-person zero-sum game." That is, he would extend von Neumann's (1928) minimax to n-person games without redefining what games are. Making such a generalization was not costless, as it depended on neglecting coalition formation as a significant phenomenon of game theory. Importantly, seeking such a generalization was not a purely mathematical exercise, as for some games a notion of "fair play" prohibited coalition formation, such as poker, Nash (1951, p. 294) argued. But his ambitions were greater than solving poker games: Nash (1951, 295) argued his theory could enlighten cooperative games provided one could map acts of cooperation into moves of a larger non-cooperative game. In such a sense, Nash's (1951) equilibrium point rivaled von Neumann and Morgenstern's ([1944] 2007) stable sets. This lead became known as "the Nash program."[5]

The most notable formal difference between Nash's (1950b, 1951) works is in how he demonstrated his main theorem (as Giocoli (2004, p. 645) pointed). While in his first paper he resorted to Kakutani's (1941) fixed point theorem, later Nash (1951, p. 288) switched to Brouwer's (1912) fixed point theorem, which he considered to be a "considerable improvement." To accomplish that switch, it sufficed to construct a continuous transformation of the strategy space into itself in which fixed points corresponded to equilibrium points. Nash's (1951, p. 288) transformation mapped each strategy profile into a modification of it, following a formula which increased weights given to pure strategies of higher payoff and, similarly, decreased weights of those of lower earnings.[6] But another formal aspect of Nash's (1950b, 1951) papers is important: when he defined what games he would solve, he mentioned strategic form games (that is, a triplet including a set of players, sets of strategies, and a payoff function). Nowhere he associated his equilibrium with extensive form games—this link would not happen through his hands. This small detail, and the reading of Nash's (1950b, 1951) framework as a "generalization" of the Minimax Theorem, sets a different stage for explaining how the Nash equilibrium fell into oblivion—if it ever did, actually.

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[5] See Serrano (2018).

[6] For reference, take a player $i \in N$. Consider a mixed strategy profile $\sigma \in \Sigma$. Nash (1951, p. 288) defined a "set of continuous functions" $\varphi : S_i \bigtimes \Sigma_i \to \mathbb{R}$ such that

$$\forall \sigma \in \Sigma,\ \forall s_i \in S_i \quad \varphi_i(s_i, \sigma) = \max\left(0, u_i\left([s_i], \sigma_{-i}\right) - u_i(\sigma)\right)$$

Then he defined "the fixed points of the mapping" needed for his demonstration using a transformation $T : \Sigma \to \Sigma$ such that

$$\forall \sigma \in \Sigma \quad T(\sigma) = \left(\frac{\sigma_1 + \sum_{s \in S_1} \varphi_1(s_1, \sigma) \cdot [s_1]}{1 + \sum_{s_1 \in S_1} \varphi_1(s_1, \sigma)}, \cdots, \frac{\sigma_n + \sum_{s \in S_n} \varphi_1(s_n, \sigma) \cdot [s_n]}{1 + \sum_{s_n \in S_n} \varphi_n(s_n, \sigma)} \right)$$

5.2 Nash's Minimax Theorem

Contrarily to what some early game theory textbooks said, (see Table 3 for a list of textbooks published in 1950–1959), there is no such a thing as a "Nash's Minimax Theorem," nor there is an "equilibrium point" for two-person zero-sum games in the *Theory of Games*—nowhere did it refer to its solution concepts (minimax and stable sets) as "equilibrium points" (although von Neumann and Morgenstern ([1944] 2007, pp. 39, 43, 45) identified an equilibrium property in their solution concepts).[7] For example, Luce and Raiffa (1957, pp. 71-72) said "von Neumann proved that every two-person zero-sum game has an equilibrium point" when they presented von Neumann's (1928) minimax result, and Luce and Raiffa (1957, pp. 391-393) named one of their sections "Nash's Proof of the Minimax Theorem"—as if Nash's (1950b, 1951) articles in non-cooperative games concerned an alternative proof for von Neumann's (1928) theorem. Terminologies in textbooks suggest that game theorists read Nash's (1950b, 1951) contribution in a very particular light, playing up its formal relationship with von Neumann's (1928) Minimax Theorem; after all, as Nash (1951, p. 296) stated, his equilibrium point was "a generalization of the concept of the solution of a two-person zero-sum game."

Contrarily to what could be expected, Nash's (1951) work on equilibrium points received a substantial attention from early textbooks: among manuals published until 1959, titles from McKinsey (1952), Blackwell and Girshick (1954), Vajda (1956c), Luce and Raiffa (1957), and Karlin (1959b) mentioned it. Since Nash (1951, p. 286) presented his research as concerning n-person games, it was natural that, to some extent, textbooks would present it accordingly. Contextually, the Cold War environment demanded a particular focus of game theory, so developments following the *Theory of Games* concentrated on two-person games. Textbooks also reflected that need. They not only had to prepare new generations of theorists by teaching what was most central to game theory around that time, but they also sought to communicate what had changed since the *Theory of Games* was out—lacking specialized journals, game theory textbooks became rough summaries one could use to keep abreast with what was going on in research. As a consequence, textbook authors also privileged two- over n-person games. The only exception which truly provided an account of n-person game theory was Luce and Raiffa's (1957) *Games and Decisions*. Yet, some texts included reflections about n-person games, and in such discussions they represented Nash's (1951) contribution.

These citations were only part of short bibliographical surveys, however—in other words, they acknowledged Nash's (1951) notion of equilibrium point existed, but they did not fit it as a main component of game theory. For instance, McKinsey (1952) cited Nash (1951) in his chapter "Games in Extensive Form–General Theory," in its

[7] Most mentions in textbooks of 1950–1959 regarding Nash's (1950b, 1951) work speak of "equilibrium points," not "*Nash* equilibrium points"—this appendage of "Nash" would happend decades later.

section "Historical and Bibliographical Remarks." There, McKinsey (1952, p. 137) only said: "The notion of an equilibrium point was introduced in Nash [2]. The proof that every n-person game has an equilibrium point among its mixed strategies is also due to Nash." This statement does not do much for Nash's (1951) paper, and it does not make a difference to look at other textbooks. The circumstances in which Karlin (1959b, p. 172) cited Nash's equilibrium points are remarkably interesting: nowhere in his presentation he defined non-cooperative games or equilibrium points. Instead of bringing up Nash's (1951) work in his exposition, Karlin (1959b, pp. 172, 174) formulated two problems inspired by a RAND Memorandum of L. S. Shapley, in which he explored questions raised in solving infinite, non-zero-sum games for equilibrium points. These exercises appeared in a chapter titled "Games of Timing (Continued)," where timing games, Karlin (1959b, p. 31) explained, are "games in which the choice of a pure strategy represents the choice of a time to perform a specific action." Being regarded as something worth of two exercises in a chapter of a particularly specific subject, Nash's (1951) equilibrium point was only a minor point in Karlin's (1959b) exposition. Textbook authors were not verbal about why they mentioned Nash's (1951) in such a way, but a context-based explanation seems satisfactory.

While von Neumann and Morgenstern ([1944] 2007, p. 220) argued they "completed" their "theory of the zero-sum two-person game," newcomers to their field found many ways of digging it deeper. For example, Kuhn and Tucker (1951, pp. x-xi) indicated many open problems concerning computational techniques for finding solutions in two-person games of many strategies, establishing theorems for games of infinite strategies beyond "polynomial-like games," among others.[8] During 1951–1959, Nash (1951) did not receive many citations and, consequently, it did not receive as many extensions as von Neumann's (1928) minimax did. Looking at published papers, one example of an use of Nash's (1951) article comes from Nikaidô (1954, p. 65), who formulated another proof for von Neumann's (1928) Minimax Theorem. In particular, what Nash (1951) offered for Nikaidô (1954) was a particular strategy of employing fixed-point theorems.[9] Early surveys of game theory also placed Nash's (1951) work as part of two-person game theory. For instance, Wagner (1958, pp. 378-379) discussed it in his section "Two-Person Games," not in his section "n-Person Games." The Nash equilibrium, although conceived as a contribution toward n-person game theory, remained being a piece of two-person games. There was, however, one exception among textbooks: Luce and Raiffa's (1957) classic *Games and Decisions*.[10]

Games and Decisions, after covering two-person games but before explaining

[8] A game is *polynomial* when payoff functions are polynomial functions.

[9] Put simply, Nikaidô (1954, p. 69) built a transformation similar to that in Footnote 6.

[10] Rapoport's (1959, p. 61) survey mentioned Nash's (1951) as belonging to n-person game theory, but presented it as something of small importance; in surveying n-person games, he focused on cooperative games.

characteristic functions and stable sets (that is, cooperative n-person game theory), had a chapter named "Theories of n-Person Games in Normal Form." There, Luce and Raiffa's (1957) goal was to generalize previous discussions of mixed strategies and equilibrium points for n-person problems—until then, "equilibrium point" and "minimax" were synonyms because they had only explored two-person zero-sum games. The reason why they thought of "Theories of n-Person Games in Normal Form" was a certain dissatisfaction with the *Theory of Games* approach for n-person games. Luce and Raiffa (1957, p. 156) explained "A major obstacle to developing a satisfactory theory of coalition formation is that in the present formalizations of a game no explicit provisions are made about communication and collusion among the players". That is, to effectively lay down a "theory of collusion" game theorists had no way around adopting *ad hoc* hypotheses. Such situation had a critical implication: different assumptions yielded different solution concepts. Figure 3 below reproduces a table of *Games and Decisions* matching assumptions and solution concepts.[11] Luce and Raiffa (1957, p. 168) stated that most developments in game theory accepted side-payments, meaning players could pay "considerations or bribes," earning payoffs that game rules do not anticipate. To consider side-payments, it was necessary to admit that a commodity behaving similarly to money existed. Luce and Raiffa (1957, p. 169) also entertained whether players could communicate (and how), and if they could select mixed strategies with statistical correlation, effectively performing some type of coordination or not. These assumptions referred to different subareas of game theory.[12] Nash (1951, p. 286) brought up a new hypothesis, which implied opening a new subarea in game theory.

The critical assumption in Nash's (1951, p. 286) article was "the *absence* of coalitions:" players were not allowed to form groups to play against one another. *Games and Decisions* had a problem with Nash's (1951) assumption. Luce and Raiffa (1957, p. 164) thought that discussions within game theory and "common observation" suggested that "one important aspect of the phenomenon are the restrictions society places upon coalition formation and coalition changes." These limitations could come from many sources (either historical, moral, legal, or else). Here, Luce and Raiffa (1957, pp. 164-165) identified a major fault of n-person game theory, arguing that such restrictions were not formal elements of game theory, as assumptions about rational behavior and perfect information were. Consequently, game theory included none of such restrictions, and coalitions functioned freely. For Luce and Raiffa (1957, p. 165), von Neumann and Morgenstern's ([1944] 2007) approach was "extreme" because "any collusion logically possible is allowed to occur." Not only the *Theory of Games* way made it difficult to identify solutions, but it also promoted solution multiplicity without providing means

[11] In Figure 3, "Equilibrium Points," "Solutions," and "ψ-stability" refer, respectively, to the works from Nash (1951), von Neumann and Morgenstern ([1944] 2007), and Luce (1954).

[12] For instance, there are large literatures about games with side-payments and about games without side-payments.

Figure 3 – Hypotheses and Solutions of n-Person Games

Side Payment	Preplay Communication	Correlation of Strategies in Coalitions	Transferable Utility	Section	Name
No	None	Irrelevant	Irrelevant	7.8	Equilibrium Points
	Partial	Yes	Irrelevant	7.9	. . .
		No	Irrelevant	7.9	. . .
Yes	All	Yes	Yes	9.1–9.7	Solutions
			No	10.4	. . .
	Partial	Yes	Yes	10.1, 10.2	ψ-stability
			No	10.4	. . .
		No	. . .	. . .	. . .

Source: Luce and Raiffa (1957, p. 170).

of selecting one, opening a door "to the *ad hoc* assumption that in practice there exist social standards which determine *the* solution which actually occurs," Luce and Raiffa (1957, p. 165) contended. Just as the *Theory of Games* setup was extreme, so were Nash's (1951) non-cooperative games:

> [. . .] there is the other extreme which prohibits any collusion at all. Such a condition may not be nearly so limiting as it first seems. Certain authors, notably Nash [1951], hold that non-cooperative games are theoretically basic and that cooperative games can and should be subsumed under that theory by making communication and bargaining formal moves in a non-cooperative extensive game. [. . .]
>
> In a way, this conceptual solution to the formalization of preplay communication simply buries some of the most interesting aspects of the problem. One is interested in understanding the forces which lead groups to cooperate, in the cohesiveness of coalitions over repeated plays of the game, and so on, and we do not want to prejudge these problems by entering them into the extensive form in some special manner. (LUCE; RAIFFA, 1957, p. 165; underlining added)

Merits and deficiencies of Nash's (1951) equilibrium in n-person games explain, at least partially, how it became confused with von Neumann and Morgenstern's ([1944] 2007) minimax. *Games and Decisions* contained a discussion in such direction. The first merit Luce and Raiffa (1957, p. 170) saw in Nash's (1951) work was allying a general existence proof for n-person games and showing his concept was identical to von Neumann and Morgenstern's ([1944] 2007) minimax notion for $n = 2$ (under a zero-sum assumption). Remember, the *Theory of Games* provided distinct ways of solving games when $n = 2$ (using minimax strategies) and when $n \geq 3$ (employing stable sets). Then, Nash's (1950b) equilibrium point "was an important step, for, previously, no one had seen how to extend the maximin notion beyond $n = 2$," Luce and Raiffa (1957, p. 170) argued. Most mathematicians who worked on von Neumann and Morgenstern's ([1944] 2007) Minimax Theorem sought to simplify its proof or slightly modify its statement, and not to make it reach games of more than two players. But that is not all—Nash (1951) also attempted to connect his setup and equilibrium with research of others beyond von Neumann (1928).

These connections might look inconsequential, but they are meaningful. Nash's (1951) paper has a series of sections about seemingly disconnected minor results—"Symmetries of Games," "Solutions," "Simple Examples," "Geometrical Form of Solutions," "Dominance and Contradiction Methods," and "A Three-Man Poker Game"—, which effectively helped anchor Nash's (1951) framework within game theory. They established relations between what Nash (1951) did differently—non-cooperative games and equilibrium points—and traditional research subjects within game theory. The equivalence with von Neumann's (1928) Minimax Theorem and such other minor results more or less guided what mathematicians made out of Nash's (1951) paper. Remember, Nikaidô (1954, p. 65) provided "an alternative proof of the [minimax] theorem" based on Brouwer's (1912) fixed point theorem, citing Nash's (1951) as an important source for inspiration. To mention a second example, since mixed strategies were critical for Nash's (1951) existence theorem just as they were for von Neumann's (1928), a natural question was asking what conditions are sufficient for warranting equilibrium point existence in pure strategies—just as it happened with von Neumann's (1928) minimax. Dalkey (1953, pp. 217-218) sought an answer, identifying a "necessary and sufficient condition for a general game to have an equilibrium point in pure strategies independently of the particular pay-off function or of the particular probability distributions assigned to chance moves." These links help explain how Nash's (1951) fitted in two-person game theory; but it remains to consider how it did *not* fit in n-person games.

Although Nash (1951, pp. 288-289) guaranteed all n-person (non-cooperative) games have at least one equilibrium point, and maintained some connections with topics of interest of game theorists, his concept had some shortfalls. Two properties are important here, *interchangeability* and *equivalence*. To put them into words, interchangeability means that mixing any two equilibrium strategy profiles (by picking some components from one and others from another) makes up another equilibrium point. Consequently, if a game has multiple equilibria and players chose strategies corresponding to different equilibria, they will still end in an equilibrium point. In turn, equivalence means players do not have strict preferences over equilibria—their payoffs do not change in moving from one equilibrium to another. The minimax solution enjoyed both properties in two-person zero-sum games (but not in non-zero-sum scenarios). Nash's (1951) equilibrium point also missed both properties in non-zero-sum games, and Luce and Raiffa (1957, pp. 172-173) criticized it for such a lack. Luce and Raiffa (1957, p. 172) thought "The failure of the general equilibrium notion [of Nash (1951)] to have these two properties raises much more serious questions as to its merits than could be raised against the minimax concept." Put simply, they contended a player would not know how to play Nash's (1951) equilibrium if he faced a game of multiple equilibria (of potentially different payoff vectors). Both properties were necessary for Nash's (1951) equilibrium to attain in n-person non-cooperative games that success which von Neumann's (1928) minimax achieved in two-person zero-sum. There was hope, however:

> Nonetheless, we continue to have one very strong argument for equilibrium points: if our non-cooperative theory is to lead to an n-tuple of strategy choices and if it is to have the property that knowledge of the theory does not lead one to make a choice different from that dictated by the theory, then the strategies isolated by the theory must be equilibrium points.
>
> The complications of non-equivalence and non-interchangeability of equilibrium points lead one to ask whether there is not some plausible condition which may be added to isolate a single equilibrium point as more acceptable than the others. [. . .] (LUCE; RAIFFA, 1957, p. 173; underlining added)

Luce and Raiffa (1957, p. 173) contended that one way to enhancing Nash's (1951) equilibrium point, perhaps making it an adequate solution for n-person non-cooperative games, was to restrict it further, thus reducing equilibria multiplicity. They mentioned a work in such a direction from Gale (1953)—yet not fully satisfactory—, who proposed a procedure that effectively isolated one equilibrium in a finite number of steps (consisting of averaging payoff-equivalent strategies and eliminating dominated options).[13] This suggestion of narrowing Nash's (1951) concept by using some reasonability criteria strikes a familiar note in light of modern refinements of Nash's (1951) equilibrium points. Although a popular refinement would appear in a German paper already in 1965 (Selten's (1965a, 1965b) work on sub-game perfect equilibrium points), game theorists would not follow such a lead for a while; a meaningful flow of research would start only around 1975. Instead, they would emphasize a second deviation from von Neumann and Morgenstern's ([1944] 2007) cooperative framework, studying cooperative games without side-payments, which allows for strategy correlation and pre-play communication. Looking at textbooks, it appears that mathematicians were not comfortable with any alternatives for studying "strategy picking" in n-person games;

[13] Against Gale's (1953) work, Luce and Raiffa (1957, p. 173) questioned "there is no compelling reason why a player should put equal probability weights over all equivalent strategies."

and so they followed a more agnostic approach for such games based on coalitional-forms, which erased strategies from games. In such a context, Nash's (1951) equilibrium remained being an interesting piece but mostly for two-person game theory, what swept away its distinguishing features (most importantly, its ability to solve n-person games).

5.3 The Rise of Cooperative n-Person Games

As long as textbooks focused on two-person games, they did not have a strong reason for distinguishing Nash's (1951) equilibrium point and von Neumann and Morgenstern's ([1944] 2007) minimax solution. For mathematicians who pushed game theory to start paying more attention to equilibrium points, it would be necessary that they switched their research focus. Things would not change before 1967, when textbooks oriented toward n-person game theory started to appear. This does not mean that Nash's (1951) contribution passed unnoticed; when he published his key papers about non-cooperative games and equilibrium points, it immediately caught everyone's attention as a strong contender of von Neumann and Morgenstern's ([1944] 2007) solution for n-person game theory. As many other authors, Gale (1953) described that game theorists knew how problematic—read "complex"—n-person games were as the *Theory of Games* laid them down, and that Nash's (1951) approached functioned as an alternative path for its development:

> The theory of the general n-person game, in contrast to that of the zero-sum two-person game, remains in an unsettled state. The chief problem seems to be that of determining the proper definition of a solution for such games. The efforts in this direction divide themselves into two groups, the cooperative theory in which the players are expected to form coalitions, and the non-cooperative in which such coalitions are forbidden. The first group includes the theory of von Neumann and Morgenstern and the more recent work of Shapley, the second the equilibrium point theory of Nash. [. . .] (GALE, 1953, p. 496; underlining added)

Gale's (1953, p. 496) words seem to imply that by 1953 there was a substantial effort in developing non-cooperative n-person games. Citations to Nash's (1950b, 1951) papers show otherwise. In a survey titled *What Has Happened to the Theory of Games*, Hurwicz (1953, p. 398) pondered on why since the *Theory of Games* there was only "a minor flood of contributions to the various aspects of the theory of games and its applications" and discussed Nash's (1951) equilibrium concept. As Hurwicz (1953, pp. 401-402) explained, in constant-sum two-person games, von Neumann's (1928) minimax enjoyed two valuable properties. First, it embedded a rationality principle of maximizing payoff minima. In a way, such principle represents a defensive behavior, suggesting that players pick strategies attempting to secure a minimal earning. Alternative principles existed;

Hurwicz (1953, p. 398) himself named a few: L. J. Savage's principle of "minimaxing the regret rather than the loss," F. Modigliani's "maximax principle," and Hurwicz's own "principle of maximizing some weighted average of the maximal and minimal expected gains." Second, von Neumann's (1928) minimax possessed an equilibrium property: if one player plays his minimax strategy, it is optimal for his opponent to also choose his minimax strategy. Beyond two-person constant-sum games, however, one would have to choose between having a rationality principle or having that equilibrium property. For Hurwicz (1953, p. 402), it was questionable "the advisability of seeking solutions possessing the required equilibrium properties but sacrificing the rationality of behavior," as Nash (1951) did. Put it differently, he thought Nash's (1951) equilibrium missed an underlying rationality scheme, some intuitive explanation of why would anyone play at a Nash equilibrium. Textbooks show a different story.

Two textbooks are central to understanding what place Nash (1951) occupied when game theory started to concentrate on n-person games, a moment in which non-cooperative games could (but would not) rise in interest: G. Owen's *Game Theory*, of 1968, and A. Rapoport's *N-Person Game Theory: Concepts and Applications*, of 1970.[14] Around 1968–1970 Nash's (1951) work remained somehwat connected with von Neumann's (1928) minimax and two-person games, as Owen's (1968) exposition shows. The thread of his presentation runs as follows. In Chapter 2, "Two-Person Zero-Sum Games," Owen (1968, pp. 12-13) started his discussion with equilibrium points, emphasizing equilibria in pure strategies and noting that not always do two-person zero-sum games admit one equilibrium. This discussion motivated his presentation of mixed strategies, which finally led him to introduce von Neumann's (1928) minimax. Later, in his Chapter 7, "Two-Person General-Sum Games," Owen (1968, pp. 136-137) distinguished that (non-zero-sum) two-person games could be either cooperative or non-cooperative. He decided to present non-cooperative games first.[15] There, he defined equilibrium points and showed that all two-person games admit at least one equilibrium—now allowing for mixed strategies. There is no formal mention to Nash's (1951) work, but in demonstrating that theorem, Owen (1968, pp. 137-138) reproduced Nash's (1951) demonstration, which relies on constructing a transformation whose fixed points are equilibria, and then applying Brouwer's (1912) fixed-point theorem. This description shows that von Neumann's (1928) minimax and Nash's (1951) equilibrium point remained entangled. But one caveat is critical: equilibrium points in general-sum games were not as satisfactory as minimax strategies in zero-sum games.

The criticisms that Owen (1968, p. 139) raised against Nash's (1951) approach

[14] The Subsection 3.1.1, starting on page 60, covers biographical information on both authors.

[15] The next section of his Chapter 7 was about cooperative two-person games—a fancy name for bargaining models. Owen (1968, pp. 140-142) presented Nash's (1950a) bargaining model, paying him a formal reference.

relied on two famous examples: "The Battle of the Sexes" and "The Prisoner's Dilemma."[16] Both examples illustrate fundamental features of non-cooperative game theory: the Dilemma exemplifies how rational behavior does not always lead agents to socially optimal outcomes; and the Battle shows Nash's (1951) framework easily produces multiple equilibria, and also that some equilibria are inefficient (meaning all players would be better off playing some other equilibrium).[17] Back in 1968, both examples served to show how unfit Nash's (1951) setup was. For Owen (1968, p. 139), the Battle put in check how truly "stable" equilibria are: while it would be rational to play toward some equilibrium point, different players might have different preferences over what equilibrium they should play.[18] This thought of equilibrium preference is not embedded in Nash's (1951) approach, but it is, in a certain sense, in cooperative approaches. The notion of "dominance" in von Neumann and Morgenstern's ([1944] 2007) theory manifests precisely that.[19] Besides, Owen (1968, p. 139) continued, "Even when there is only one equilibrium pair [...] it is not clear that this equilibrium pair is exactly what we want." The Dilemma did not only mean players do not achieve a social optimum: for Owen (1968, p. 139), prisoners using Nash's (1951) concept would simply "play 'wrong.'" This criticism extended itself for larger games, having more than two players:

> In the non-cooperative case, <u>the principal question is the existence of equilibrium n-tuples</u>. This question is answered by the following theorem:
>
> **VIII.1.1 Theorem.** Any finite n-person non-cooperative game has at least one equilibrium n-tuple of mixed strategies.
>
> We will not give a proof of VIII.1.1. here. [. . .]
>
> <u>Although Theorem VIII.1.1. is certainly a valuable result, it may be pointed out that all the difficulties which were observed for equilibrium points of bimatrix games are also present here</u>. [. . .]
>
> In general, <u>there is no great difference between the theory of non-cooperative n-person games and non-cooperative two-person general-sum games</u>. (OWEN, 1968, pp. 155-156; underlining added)

This quote appears in Owen's (1968, pp. 155-156) roughly one-page long exposition of non-cooperative games. The passage suggests that mathematicians who worked on game theory had problems with Nash's (1951) equilibrium going beyond its multiplicity and lack of social optimality (as the Prisoner's Dilemma exemplifies). From

[16] Figure 2 on page 86 illustrates both examples.

[17] See Myerson's (1991, pp. 97-98) discussion, for instance.

[18] This problem refers to that property of equivalence which Luce and Raiffa (1957, pp. 170-173) addressed.

[19] To be more specific, in non-cooperative games, players obviously would have preference for equilibria which yield them higher payoffs. But "the making" of an equilibrium does not take that into account. Things are different with stable sets. To characterize one stable set solution, players need to select imputations which do not dominate one another—so even if players prefer one over another, there is no way for them to impose it. And more than that, such selection also bears a relation of being "dominant" over other imputations, left out of that stable set. Thus, what imputations players prefer is closely tied with stable sets.

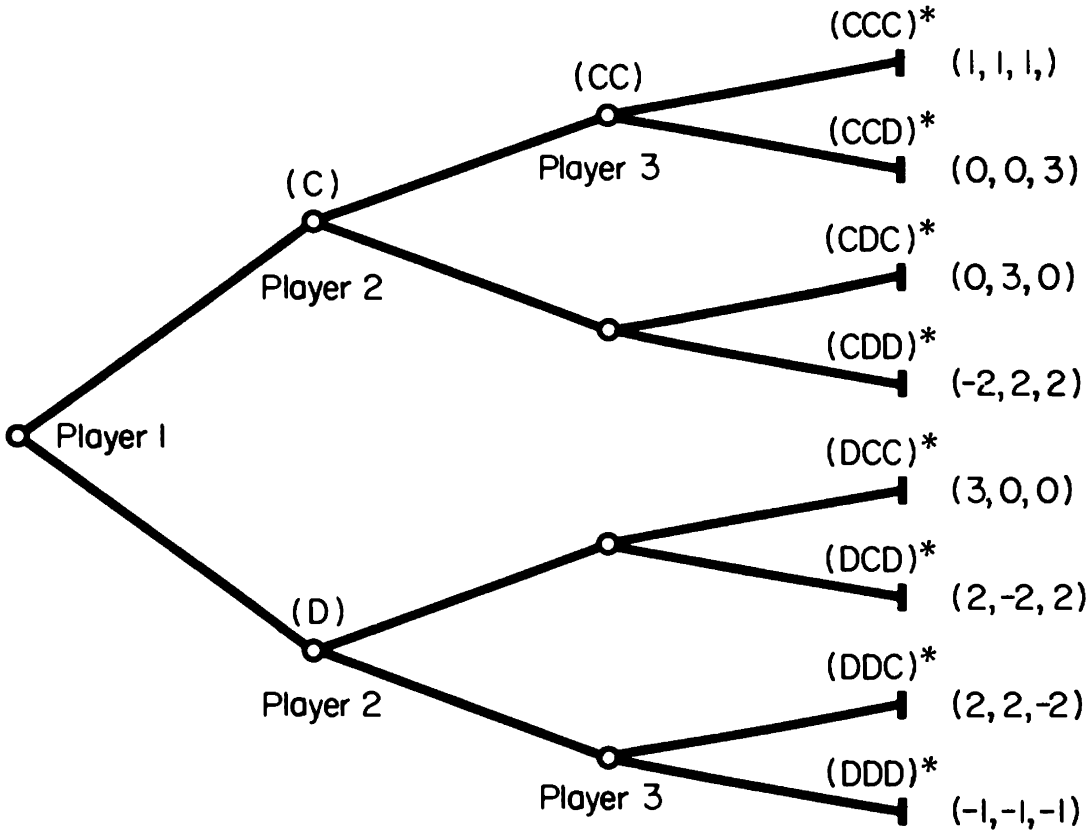
a mathematical perspective, there was no interesting question one could ask from Nash's (1951) approach. That means, if a mathematician studied Nash's (1951) non-cooperative games, he would not find many mathematical problems worth pursuing; other areas of (cooperative) game theory were far more fruitful in that regard. The context of that time explains it. As researchers switched focus from two- to n-person games, they looked back at the *Theory of Games* to take a starting point. This meant putting an emphatic interest in existence properties of game solutions, as von Neumann and Morgenstern ([1944] 2007) left them almost as a puzzle (concerning their stable set solution). There could be no related question about Nash's (1950b) equilibrium. The most pressing question of game theory circa 1968–1970 already had a solution in what regarded non-cooperative games. In fact, Owen (1968, pp. 155-156) did *not* discuss non-cooperative n-person games: he judged it was simply too easy for his reader to generalize his previous coverage for two-person games. Neglecting non-cooperative games, Owen (1968) could work on more exciting problems, which referred to cooperative game theory. This lack of involvement with Nash's (1951) work also appears by looking at citations it received in 1960–1967: surprisingly, while mathematicians still monopolized research on game theory, Nash's (1951) research received more attention from other scientists, such as economists.

The most cited papers of 1960–1967 which referred to Nash's (1951) classic appeared in journals of management, economics, and psychology.[20] Looking for mathematics papers—which are closer to the textbooks under discussion here—, two features rise up. First, they involved fewer citations to Nash (1951) than journals of other disciplines. Second, most of them were not about n-person non-cooperative games. The most cited article (which cited Nash (1951)) that regarded n-person games actually concerned cooperative n-person games: it was Aumann's (1961, p. 539) work about Gillies and Shapley's core, in which he just mentioned Nash's (1951) equilibrium point as one among many "different applications of the generalized 'core' notion." Apart from that, most papers cited Nash (1951) in a two-person context. Two examples are Lemke and Howson (1964, p. 414), who just mentioned that Nash (1951) used a fixed-point theorem in his main proof; and Shapley (1964, p. 1), who mentioned Nash (1951) because of his definition of symmetric games—and not because of his definition of non-cooperative games and equilibrium points.

This large disinterest for non-cooperative n-person games reflected back on other objects of game theory, so as to produce artifacts which could cause puzzlement today, as a cooperative three-person Prisoner's Dilemma, written as game in coalitional form. Such a curious take on the Dilemma appeared in Rapoport's (1970) textbook, which fully dismissed non-cooperative games—it was all about cooperative games.

[20] Some of such journals were *Management Science*—where Harsanyi (1967, 1968a, 1968b) published his famous papers on games of incomplete information—, *Econometrica*, and the *Annual Review of Psychology*.

Figure 4 – A. Rapoport's Three-Person Prisoner's Dilemma



Source: Rapoport (1970, p. 80). "C" stands for "cooperate," whereas "D" stands for "defect."

Historically, the Dilemma was an example devised to contrast von Neumann and Morgenstern's ([1944] 2007) cooperative and Nash's (1951) non-cooperative approaches, but Rapoport (1970) transformed it into an element of cooperative games alone.[21] In particular, Rapoport (1970, pp. 79-80) constructed a three-person Prisoner's Dilemma, in which each player should choose between cooperation and defection, as it is usual. Such a three-person game would yield a single solution in which all players defect, being analogous to the standard two-person Dilemma. Rapoport (1970, p. 80) pointed, however, that if "we allow coalitions in the game of perfect information, we get some curious results." To help understand what he found to be "curious," Figure 4 reproduces Rapoport's (1970, p. 80) Dilemma. The payoff structure of his game made one defector earn more than a coalition of two cooperative players, and a three-person coalition implied a socially optimal outcome.[22] As a consequence, "the dilemma is even more

[21] The idea of a three-person Prisoner's Dilemma was no novelty outside of textbooks. Psychologists, who approached two-person Dilemmas through experimentation, also investigated multiplayer versions of such example.

[22] Although Figure 4 has an extensive-form game, to analyze it, Rapoport (1970, p. 81) converted it to coalitional-form. In particular, such a game has a characteristic function as follows: $v(\varnothing) = 0$; $v(\{1\}) = v(\{2\}) = v(\{3\}) = 2$; $v(\{1,2\}) = v(\{1,3\}) = v(\{2,3\}) = 0$; and $v(\{1,2,3\}) = 3$). In words, Rapoport's (1970, pp. 79-80) game was so that: "If all three cooperate, each wins 1 unit. The Prisoner's Dilemma feature enters via rewards accruing to defectors, provided not all three players defect. In particular, a single defector gets the largest payoff 3. Each of two defectors gets 2, i.e., a smaller payoff

severe in the Three-person version of Prisoner's Dilemma than in the Two-person version," Rapoport (1970, p. 82) suggested. This is so because in a two-person game, no dilemma remains if players can collude: they would not defect one another. But in a three-person game, no player wants to join a coalition with just one more player; and even if other two players collude, the third will prefer to remain alone. The third player added a layer of complexity which would require a more complex tweak to make cooperation be a viable option—just allowing players to talk and collude would not be enough.[23]

Until the mid-1970s economists sort of "rejected" Nash's (1951) contribution and game theory more broadly.[24] Regardless of what motivated this early "rejection," still Nash (1951) could attain some success among mathematicians, who predominantly pushed game theory forward back in 1944–1970. Textbooks suggest two factors explaining how even Nash's (1951) fellow mathematicians dismissed his work. First, there is a question of disciplinary image. As Dresher, Tucker and Wolfe (1958b, p. 2) explained in criticizing the *Theory of Games* approach for cooperative games, mathematicians "demand" their research subject to be "both mathematically deep and elegant if it is to hold" their "attention." Owen's (1968) textbook painted a particular picture of Nash's (1951) framework as too easy, not even requiring presentation once one of his readers studied two-person games. The non-cooperative equilibrium was not as arresting as other parts of game theory, which posed greater challenges for mathematicians. Second, mathematicians felt Nash's (1951) underperformed as a solution concept even in simple games, as the Prisoner's Dilemma; and here, to "make it right" what Nash's (1951) equilibrium suggested—that prisoners should defect one another—, it was enough to think of coalitions to make up a better solution. As a result, to a certain extent Nash's (1951) non-cooperative games remained either out of radar or remembered as a part of two-person game theory (because of its relationship with von Neumann's (1928) elegant Minimax Theorem). Apparently nothing changed between 1950–1959 and 1968–1970, textbooks indicate. For Nash's (1951) equilibrium to break free, it seems that game theory would need some shock—and that was about to happen around 1968–1970, when W. F. Lucas found a 10-person game having no stable set solution.

than that of a single defector but more than that of a single cooperator or of each of two cooperators, 0. A single cooperator (the 'sucker') suffers the largest loss, −2. If all three defect, each loses 1 unit."

[23] This "tweak" referred to a property o characteristic functions, and the Dilemma became an example to discuss that property in Rapoport's (1970, pp. 82-86) text.

[24] For instance, check Giocoli's (2004, p. 640) description: "The popularity of game theory in general, and of NE in particular, is indeed a relatively recent event. In the 1950s and 1960s most neoclassical economists simply ignored that their discipline's central concept, rational equilibrium behavior, had finally found a precise, simple, and very general formulation. Even in the 1970s, game theory still remained a discipline for the specialists, and it was at least a decade away from making its official entry into the tables of contents of standard textbooks in economics."

5.4 J. W. Friedman's Reconciliation

The years around around the time when Lucas (1967, 1968) obtained his counter-example are remarkably curious. Until then, mathematicians predominantly developed game theory—and not other scientists—, and they mostly did it following von Neumann and Morgenstern's ([1944] 2007) lead for coalitional-form games, in spite of many problems they had with such games.[25] The counter-example would stimulate some changes in cooperative game theory, especially because the *Theory of Games* stable set solution could not be as central as it was any longer. What is surprising is that, precisely at that moment of questioning and reorientation, game theory spread over many fields. Textbooks of 1972–1978 meshed game theory and sociology, political science, linguistics, and even theology. In such a period, economics and game theory also showed signs of a new relationship. This does not mean game theory abruptly became popular among economists; instead, the textbooks of 1972–1978 functioned as an attempt to bridge both disciplines.[26]

The main textbook presenting Nash's (1951) equilibrium as part of economics was J. W. Friedman's *Oligopoly and the Theory of Games*, of 1977.[27] As it concerned industrial organization, it is natural to ask how game theory reached industrial organization. Figure 5 suggests that it was a slow-paced matter. The chart considers papers published in journals of economics which had "duopoly" or "oligopoly" in their keywords, and which also had "game." Three facts deserve attention. First, the *Theory of Games* and Shubik's (1959b) *Strategy and Market Structure: Competition, Oligopoly, and the Theory of Games* seemingly did not stimulate much use for game theory in imperfect competition models; although Figure 5 shows a rise in proportion of papers between 1955–1960, it preceded Shubik's (1959b) work.[28] Second, between 1965–1975, there was a rise and a partial retraction, which textbooks do not account for (around that time, no textbook was linking both disciplines). Third, somewhere between 1980–1985, a forceful acceleration happened—it started after Friedman's (1977) text and before Tirole's (1988) now-classic manual. These facts point that possibly Friedman's (1977) text might show what was

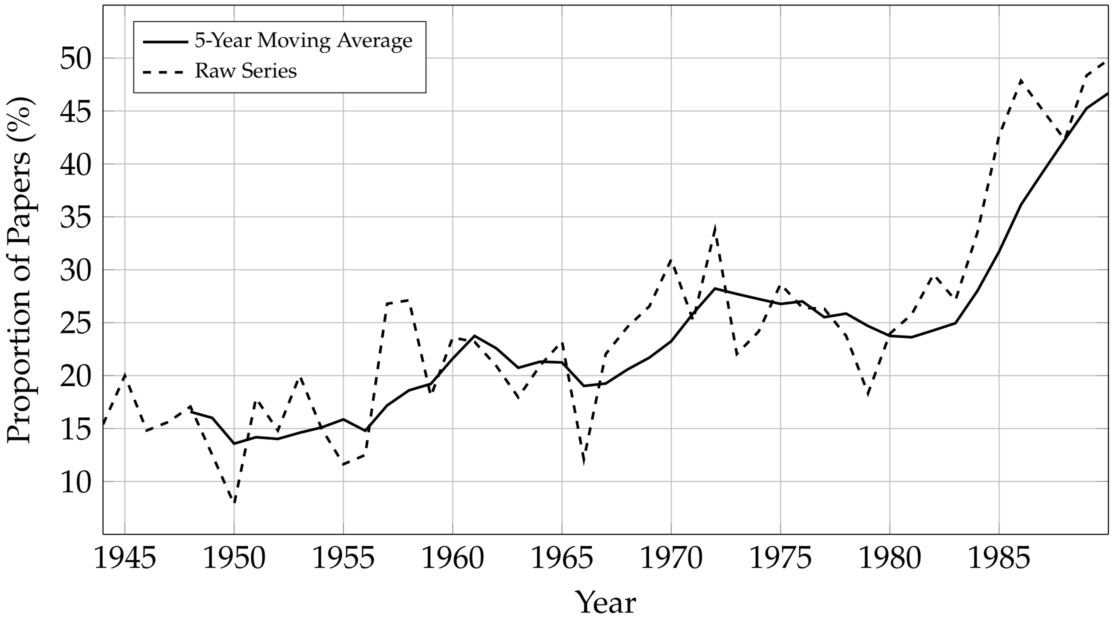
[25] For instance, Dresher, Tucker and Wolfe's (1958b, pp. 2-3) preface for the Volume III of *Contributions to the Theory of Games* summarized some existing criticisms.

[26] The literature on incomplete information games and refinements of Nash's (1951) equilibrium could have played a major role here. For instance, Giocoli (2009, pp. 191-194) pointed it and J. C. Harsanyi's contribution on games of incomplete information may explain how come game theory experienced a boom in economics in the late 1970s and the 1980s. Truly, key contributions were already out by 1972-1978: Harsanyi (1967, 1968a, 1968b) had published his pieces which grounded Bayesian games, and before that Selten (1965a, 1965b) worked on his sub-game perfect Nash equilibrium. However, such contributions would gain traction a bit later, as far as textbooks goes. In particular, sub-game perfect equilibrium would receive more attention after Selten (1975) translated his ideas from German to English, and while Harsanyi (1967, 1968a, 1968b) stimulated a considerable research flow, it was mostly of theoretical nature, and it does not account for textbooks of 1972-1978.

[27] For a biographical account of Friedman, see Section 4.2, on page 88.

[28] For a quick summary of Shubik's (1959b) contribution, check out Subsection 4.2.1, on page 92.

Figure 5 – Papers of Oligopoly Theory Mentioning "Game," 1944–1989



Source: JSTOR's Constellate.

going on when (non-cooperative) games started to gain popularity among economists.

To reinforce what Figure 5 indicates, consider Schotter and Schwödiauer's (1980) survey for the *Journal of Economic Literature*, titled *Economics and the Theory of Games: A Survey*. In it they described in which ways economics had been using game theory, especially in years close to 1980.[29] This survey concerned industrial organization among other topics, but Schotter and Schwödiauer's (1980, pp. 511-512) feelings were not particularly positive. Even if "oligopolistic decision" was a "natural domain of game-theoretical analysis" because of its intrinsic strategic component, they still believed game theory was not "doing much more than generalizing the results obtained already by A. A. Cournot (1838), Joseph Bertrand (1883), Wilhelm Launhardt (1885), Edgeworth (1897), Harold Hotelling (1929), Edward Chamberlin (1933), and Heinrich von Stackelberg (1934)." In such a sense, the textbooks of 1972-1978, such as Friedman's (1977), reflect more what their authors researched than a general trend of what most economists worked on in industrial organization.

The main characteristic of Friedman's (1977) textbook, which differentiated it from other texts mixing industrial organization and game theory, is that it presented Nash's (1951) contribution in a new form. The running thread of his text was, at first sight, in line with Schotter and Schwödiauer's (1980, pp. 511-512) comment, putting old models in

[29] Schotter and Schwödiauer's (1980) had sections about applications, whose titles illustrate in what subareas of economics games were making their way into: "Strategy-Proof Voting Mechanisms, the Gibbard-Satterthwaite Theorems, Arrow's Problem, Implementation, and Game Theory," "Externalities and Public Goods," "Models of Multilateral Exchange; Games and Markets," and "Models of Oligopolistic Competition and Collusion."

game theory clothes. *Oligopoly and the Theory of Games* has two parts. Part I, "Tradidional Oligopoly Models and Their Extensions," presented classic models, meaning they were non-game theoretical (at least not in a direct sense). The chapters in this part discussed single-period Cournot models, price-setting in markets of differentiated products, and dynamic models based on reaction functions.[30] Part II, "Game Theory," was an introduction to game theory. Chapters here mirrored those of Part I: for instance, Friedman (1977, p. 9-10) pointed how "In part I, ch. 2 covers single-period models of the Cournot type" and "Chapter 7 is a game theoretic counterpart of chs. 2 and 3." But contrarily to what it might look like, Part II of *Oligopoly and the Theory of Games* was more than a replay: in Friedman's (1977) attempt to bridge industrial organization and game theory, he presented a "reshaped" or "extended version" non-cooperative game theory, which include artifacts not seen before in textbooks.

The key chapter in Friedman's (1977) book was "Noncooperative Equilibria for Supergames Lacking Time Dependence," which closely followed his research—and which possibly explains how come other textbooks discussing Nash's (1951) did not do it as Friedman (1977) did. Friedman (1977, p. 173) extracted most of that chapter from a paper he published in 1971, in the *Review of Economic Studies*. There, Friedman (1971a, p. 1) defended that "Oligopoly may profitably be viewed as a supergame," and stated that he would present a "new concept of solution for non-cooperative supergames." For clarification, a "supergame" is simply a repeated game. Until then, most textbook-presentations of non-cooperative games focused on Nash's (1951) existence theorem, and if they discussed economics, they would establish a connection between it and Cournot's (1938) model. Friedman's (1977) textbook added new ingredients to that mix. To mention a few, *Oligopoly and the Theory of Games* brought forward extensive-form games; the Folk Theorem; and even sub-game perfect equilibria.

Friedman (1971a, p. 4) was concerned with infinitely repeated games, in which players cared about their discounted payoff sums and, as he argued, "Existence of non-cooperative equilibria in the supergame is no problem [. . .] it is easy to show existence of a large number [of equilibria]." Although proving existence was easy, he contended that "The principle task of this paper is to choose among these [equilibria] in a particular way and single out certain equilibria as being of special interest." His stage game involved some simplifying assumptions—including that such game had only one equilibrium point, which was not Pareto optimal. The usual solution for it would be to play at the non-Pareto optimal Nash equilibrium every time (just as in the Prisoners' Dilemma), what Friedman (1971a, pp. 4-5) called "Cournot strategy." Friedman (1971a) wanted to "escape" that outcome, showing firms could cooperate

[30] Put simply and as Friedman (1977, p. 9) did, "A reaction function is a decision rule for a firm which gives its current period choice as a function of the observed choices of all the firms in the market in the preceding time period."

in an industry under certain conditions, even if they played a non-cooperative game, which forbid communication and non-self-enforcing agreements. To do so, he reflect on how threats should work in an extensive-form game.

> Now consider the cooperative game from another vantage point. When a single player (or a subset forming a coalition) calculates the best payoff he can get by himself, he does so on the assumption that all other players will band together and adopt a strategy aimed at minimizing his payoff. Even in a cooperative game, this may appear an unduly costly way for the others to act; however, as a threat to coerce the player into an agreement with all other players, it has some appeal. By contrast, in the non-cooperative game coalitions are ruled out, players cannot talk and bargain with one another; hence, it is foolish to think other players wish to minimize one's own payoff. Each will want to maximize his own payoff and will not really care about payoffs to others. <u>In other words, threats are out of place in non-cooperative games because they cannot be clearly and effectively voiced, and because they are not credible. They need not be carried out and there is no incentive to do so</u>. (FRIEDMAN, 1971a, p. 7; underlining added)

That is, one distinctive feature of his solution was showing that "the usual notions of 'threat' which are found in the literature of game theory make no sense in non-cooperative supergames," Friedman (1971a, p. 1) explained. This quote should strike a familiar note for economists: it is well-known that Selten's (1965a, 1965b, 1975) notion of sub-game perfect Nash equilibrium point achieved precisely that; it shows some threats are non-credible, meaning that no rational player would carry them on if they "had" to. Importantly, Selten's (1965a, 1965b) solution concept was not available in English-language papers. He wrote his research in German, and would only publish an English article on sub-game perfect equilibria in 1975. Friedman (1971a) did not identify his solution as a sub-game perfect equilibrium, nor did he cite Selten's (1965a, 1965b). But effectively he applied sub-game perfect equilibria, making his players chose sequentially rational moves before any "boom" in research on refinements of equilibria.

Possibly, Friedman thought of working out such solution after meeting Selten or someone who was familiar with Selten's research. In a paper he submitted on August 13, 1969, Friedman (1971b, p. 106) said in a footnote that his "paper was originally written during the author's tenure as a Yale Junior Faculty Fellow, while he was a guest at the Center for Research in Management Science at the University of California, Berkeley." It is known that during 1967–1968 Selten (1968) visited that same Center, where he distributed a working paper titled "An Oligopoly Model with Demand Inertia," which repeated his definition of sub-game perfect equilibrium (now in English). Given that Friedman (1971a, p. 1) firstly submitted his paper about supergames on May, 1969, it is possible to conjecture that he met Selten and his work at Berkeley (or heard about it from someone else). But Friedman's (1977) textbook not only slipped in a Nash equilibrium refinement, it also formalized an old result mathematicians thought of decades before.

To fully grasp such a feature of Friedman's (1977) textbook, it is necessary to take a step back. The lore of game theory frequently brings up a story about a result that everybody knew about, but no one cared to publish: "The Folk Theorem," it ended up being called. As Aumann (1987, pp. 16, 31) put it, "The authorship of the Folk Theorem, which surfaced in the late 1950s, is obscure," adding that it "passed on by word of mouth, or remained hidden in ill-circulated research memoranda." Modern textbooks also repeat that story that Aumann (1987) told. For instance, Myerson (1991, p. 332) said "these feasibility theorems have been referred to as *folk theorems*, because some weak feasibility theorems were understood or believed by many game theorists, as a part of an oral folk tradition, before any rigorous statements were published." Before linking Friedman's (1977) textbook to the folk theorem, a minor clarification is due—after all, there is no such a thing as *the* Folk Theorem.

The Folk Theorem is better described as a cluster of theorems than as a single theorem by itself. In repeated games, be them finite or infinite, they answer what payoff allocations players can achieve in equilibrium (and under what conditions they do so). A classic example involves the Prisoners' Dilemma. In a one-shot play, prisoners defect on one another and end in a non-Pareto optimal allocation. But in an infinitely repeated play, matters may change. For example, if players are sufficiently patient, they may play a grim-trigger strategy: one player might cooperate indefinitely, unless his opponent defects, in which case he will defect from then onward. Here, it does not matter who first thought of one such Folk Theorem, or who first formally published it. By including a Folk Theorem, *Oligopoly and the Theory of Games* distinguished itself from other books and textbooks which also applied Nash's (1951) setup to industrial economics (for example, Shubik's (1959b) and Telser's (1972) books). Friedman (1971a, p. 11) used his Folk Theorem to attack an ancient problem in industrial organization. Looking at real-word industries, he noted that making coalitions (that is, behaving as in cooperative game theory) is troublesome because there are issues of communication, trust, and legislation. Nonetheless, firms still collude sometimes. Translating that feature in an economic model was problematic; until then, economists usually assumed firms would either collude or compete. In particular, Friedman (1971a, p. 11) criticized a certain "shortsightedness" of what he called "Cournot behaviour," which implied a non-Pareto optimal equilibrium. The Folk Theorem allowed a "reconciliation," an "equilibrium which is both Pareto optimal and a non-cooperative equilibrium."

In sum, Friedman's (1971a) model had three noteworthy ingredients that distinguished it from other uses of game theory in economics: it privileged extensive form games; it applied a refinement of Nash's (1951) equilibrium before they became popular; and it formalized a Folk Theorem in a journal of economics. This model reappeared in Friedman's (1977) textbook, and it was precisely what reviewers found most interesting in it.[31] No texts discussed such elements before. Naturally, it is problematic to infer that Friedman (1971a, 1977) made Folk Theorems or refinements of Nash (1951) equilibria gain momentum in economics—especially considering how difficult it is to appraise how influential a textbook is, considering their citation track record might be misleading. Yet, it is undeniable that Friedman (1971a, 1977) did something different, and not much before a "boom" of game theory would hit industrial organization (as Figure 5 shows).

5.5 Conclusion

Textbooks show how the Nash equilibrium became "the" Nash equilibrium. At first sight, Nash's work is simply an outcome of a mathematician's mind: in textbooks, it is simply a sequence comprising a definition, a theorem, and a proof. As game theory had always been populated by mathematicians or mathematically-inclined scientists, textbook authors continuously represented Nash's contribution by applying that definition-theorem-proof scheme. However, textbooks are valuable insofar they display how their authors organized game theory—in particular, by pointing how they connected game theory's assorted pieces; how they ordered and compared subjects; how they justified and exemplified statements; and so on. Under such light, textbooks show that the Nash equilibrium went through substantial change since its publication in 1950–1951 until its later absorption.

In a first moment, when game theory was synonymous with two-person game theory, Nash's equilibrium got mixed with von Neumann's minimax. Such a mixture happened for formal reasons: Nash designed his equilibrium point as a generalization of von Neumann's minimax, so both concepts should be equivalent in a situation in which von Neumann's minimax had its maximum prestige, namely, two-person zero-sum games. As long as game theorists focused on two-person games, however, there would be no compelling reason to favor Nash's approach. This situation could only change if researchers switched focus to n-person games, where Nash's way could excel, but even when that happened—around 1968–1970—, Nash's contribution did not raise to prominence. Textbooks are not fully clear about how that happened, but everything seems to point toward the mathematicians' disciplinary image. Game theory was never an esteemed subject within mathematics and, thus, for a mathematician to be "arrested" by game theory, it should provide him interesting and meaningful problems to solve. When practitioners started looking at n-person games, they observed that the *Theory of Games* left a huge open question, related to showing that all games have a stable set solution. Doing so was exceedingly difficult, so a main line of research in game theory

[31] For instance, Binmore (1978, p. 103) contended such discussion of Friedman's research on supergames and oligopolies is what is "most interesting" in Friedman's (1977) book, in which he found situations where non-collusive yet efficient behavior emerges. Eichner (1978, p. 1019) sustained a similar praise.

emerged: that of seeking particular solutions for particular problems. In such a context, Nash's non-cooperative games and equilibrium did not have challenging open problems: its existence was a simple matter of applying one fixed-point theorem or another.

This matter of non-cooperation *versus* cooperation was also a theme in economics, especially in industrial organization. Textbooks suggest that a possible spark explaining how industrial organization and game theory suddenly became inseparable comes from reconciling both approaches through some Folk Theorem. This result was widely known since the two-person era of game theory, but it never became formalized. Here, J. W. Friedman appears to be an important character. His textbook followed his research closely and such a characteristic distinguished its presentation from past expositions of Nash's contribution, which economists knew to be equal to A. A. Cournot's model solution. Based on one Folk Theorem, Friedman was able to answer if firms in a given industry would compete or collude. While it is hard to appraise how much influence his textbook enjoyed, it seems that it was a considerable: soon after its publication "the boom" of game theory would kick off, and Friedman would publish two other textbooks about game theory and industrial organization. By then, Nash's work was in ascension within economics, and in no time J. Tirole would publish his now-classic textbook, which cemented Nash's equilibrium in the heart of industrial organization.

6 The Core: What Textbooks Say and Don't Say

Looking at modern microeconomics textbooks, "the core" appears as a fundamental ingredient of general equilibrium theory. In Mas-Collel, Whinston and Green's (1995) textbook, it comes up in a chapter titled "Some Foundations for Competitive Equilibria." Accounts about that concept usually go back to F. Y. Edgeworth's *Mathematical Physics*. For instance, Mas-Collel, Whinston and Green (1995, pp. 652-653) said that Edgeworth attempted to explain how "the presence of many interacting competitors would lead to the emergence of a system of prices taken as given by economic agents, and consequently to a Walrasian equilibrium outcome." However, Edgeworth's point would not stimulate much research, they continued. The core fell into oblivion, only to be rediscovered later. As Cogliano (2019, p. 1) documented, the core would become an essential part of general equilibrium theory only in the 1960s through Herbert Scarf, Lloyd Shapley, and Martin Shubik, who used it to develop existence results which did not rely on fixed-points theorems.[1] However, these authors did not look back to Edgeworth's *Mathematical Physics* for insight; instead, they got "the core" they used from cooperative game theory.

Before proceeding, clarifying what "the core" means might be helpful. In exchange economy models, it consists of allocations which are simultaneously feasible—meaning they respect endowment restrictions—and unblocked—meaning a subset of consumers cannot improve their situation by exchanging among themselves. In game theory, "the core" specifies what payoff distributions (called "imputations") coalitions of players do not block from whatever game they are playing. In modern game theory textbooks, that concept appears in their final chapters only. For example, Myerson (1991) has a section on cores, while Osborne and Rubinstein (2012) and Maschler, Solan and Zamir (2013) have one full chapter about it; but Fudenberg and Tirole (1991) left out cooperative games completely from their presentation. Possibly, most courses addressing economists skip such sections and chapters. However useful for general equilibrium theory, it is striking that such a concept has not sustained a similar significance in game theory.

Historical accounts of game theory say that while Edgeworth's (1881) and von Neumann and Morgenstern's ([1944] 2007) writings already embedded a certain notion of core, its formal definition appeared firstly in a Ph.D. dissertation of a Princeton University mathematics student. His name was Donald B. Gillies, and he defended his

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1 Cogliano (2019) provides an historical account of the core in general equilibrium theory, including a discussion of why it eventually declined in importance.

research in 1953.[2] Usually Lloyd S. Shapley also receives credit for "creating" the core, possibly because of his assistance to Gillies.[3] In particular, Gillies (1953) only thanked Albert W. Tucker and Shapley in his "Acknowledgment" section. Besides, Shapley and Shubik (1966, p. 805) later stated that "The term 'core' was introduced by Gillies and Shapley [13, 10] in studying properties of the von Neumann-Morgenstern solutions; the core as an independent solution concept was developed by the latter in lectures at Princeton in the fall of 1953." The reference to Shapley concerned a report of an informal conference held at Princeton University in 1953. That is, Shapley's contribution occurred off the record, so it is difficult to disentangle his and Gillies' roles in crafting the modern notion of core.

Apart from its not-so-certain origin, a broad look at game theory shows a story of rise and fall for Gillies and Shapley's core.[4] This story has two parallel threads. First, around 1953 game theory-inclined mathematicians favored research in two-person problems, and the core simply could not draw much attention; it belonged to n-person games, situations in which players form coalitions. Years later, when n-person games arouse in interest, surveys of game theory always brought up Gillies and Shapley's core side-by-side with von Neumann and Morgenstern's ([1944] 2007) solution concept, attesting to its importance.[5] Here, textbooks suggest that the fate of Gillies and Shapley's core was closely related to that of von Neumann and Morgenstern's ([1944] 2007) stable sets: that is, as long as stable sets enjoyed some prestige, so it would Gillies and Shapley's core.

Second, M. Shubik found a remarkable application for Gillies and Shapley's core in 1959, making *cooperative* games relevant for economics. His research stimulated other inquiries, and until very recently textbooks ascribed to Gillies and Shapley's core more importance than modern textbooks do. Textbooks are far less helpful to uncover what occurred here, but they suggest an explanation of how Gillies and Shapley's core became a subject of final (and often skipped) chapters.

6.1 The Original Role

To understand what happened with Gillies and Shapley's core, it is necessary to go back to Gillies' Ph.D. dissertation, which contextualizes its creation. Gillies was one

[2] For example, Aumann (1987, p. 539) said Edgeworth (1881) and von Neumann and Morgenstern ([1944] 2007) made an implicit use of cores before Gillies's (1953) formal definition.

[3] For instance, see Aumann's (1987, p. 539) and Serrano's (2013, p. 608) comments.

[4] Game theory also had a role in Arrow and Debreu's (1954) contribution on general equilibrium theory (through Nash's work). Interestingly, their piece had a different fate in general equilibrium theory in comparison with the core. This point is, however, beyond reach here—pursuing it would involve studying also microeconomics textbooks.

[5] Examples include Vorobyov's (1970, p. 98) and Lucas's (1971, pp. 501-502) surveys.

of von Neumann's few advisees at Princeton University, finishing his Ph.D. in 1953.[6] His dissertation, titled *Some Theorems on n-Person Games*, was not following any trends; around 1953, most game theorists prioritized two-person over n-person games. The *Contributions to the Theory of Games* books, a series of the Annals of Mathematics Studies collecting new papers from leading game theorists, shows so. Volumes I and II, of 1950 and 1953 respectively, possessed 36 papers, of which only 5 fell into a category of "General N-Person Games."[7] By then, von Neumann and Morgenstern's ([1944] 2007) solution concept had no rivals. This explains how, by 1953, people referred to it as "solution" instead of "von Neumann-Morgenstern solution." Only later it became known as "stable set," when other alternative solution concepts emerged. Studying n-person games by stable sets was a task of high complexity—no wonder that von Neumann and Morgenstern ([1944] 2007) left gaps in their investigations of 4-person and larger games. In such context, Gillies' dissertation about n-person games primarily attempted to understand more about stable sets: it was not among its goals to design a new solution for n-person games.

More specifically, Gillies's (1953, p. 8) presented his research offering it as a tool only: his conception of "core" was helpful to find a game's stable set, and he did not think of it as an alternative solution concept to von Neumann and Morgenstern's solution. For illustration, reconsider Table 1, which contains a three-person cooperative game. Instead of solving it directly for von Neumann and Morgenstern's stable sets, it is also possible to first identify its core, and then complement it with other imputations. As pointed by then, Table 1's game has a single imputation in its core, namely, $(1, 0, 0)$. To apply such information, one possible way to proceed consists in finding what imputations does $(1, 0, 0)$ dominate, so it is possible to distinguish some imputations which are *not* part of *any* stable set solution. Naturally, Gillies and Shapley's core is not always useful for finding stable sets—just as in Table 2, many games have empty cores.

Although Gillies (1953, p. 21) discussed situations in which cores and stable sets would coincide, he did not portray his concept as a rival for stable sets. Similarly to his contemporaries, Gillies (1953, p. 6) thought of stable sets as *the* way of solving n-person games. Modern textbooks covering cooperative games always present a handful of solution concepts, but it is not clear how such a literature emerged. On one side, the mathematicians at the Hebrew University of Jerusalem produced more solution concepts than anyone else; and curiously, modern textbooks suggest that some game theory concepts which were not solution concepts, such as Gillies and Shapley's core, eventually

[6] The Mathematics Genealogy Project indicates von Neumann advised only 5 graduate students between 1936–1956, all of them in Princeton University. Gillies's turn to game theory reads as a detour in his career: his initial work was about computer science at the University of Illinois, until he got transferred to Princeton to work under von Neumann. In spite of his work on cores between 1953–1959, he is mostly remembered as a computer scientist.

[7] This information is available in Table 4 (on page 41).

became seen as solutions.[8]

6.2 Becoming an "Alternative Solution"

Early game theory textbooks of 1950–1959 did not discuss Gillies and Shapley's core, as it would be expected since they focused on two-person games. The only exception was Luce and Raiffa's (1957) classic, *Games and Decisions*.[9] Their presentation of cores, unlike that of Gillies (1953), was not simply of a tool aiding in determining stable sets. Effectively, *Games and Decisions* suggested changes in how practitioners should approach Gillies and Shapley's core. Two factors explain how come a textbook would propose such an "innovation." First, Luce and Raiffa's (1957, p. vii) project had a particular objective: "By laying bare the main structure of the theory—its assumptions and conclusions, its deficiencies and aspirations—we hope that the book will serve as a useful critical introduction to the theory and a guide to the literature." This quote means their exposition was not a mere textbook repetition of what was most well-accepted in game theory; instead, they wanted a more insightful text, criticizing any pitfalls and suggesting changes. Thus, it should surprise no one that *Games and Decisions* could pose a different view on cores: providing new readings was part of its proposal.[10] Second, Luce (1954, pp. 357-358) sought in his research a "subclass" of "acceptable" imputations of n-person games—that is, an alternative solution concept, named ψ-stability.[11] Thinking of alternatives to the *Theory of Games'* solution was not in vogue around 1957, and while Raiffa was not working on n-person games and solutions, Luce's interest clarify how *Games and Decisions* treated Gillies and Shapley's core an alternative solution. But that text did more than that.

Games and Decisions proposed two novel readings of Gillies and Shapley's core or, to put it in different words, Luce and Raiffa (1957) identified two different "places" it could occupy in game theory. The first use they thought of was to apply Gillies and Shapley's core to substitute a hypothesis of the *Theory of Games* which was far from being indisputable. Remember, n-person cooperative games are made of just a set of players and a characteristic function, saying how much worth each coalition has.

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[8] See Subsection 4.1.1 (starting on page 79) for some information on the researchers of the Hebrew University.

[9] For biographical information on Luce and Raiffa, see Subsection 2.1.2 (starting on page 48).

[10] Most book reviewers made flattering comments about such characteristic, and it contributed to form Morgenstern's (1958, p. 62A) opinion that "It is a unique accomplishment to have produced a book which is at once suited for the novice and yet indispensable for the expert." Another book reviewer, Fels (1960, pp. 165-166) felt Luce and Raiffa's (1957) critical remarks were what *Games and Decisions* had of more valuable.

[11] Luce (1954, p. 358) even identified Shapley's (1953c) work on game values as one among "Several studies in this vein" of seeking alternative solutions, when Shapley (1953a) himself did not present his work in such a way. This is similar to what *Games and Decisions* made of cores; although original authors did not intend to propose new solution concepts, Luce read them as such.

Still, in the *Theory of Games*' analysis it was important to consider the set having all possible payoff distributions a game could entail. Originally, when von Neumann and Morgenstern ([1944] 2007, pp. 263-264) studied n-person games, they considered only payoff distributions satisfying individual and group rationality. To put it in characteristic form language, any player $i \in N$ should earn at least $v(\{i\})$, and summing what all players get should amount to $v(N)$.[12] Payoff distributions satisfying both rationality requirements were called "imputations," and the *Theory of Games* focused on them. Other payoff distributions were simply out of scope.

This simple step of selection payoff distributions might sound innocuous, but it raised a controversy among the *Theory of Games*' readers. For instance, Shapley (1952, p. 3) called such processes as "blocking" (selecting what payoffs are valid for analysis) and "domination" (actually finding a solution) pointing how "In the von Neumann-Morgenstern theory, 'blocking' takes precedence over 'domination', in that it actually prevents the players from considering, let alone accepting," some payoff distributions. Requiring that payoff distributions should be individually rational was not problematic at all; after all, utility maximization backed it. Group rationality was less straightforward. Shapley (1952, p. 3) thought it was not "obvious" that group rationality followed simply as a refinement of individually rationality. As a consequence, it was not clear if post-1944 game theory should dismiss all payoff distributions the *Theory of Games* did. Few years later, Luce and Raiffa (1957) mixed such controversy with Gillies and Shapley's core. The *Theory of Games* "group rationality" concerned a "grand coalition" of all players, N; that is, it demands a certain efficiency from payoffs regarding a sum of what all players get, even those who do not play together in coalitions—it is about $\sum_{i \in N} x_i$, where "$i \in N$" is key. *Games and Decisions* extended such a logic for all other coalitions, calling it "Pareto optimality:" in comparison with "group rationality," it required efficiency for payoff sums for all coalitions; that is, it regarded $\sum_{i \in S} x_i$ for all possible coalitions S, where "$i \in S$" is key. This discussion would drag Gillies and Shapley's core in because it was equivalent to "Pareto optimality."

Luce and Raiffa (1957, pp. 193-194) contended that if payoffs in a coalition S sum less than $v(S)$, surely some of its members could be in a better position (intuitively, S would be wasting its earnings), however, "it is by no means clear that players will be able to reach agreements effecting this." That is, they put Pareto optimality in check. Surely, one could defend Pareto optimality by saying that in such situation any player who could gain more would refuse such distribution (or "block" it, in Shapley's (1952, p. 3) terms). They were not sure if supposing Pareto optimality was adequate because of what it demanded from players, but still, Pareto optimality was much similar to group rationality. Luce and Raiffa (1957, pp. 193-194) suggested that, if group rationality is an

[12] Group rationality means that players earn payoffs as if they joined a "grand coalition" of all players, N. That is, players would not organize themselves in an "inefficient" arrangement of coalitions.

acceptable postulate, there would be no reason not to accept that every coalition should satisfy it as well. Accepting that payoffs should satisfy Pareto optimality would mean that game theory should only study core allocations (and select solutions among them). *Games and Decisions* had no final answers on what should imputations be, but such a discussion led to another rereading of Gillies and Shapley's core: if it was not adequate to make game theory to select solutions among core allocations only, perhaps Gillies and Shapley's core could function as a solution in its own right.

More precisely, *Games and Decisions* spoke of interpreting Gillies and Shapley's core as an "equilibrium" (here, "equilibrium" and "solution" are synonyms).[13] Put simply, Gillies and Shapley's core could work as a solution because it embedded a notion of stability: it only comprised payoffs which subsets of players could not improve. But reframing it as a solution bumped into a problematic property: every constant-sum game has an empty core. This was "The difficulty in setting up the core as the equilibrium definition for characteristic function theory" for Luce and Raiffa (1957, pp. 194-195). If game theory thought of Gillies and Shapley's core as an equilibrium concept, any player i would earn precisely $v(\{i\})$ in constant-sum games, what he gets by going solo. This would have far-reaching implications. The n-person theory of von Neumann and Morgenstern ([1944] 2007, pp. 238-240) was about coalitions; what each player specifically did (that is, which strategy he picked) remained as a background issue. If Gillies and Shapley's core became an equilibrium concept, game theory would not fully amount to a study of coalitions because it would never make sense for players to join them in constant-sum games. Because of that property, *Games and Decisions* only discussed a possibility that Gillies and Shapley's core could work as a solution, without suggesting most researchers already thought along such line and, again, it offered no final answers.

Naturally, Luce and Raiffa's (1957) views do not necessarily reflect how most game theorists approached Gillies and Shapley's core, so analyzing citations could clarify how well spread was such an understanding that cores could "solve" n-person

[13] Luce and Raiffa (1957, p. 199) explained that von Neumann and Morgenstern ([1944] 2007) offered one definition of "solution" for n-person cooperative games, and since it received "primary attention" from researchers, "it was given the name solution." That is, "stable set" and "solution" became synonyms almost organically. While *Games and Decisions* did not precisely define "solution" as an abstract category, its examples unveil what "solving a game" meant. To make it clear, consider a simple equation: $x - 2 = 0$. Here, 2 is a solution because it makes $x - 2 = 0$ a true statement. What is implicit in *Games and Decisions* and its examples is what a solution brings to an n-person game (analogously to "making it a true statement"). Looking at examples of Luce and Raiffa (1957, pp. 199-201), it follows a solution for a game is something pointing "reasonable outcomes" (because solutions reflect rational behavior) and possess some "inner stability" (there is no sufficient reason to deviate from a solution, even if they are many). Now, when Luce and Raiffa (1957, pp. 170-173) presented "equilibrium points" (in discussing Nash's (1951) work), they described them as when players maximize expected utilities in a way that, once at equilibrium, players would not want to change their choices—again, rationality and some form of stability. That is solutions and equilibria were profoundly similar, if not equal.

games.[14] Prior to *Games and Decisions*, no works citing Gillies (1953) mentioned his core concept as an alternative solution concept for game theory; at best, some of them pointed cases in which solutions (in von Neumann and Morgenstern's sense) and cores coincided, so a game's stable set solution would be "precisely the core," as Shapley (1955, p. 4) did, for instance. This type of comment regards primarily mathematical properties, and reading too much into it might be misleading. Even ten years later after Luce and Raiffa (1957) published their book, citations to Gillies's (1953) work would not discuss its merits as a solution; instead, they would cite it mostly focusing on mathematical objects—properties, results and proofs. Yet, a later generation of textbooks from 1968–1970 would bring up Gillies's (1953) as a natural way of solving n-person games—even taking precedence over von Neumann and Morgenstern's ([1944] 2007) stable sets.

6.3 Changing Textbook Narratives

Games and Decisions, which brought up Gillies and Shapley's core already in 1957, was an isolated case: around its year of publication, n-person game theory was not a frequent subject in new publications, and such situation would last for a few years. During 1960–1966, two types of game theory books were dominant: they either regarded two-person game theory and its connections with linear programming (being more "linear programming textbooks" than "game theory textbooks"), or they brought up new ways of thinking game theory, such as Thomas C. Schelling's distinguished books, including *The Strategy of Conflict*. New manuals of game theory including n-person games (in which Gillies and Shapley's core could play some role) would start to appear in 1968–1970. Two authors were more relevant: Anatol C. Rapoport, who published *Two-Person Game Theory: The Essential Ideas* and *N-Person Game Theory: Concepts and Applications*—two parts of a single project—, and Guillermo Owen, who wrote *Game Theory*.[15] Their works show Gillies and Shapley's core acquired a sort of "new role" in game theory as it established itself as a solution concept.

The textbooks of 1968–1970 presented Gillies and Shapley's core as a solution concept for n-person games in its own right, crystallizing changes seen more than a decade before in informal sites and in *Games and Decisions*. As a matter of fact, such textbooks presented *many* alternatives to von Neumann and Morgenstern's stable sets. For instance, both Owen (1968) and Rapoport (1970) discussed, beyond Gillies and Shapley's core, Shapley's (1953c) value and Aumann and Maschler's (1964) bargaining set. Individually, Owen (1968) and Rapoport (1970) covered, respectively, Luce's (1954) ψ-

[14] Here, citations to Gillies's (1953) work came up in searches in JSTOR's and Google Scholar's databases. As Shapley's contributions occurred off the record, it is not possible to track citations to his research.

[15] See Subsection 3.1.1 (starting on page 60) for more information on Rapoport and Owen.

stability and Davis and Maschler's (1965) kernel. This points toward a crucial difference between two- and n-person games. The Minimax Theorem of von Neumann (1928) had always been a sort of "centerpiece" of two-person games; but n-person theory lacked such an element. While stable sets did enjoy some prominence, they were never as incontestable as a solution as von Neumann's (1928) solution for two-person problems. It was far from clear which solution concept, if any, should rule all others. Textbooks reflected such a fact, especially because they would attempt to impose some sort of logical sequence over possible solution concepts. By doing so, they would put Gillies and Shapley's core in a new position.

There was a new reasoning behind Gillies and Shapley's core, which would come before any other solution in textbook presentations. This in itself already poses a reversal: in his dissertation, Gillies (1953, p. 6) devised his (and Shapley's) core as tool for finding stable sets; that is, to use it as-intended, it would be necessary to learn stable sets before. For textbook authors such as Rapoport (1970, p. 89), Gillies and Shapley's core was "a first attempt to single out the payoff vectors that can be expected to serve as possible disbursements among rational players." Similarly, Owen (1968, p. 163) started his quest of solving n-person games with cores because doing it was an "obvious" approach. This naturalness or obviousness derived from how simple Gillies and Shapley's core is: solving a game using it means solving a game applying only individual rationality and Pareto efficiency.

While "naturalness" made Gillies and Shapley's core come up first, textbook authors needed a justification for looking at other solution concepts. The main motivation for doing so was, for Owen (1968, pp. 164-165) and Rapoport (1970, pp. 89-90), that Gillies and Shapley's core could be empty. This deficiency of Gillies and Shapley's core motivated textbooks to present von Neumann and Morgenstern's ([1944] 2007) stable sets not simply as just another solution, but as a response to that fault of Gillies and Shapley's core. For example, Owen (1968, p. 165) stated: "Inasmuch as the core is often empty, it becomes necessary to look for some other type of solution concept. Such a concept is that of *stable sets*."[16] Perhaps what explains how both authors—who come from different backgrounds—provided a very similar account is that what most troubled game theorists around 1968–1970 was finding a general existence result for von Neumann and Morgenstern's ([1944] 2007) stable sets. For almost 20 years, all games practitioners studied admitted at least one stable set solution, so it was natural to expect that eventually an existence theorem would come up. Lucas's (1967, 1968) counter-example would change that but, for now, what matters is that textbooks imprinted a

[16] Similarly, Rapoport (1970, pp. 90-91) said: "It can be proved that all constant-sum N-person games have empty cores. This makes the core unsuitable, at least in the context of constant-sum games, as a 'prescription to rational players,' in the sense of suggesting how they should divide the joint payoff (which is always the same regardless of outcome in a constant-sum game). We must therefore seek other bases for constructing 'solutions' of such games."

sort of common narrative over Gillies and Shapley's core.

This story of why they covered Gillies and Shapley's core, and of why should anyone study stable sets instead of it, manifested itself in textbooks exclusively. Until 1968, no published article discussed Gillies and Shapley's core as an "alternative" to stable sets, or claimed stable sets were a response to Gillies and Shapley's core possible emptiness.[17] This was a textbook phenomenon: it reflected changes in Gillies and Shapley's core that one would not find elsewhere. From just a tool to a solution, and them from a solution to a motivation for studying stable sets, revisions of Gillies and Shapley's core would never become "stabilized" in textbook presentations. That is, while texts of 1968–1970 shared a single story to introduce Gillies and Shapley's core and to move forward to stable sets, later textbooks would display no such homogeneity.

Given how impactful Lucas's (1967, 1968) result was, it would be expected that game theory would reorganize itself. In a survey, Lucas (1971, pp. 516-517) reflected on his research, saying that his counter-example could make practitioners rethink how important existence is for game theory. If they did so, the way in which textbooks presented Gillies and Shapley's core would change. It is difficult to track what happened then, mostly because textbooks similar to Owen's (1968) and Rapoport's (1970) became scarce from 1970 onward. Taking texts of 1985–1988 as a basis for comparison, it seems that game theory did not "converge" to a standard organization. To put it differently, there was a "common" story behind Gillies and Shapley's core in 1968–1970, but its accounts in later textbooks differed from one another. To mention a few examples, Jianhua (1988, p. 117) did not present that possibility of being empty with much gravity: it was a simple and almost inconsequential mathematical property; for Driessen (1988, p. 20), such an emptiness was so grave that his presentation actually focused on a generalization of Gillies and Shapley's core (called "strong ε-core"); and Szép and Forgó (1985, p. 252) went as far as saying that Gillies and Shapley's core could not be a solution concept because it could be empty. These examples illustrate that after 1968–1970, game theory textbooks did not share an unique story to tell about Gillies and Shapley's core.

6.4 What Happened in Economics

The preceding discussion concerns "agnostic" game theory textbooks—they were *not* specifically tailored for economists, having a more "general purpose" intent. Gillies and Shapley's core is still a staple in modern books of that category, but such a statement is not fully true for textbooks oriented toward economists. For instance, while Myerson's (1991) and Osborne and Rubinstein's (2012) expositions covered it, Fudenberg and Tirole's (1991) did not. This is not simply a question that what matters for economics

[17] This argument relies on tracking citations to Gillies's (1953) dissertation using Google Scholar.

are non-cooperative games: in microeconomics texts, Gillies and Shapley's core appears in chapters unrelated to game theory such as, for example, general equilibrium theory. Gillies and Shapley's core experienced a sort of displacement in economics, and such a process has two main explanations. Where Gillies and Shapley's core thrived in economics, it lost its references to game theory; and where it found a truly game theoretic use, it simply did not raise to prominence. Being a pervasive concept, it would be challenging to track what economists made out of Gillies and Shapley's core in all subareas of economics. Two examples will illustrate how modern textbooks present it. The first, of general equilibrium theory, regard how Gillies and Shapley's core "lost its references to game theory."

Agnostic textbook always put Gillies and Shapley's core possible emptiness as a point of primary importance in their presentations. Texts which addressed economists in particular had a different driving force: while existence was certainly important, so it was how well could the core describe and solve economic problems. This difference also manifests itself in how "influential" Gillies and Shapley's core was in game theory and in economics: in game theory alone, it experienced a sort of ascension when it became a solution concept, but practitioners never felt it was good enough in such a position (especially because of its emptiness in all constant-sum games); but in economics, it became a hit, apparently. In a survey for the *Journal of Economic Literature*, Schotter and Schwödiauer (1980, pp. 479-480) supported that Gillies and Shapley's core was essential in "the first renaissance in game theory." That means, economists enjoyed a renewed interest in game theory—even if short-lived—after neglecting the *Theory of Games* for years, and the core played an important role in it. The "vehicle" for that episode, surveyors pointed, was Shubik's (1959a) paper *Edgeworth Market Games*.[18]

In early "agnostic" game theory textbooks being empty was a serious defect of Gillies and Shapley's core, even if it sounded "natural" as a way of solving games. When Owen (1968) and Rapoport (1970) presented it and stable sets, they made it seem that von Neumann and Morgenstern's ([1944] 2007) solution was superior in a certain way. Shubik's (1959a) paper displayed an inverted hierarchy. Truly, he applied both solution concepts in his market game, without making statements on the advantages of one concept against another. But in modeling a market through an n-person game, *Edgeworth Market Games* found that Gillies and Shapley's core shrank as n got large, whereas stable sets did not (in particular, it converged to a competitive equilibrium). Even if Shubik (1959a) did not make comments in such a direction, his readers inferred that Gillies and Shapley's core was relatively more suitable for economic analysis. This is manifest in tracking citations to Shubik's (1959a) paper—subsequent works in economics did not use von Neumann and Morgenstern's ([1944] 2007) solution.

[18] For information on Shubik's paper, check Section 3.2 (starting on page 70).

Considering that Gillies and Shapley's core shrinks to a competitive allocation in market games, Shubik's (1959a) work stimulated research in general equilibrium theory. Throughout 1959–1969, most citations to Shubik's (1959a) paper appeared in journals of economics (such as *Econometrica*, *International Economic Review*, *Journal of Economic Theory*, *Economica*, and *Review of Economic Studies*). This poses a curious situation. Effectively, Shubik's (1959a) spurred research in economics, what was unusual for paper-chapters appearing in the *Contributions to the Theory of Games*: its papers were notably mathematical, being inaccessible to most economists. Regarding general equilibrium theory, those who picked up *Edgeworth Market Games* were, in general, mathematically-inclined authors, who mentioned Shubik's (1959a) article as an inspiration. For instance, Debreu and Scarf (1963, p. 236) found Shubik's (1959a) use of game theory in Edgeworth's model "very stimulating." However, it is hard to defend that such uses of Shubik's (1959a) model would constitute a breakthrough moment (or "renaissance") for game theory.

Papers on general equilibrium that Shubik (1959a) stimulated did not concern strategic problems: as they mostly sought limit theorems, based on economies with many players, strategic considerations lost importance—after all, each individual player was "small." Effectively, such approach reflected a different framing of Gillies and Shapley's core. While for Shubik (1959a, p. 272) "The core of a game consists of the set of imputations which are undominated in all solutions," for Debreu and Scarf (1963, p. 238) "An allocation is in the core if it cannot be recontracted out by any set of consumers" (that is, if they cannot redistribute their commodities improving one consumer's situation without negatively affecting any other). Both formulations followed Edgeworth's (1881, p. 19) notions of "recontracting" and "final settlements," but they have an important distinction. While Debreu and Scarf's (1963, p. 240) analysis of Gillies and Shapley's core ran in terms of individual preferences, Shubik's (1959a, p. 272) original modeling relied on characteristic functions. The uses of Shubik's (1959a) paper in general equilibrium theory had nothing about strategic behavior, and nor did they use games: Gillies and Shapley's core soon lost its "game theory content" in general equilibrium.

Nonetheless, researchers of general equilibrium theory, even if disregarding issues of strategy and coalition formation, still maintained a cooperative *versus* non-cooperative framing of solutions. For example, in his book *Core and Equilibria of a Large Economy*, Hildenbrand (1974, p. 123) perceived the core was a "a cooperative concept," while he interpreted competitive equilibria as "a noncooperative concept."[19] The point of classifying something in game theory as either cooperative or non-cooperative depends on whether players can or can not form coalitions. In such a sense, general equilibrium theory retained some game theoretical content from Gillies and Shapley's core: after all, thinking that players can group themselves to block some allocation speaks to coalition

[19] Werner Hildenbrand is a known character in general equilibrium theory. He also coauthored *Equilibrium Analysis: Variations on Themes by Edgeworth and Walras* with A. P. Kirman, published in 1976.

formation. However, this whole episode would hardly be a "renaissance" of game theory, to use Schotter and Schwödiauer's (1980, pp. 479-480) words. This distinction of cooperative and non-cooperative is too weak of a link and, with time, it would eventually disappear—either because such categorization is not important, or because Gillies and Shapley's core lost its importance in general equilibrium. More than that, saying that absorbing just one concept would represent a "renaissance" would be an exaggeration.

This loosing of "game theory content" explains how come Gillies and Shapley's core appeared in other parts of microeconomics. A second example explaining modern textbooks is industrial organization. In particular, while Gillies and Shapley's core remained fully game theoretical in industrial organization, it did not achieve a high status among economists. This line of research also developed from Shubik's (1959a) insights. This approach was much different from modern (read "non-cooperative") industrial organization: even if it concerned markets with a finite number of agents, its main model was a characteristic function form game. Around 1972–1978, industrial organization was not mainly about games, such a field was slowly incorporating game theory. In special, Telser's (1972) evinces such a fact.[20]

Telser's (1972) textbook reads off as a truly *tour de force* in making Gillies and Shapley's core function as a central element of industrial organization. The book included an array of different models which diferred in small details: models could be of constant returns or else; they could have finite or infinite time horizons; they could use quantity or prices as the policy variable; and so on. Much of Telser's (1972) ran in terms of linear programming— Gillies and Shapley's core can be reframed as solutions to a system of linear inequalities—, but it was still closely related to how game theorists used Gillies and Shapley's core: his model was still a game, and thinking strategically was an integral part of his reasoning. While Telser's (1972) textbook made a "faithful" use of Gillies and Shapley's core, it did not stimulate much further research. Looking at later game theory textbooks oriented toward economists—such as Shubik's (1982), Ichiishi's (1983), and Friedman's (1986)—, it appears that Gillies and Shapley's core did not produce any compelling applications; to illustrate that concept, later textbooks simply resorted to Shubik's (1959a) original model, as if anything that came up later was not as interesting.[21]

Yet, such textbooks from Shubik (1982), Ichiishi (1983), and Friedman (1986) suggest something more. While Gillies and Shapley's core lacked a persuasive application in economics, and while non-cooperative games were possibly already rising in interest, still textbooks addressing economists presented Gillies and Shapley's core (and

[20] Check out Section 4.2 (starting on page 88) for a brief context of industrial organization around 1972 and for a biographical introduction to Telser.

[21] For a fuller appraisal of Telser's (1972) textbook and some complaints economists had against it, revisit Subsection 4.2.2 (starting on page 93).

cooperative games, more broadly). Thus, as far as textbooks go, only very recently, with textbooks from 1986 onward, "non-cooperative game theory" became synonyms with "game theory" for economists.

6.5 Conclusion

The core never really faded away from game theory. It lingers in modern textbooks as "the most important set solution concept for coalitional games," in Maschler, Solan and Zamir's (2013, p. 686) words, or simply as a "a very appealing solution concept," in Myerson's (1991, p. 428) text. Still, some economics-oriented manuals do not cover it—possibly because it belongs to cooperative game theory. Textbooks illuminate what happened to Gillies and Shapley's core. In more mathematical circles, its history confounds itself with that von Neumann and Morgenstern's stable sets. Apparently, it gained a "solution" status when practitioners started to investigate n-person games, but as Gillies and Shapley's core could be empty, they presented it as a natural solution concept, but which underperformed in face of others, such as von Neumann and Morgenstern's stable sets. This way in which textbooks organized game theory would change with W. F. Lucas's counter-example of a game without stable sets. This result called for a reorganization of how textbooks presented solutions of cooperative games, but no agreement emerged: later textbooks did not share a common view on how to present Gillies and Shapley's core and assess its possible emptiness.

Tracking what happened with Gillies and Shapley's core in economics using textbooks also brings up new insights. Texts oriented toward economists do not always cover Gillies and Shapley's core and, when they do, their expositions do not make references to economic applications, save for M. Shubik's *Edgeworth Market Games*. This might come across as surprising given that Gillies and Shapley's core thrived in general equilibrium theory. Most likely, textbooks do not extract an example out of general equilibrium theory because Gillies and Shapley's core lost its connection with game theory in that area of economics, becoming a concept of general equilibrium theory itself. Still, Gillies and Shapley's core found truly game theoretical use in other areas, such as industrial organization, but they did not make it into modern texts. Apparently, Gillies and Shapley's core did not produce results in industrial organization as compelling as Nash's framework did. Lacking a compelling result, the standard illustration of what Gillies and Shapley's core could offer for economists remained being M. Shubik's model of 1959.

7 Conclusion

This dissertation is a historical account of game theory through textbooks. Historiographically, a history based on multiple textbooks brings with it a few challenges. Two are particularly notable. First, its outcome might sound as "too internal," presenting game theory as an isolated subject led by mathematicians, without much reference to other disciplines and how they influenced game theory. However, it is important to emphasize that, for decades since 1944, most researchers developing game theory were trained mathematicians. Being sort of a "dominant group," they imprinted in game theory their mother discipline's logic. In such a sense, a "too internal" story still has its merits for helping one seeing that process more closely. Second, characters in a multi-textbook account tend to be one-dimensional. That is, in keeping in sight many textbooks, it is troublesome to keep following what happened to each textbook author. This surely would not happen in an investigation focusing on a single textbook, but in looking at many texts, published in a large variety of time and places, textbook authors somewhat disappear. A biographical sketch of what they did and what they worked on is helpful in describing their book, but oftentimes, it is not as helpful in understanding batches of textbooks. The timeline of game theory textbooks frequently brings up new characters, being difficult to follow them all. In such a sense, its output reads more as a collection of short stories, whose characters are unrelated, than as a novel with beginning, middle, and end, where one follows characters in a full and cohesive arc.

Despite these challenges, tracking and studying many textbooks yielded interesting practical conclusions. While it is "common knowledge" which modern textbooks are "the best" for graduate training, historians of economics know little about what texts were "the best" before that. This information is far from futile: just as modern texts mold new economists to a certain extent, it is likely that this was also the case in the past. To identify such game theory textbooks, a broad survey was necessary. This research distinguished three important "generations" of textbooks, one much different from the other, demarcating "great changes" in game theory's history. The first change of generations occurred when game theory transitioned from a War-oriented two-person theory into an n-person theory less concerned about practical uses, and more focused on mathematical challenges. The second change happened when researchers found out that not every n-person game admits a von Neumann and Morgenstern solution, in a period in which game theory also spread through several disciplines, including economics. This broad overview of game theory masks some interesting details, so keeping a close look at textbooks presentations of specific concepts—such as Nash's equilibrium and Gillies and Shapley's core—show some new features of their individual histories.

Textbooks show that Nash's equilibrium concept experienced important changes between its publication, in 1950–1951, and that boom of game theory in economics. Such changes happened in a context in which mathematicians dominated most research in game theory. As it is natural, such changes were in conformity with mathematician's disciplinary image, and they explain how come mathematicians did not put much thought into Nash's contribution. And more than that, textbooks also show something about when Nash's equilibrium entered industrial organization: they show that beyond equilibrium existence, non-cooperative games also needed support from other results—such as Folk Theorems—to become attractive for economists. In sum, textbooks have a good deal to say about the Nash equilibrium's history. Things are not so clear for Gillies and Shapley's core. Surely, textbooks add something; for instance, they show that Gillies and Shapley's core also found application in industrial organization at a time in which Nash's contribution was starting to receive some attention from economists. However, textbooks do not explain well how come such approach disappeared. Take together, such stories of Nash's equilibrium and Gillies and Shapley's core illustrate that textbook-based studies might pay off, but to different degrees.

But perhaps what is most interesting in observing specific results about such "generations" of textbooks is that cooperative game theory lingered around for more time than one would expect. An equally interesting finding is that when economics started to absorb game theory, it did not only incorporate Nash's non-cooperative games, but also von Neumann and Morgenstern's cooperative setup. This coexistence of both approaches in economics lasted, it seems, until the mid-1980s, as textbooks kept pushing cooperative games and Gillies and Shapley's core forward. A remnant of such a coexistence still lives in modern game theory textbooks, as some of them still have chapters about cooperative games. It might be natural to assume that recent textbooks are simply "lagging behind," showcasing a game theory which is not precisely what one finds in research. This is not so. Game theory textbooks walked hand-in-hand with what was new in research ever since 1950, when such textbooks started to appear. In light of such, it seems "game theory" and "non-cooperative game theory" became synonyms only *very* recently.

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Appendix

APPENDIX A – The Different Roles of Examples, 1950–1959

Textbooks of 1950–1959 addressed different audiences: Williams (1966) targeted laymen; McKinsey (1952) and Karlin (1959a, 1959b), mathematicians; and Luce and Raiffa (1957) focused on a more in-between audience, being serviceable for mathematicians and social scientists. Their mathematics was naturally different, but contrasts run deeper than having or not a theorem-proof structure. Consider, for a moment, passages where the *Theory of Games* discusses how to solve games, either of two or more players: von Neumann and Morgenstern ([1944] 2007, pp. 143-145, 199-202, 339-340) provided a full chapter of examples of two-person games, establishing connections with real-world problems (such as optimal ways of playing poker), relying on heuristics (using Stone, Paper, Scissors to justify why a theory of mixed strategies makes sense, given players seem to apply mixed strategies in reality), and reflecting about future research (saying it would be important to analyze if games of $n > 5$ would have new "qualitative phenomena," just as three-person games introduced coalition formation). The *Theory of Games* was not just an axiomatic construct of a theory; it involved deviations from a theorem-proof ideal which also appeared in textbooks in efforts of *actually* solving games. To a certain degree, here game theory was more an artistry than mathematics.

Resolving mathematical problems, such as determining solutions of a game, involves more than technical effort, although some heavy mathematics could be necessary. Book reviewers discussed so. Commenting on Karlin's (1959a, 1959b) work, Fleming (1960, p. 270) explained how solving infinite games comprised "the geometry of moment spaces, with positive kernels, and the Neyman-Pearson lemma." His remark stresses a mathematical requirement of solving games; but finding solutions for games also involved a non-mathematical "art," just as it happened with differential equations. More specifically, Contini (1963, p. 166) pushed such an argument by stating: "Solving infinite games is like solving differential equations: a combination of good intuition, adroit use of perturbation arguments, and an ability to exploit the special features of a problem." The most important textbooks of 1950–1959 worked through examples, solving games for their readers. But in doing so, they sliced von Neumann and Morgenstern's ([1944] 2007) approach, differently balancing its rigor, realism, heuristics, and reflections.

In his *Compleat Strategyst*, Williams (1966) adhered to a recipe in each of his chapters. First, he explained basic concepts of game theory—for instance, in Chapter 2, what games, mixed strategies, and other concepts are, besides pointing better and worst ways of playing. Williams always brought home a numerical example, and he discussed

Figure 6 – Three Subsequent Figures from *The Compleat Strategyst*

		Red 1	Red 2
Blue	1	6	5
Blue	2	5	4

		1	2	Row Min
Blue	1	6	5	5*
Blue	2	5	4	4

	Red 1	Red 2
	6	5
	5	4
Col Max	6	5*

Source: Williams (1966, pp. 34-35).

each concept or result *as if* it emerged from studying that particular case. The numerical case conducted the "theory" in "game theory;" formal statements or reasoning were off bounds. To illustrate, consider Figure 6 assembling three figures of *The Compleat Strategyst*, which Williams (1966, pp. 34-36) picked to explain saddle-points—a fancy name for minimax solutions.[1] Reasoning step-by-step, they exemplified how "Blue" could find a saddle-point. Williams (1966, p. 34) explained: "Blue wants to win as much as possible, but dares not be dependent on the largess of Red." He would analyze his options each one at a turn, "to see just how much he can win even though Red, in effect, may be peeking over his shoulder." Blue should simply pick "the best countermove available," which Williams (1966, pp. 34-35) found by calculating for each row (Blue's strategies) its least attainable payoff. In row 1, such a row minima is 5; in row 2, it is 4. By playing 1, Blue secured himself a larger payoff of 5, consequently. Stretching such a reasoning for one page, he concluded:

> This is again a coincidence of the kind encountered in the example The Campers [another example of *The Compleat Strategyst*]: Blue and Red have discovered single strategies which guarantee that the payoff will be some unique number—5, in this case—against an inspired adversary; and each knows that the payoff will be more favorable (than 5) to him if the enemy is not inspired. This is the situation called *saddle-point*.

[1] The notion of saddle-point might require an explanation as it is not part of what one economist learns in his classes of game theory nowadays. Consider two strategy spaces, S_1 and S_2, associated with players 1 and 2. The payoff function of such a game is a real-valued function $u : S_1 \times S_2 \to \mathbb{R}$. A pair o strategies (s_1^*, s_2^*) is a saddle point if

$$\forall s_1 \in S_1 \quad u(s_1^*, s_2^*) \geq u(s_1, s_2^*)$$

and

$$\forall s_2 \in S_2 \quad u(s_1^*, s_2^*) \leq u(s_1^*, s_2)$$

This notion is useful in two-person zero-sum game theory because a necessary and sufficient condition for a game to have a minimax solution is that u should have a saddle-point—in particular, such a minimax solution corresponds precisely to such saddle-point. This is why sometimes "saddle-point" and "minimax solution" might be interchangeable expressions. For further details, check Maschler, Solan and Zamir's (2013, pp. 117-118) exposition.

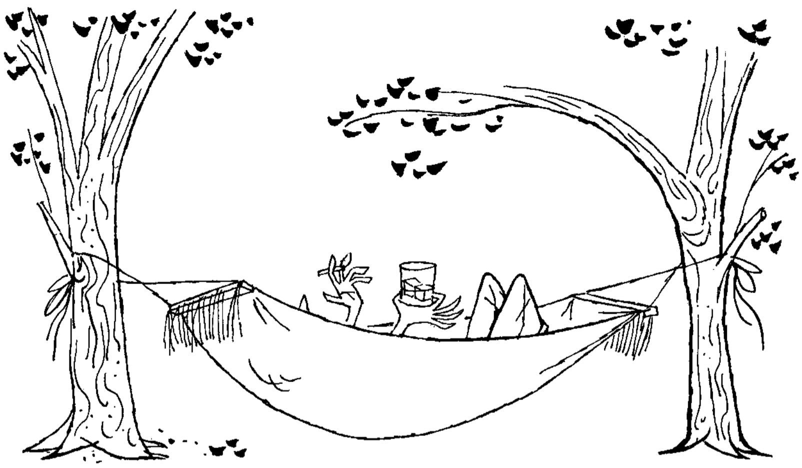
> Generally, *when the larger of the row minima is equal to the smaller of the column maxima, the game is said to have a saddle-point*; and the players should stick to the strategies which intersect at the saddle-point. (WILLIAMS, 1966, p. 35)

Hence, "the situation called *saddle-point*" emerged from combining deductive reasoning (informed by heuristic considerations of how it would be best for players to behave) and numerical case studies. This approach is a staple in every of *The Compleat Strategyst*'s chapters. Concerning saddle-points still, Williams's (1966) avoided what von Neumann and Morgenstern ([1944] 2007, p. 88) called "the symbolism of the functional calculus"—there was no need two-variable functions and their optimization in Williams's (1966) exposition: he balanced out any formalism of the *Theory of Games*, emphasizing almost a trial-and-error approach even laymen could follow. However, if in such dimension Williams (1966) subtracted from the *Theory of Games*, in another he added to it. In his chapter recipe, after explaining a given concept, he moves to life-motivated examples, situations in which games are supposedly useful. These refereed not to parlor games, which inspired many discussions in von Neumann and Morgenstern's ([1944] 2007) original presentation. Instead, they sought to game-theoretically describe situations anyone could face. There are 35 of such examples in *The Compleat Strategyst*; they are a distinguishing feature of Williams's (1966) book. To illustrate, consider "The Coal Problem," a two-player, three-strategy game, whose description follows bellow. After depicting his hypothetical yet realistic situation, Williams (1966, p. 101) reformulated it as a strategic form game, calculating "Row Min" and "Col Max," comparing such values and concluding such a "game has a saddle-point, corresponding to the 20-ton stockpile."

> On a sultry summer afternoon, Hans' wandering mind alights upon the winter coal problem. It takes about 15 tons to heat his house during a normal winter, but he has observed extremes when as little as 10 tons and as much as 20 were used. He also recalls that the price per ton seems to fluctuate with the weather, being $10, $15, and $20 a ton during mild, normal, and severe winters. He can buy now, however, at $10 a ton.
>
> What to do? Should he buy all, or part, of his supply now? He may move to California in the spring, and he cannot take excess coal with him. He views all long-range weather forecasters, including ground hogs, dimly.
>
> He considers three pure strategies, namely, to buy 10, 15, or 20 tons now and the rest, if any, later. [. . .] (WILLIAMS, 1966, pp. 100-101)

This example is more of a problem of decision-making under uncertainty, but Williams (1966, p. 101) framed it as a game against nature (in particular, against a player named "Winter"). These real-life examples bridged a more abstract, slow paced reasoning—such as a game between "Blue" and "Red"—and applications of game theory. For Gilbert (1966, p. 549), one of Williams's (1966) reviewers, they actually demonstrated

Figure 7 – C. Satterfield's Illustration for "The Coal Problem"



Source: Williams (1966, p. 100).

how hard it was to integrate game theory and applied problems. To sustain his argument, Gilbert (1966, p. 549) cited a specific example in which Williams (1966, pp. 74-76) analyzes a Russian Roulette played for a pack of cigarettes—an exceedingly deadly game for a mild payoff. To complicate Williams's (1966) case, for that game it was optimal to pull the trigger every turn; it was a "ridiculous" problem, in Gilbert's (1966, p. 549) analysis. Vajda (1956a, p. 223) agreed, calling *The Compleat Strategyst*'s examples "subtly absurd." Regardless, Vajda (1956a, p. 224) argued, one should not fall for Williams's "facetious style:" his book contained "sound theory." Altogether nonsensical, examples were simple, workable by laymen. Curiously, the *Compleat Strategyst*'s examples accompany illustrations by Charles Satterfield. Figure 7 reproduces "The Coal Problem" companion figure. Such pair of example-figure captures what game theory was in reading *The Compleat Strategyst*: solving games was a simple (and perhaps fun) exercise, one which you could do while laying in a hammock while having a cold drink. Book reviewers thought so. Barber (1954, p. 453) argued Williams guided readers "smoothly and painlessly" through game theory; Morgenstern (1954, p. 913) felt two-person games found in Williams' hands an "unsurpassed popular expression." While popular expression could disseminate game theory-informed strategical thinking, it was not adequate for those seeking to contribute to von Neumann and Morgenstern's ([1944] 2007) new mathematical field. To do game theory, more mathematical sophistication was necessary.

Differently from Williams's (1966) textbook, McKinsey's (1952) and Karlin's (1959a, 1959b) have a theorem-and-proof structure, where lemmas, propositions, and theorems work as building blocks of a larger theoretical body. The complexity brought with a formal treatment of games should not be minimized. As Lindley (1953, p. 206) highlighted, McKinsey's (1952, pp. 31-37) proof of "the fundamental theorem" (of minimax solution existence for two-person zero-sum games) ranged through seven pages. Because McKinsey (1952) emphasized mathematical aspects of game theory, Bross (1953, p. 655) argued, his first eight chapters were at a level suited for someone who already held a bachelor degree in mathematics. Later chapters were even more sophisticated technically-wise. Difficulty of readership motivated another reviewer, Gray (1953, p. 55), to ponder over who could benefit from reading McKinsey's (1952) text; he concluded someone who graduated on mathematics could use it to introduce himself to what was already done in game theory. As McKinsey (1952), Karlin's (1959a, 1959b) books also aimed at mathematicians—and their way of solving games shows so.

As seen, Williams's (1966) approach in explaining saddle-points was majorly heuristic: from a concrete, numerical case, he built a definition for saddle-points based on intuitive reasoning. In contrast, Karlin (1959a, pp. 21, 28) described saddle-points without support from numerical examples, simply saying that "Player I, seeking to maximize his pay-off or yield, naturally chooses his strategy $\boldsymbol{x_0}$ so that $K(\boldsymbol{x_0}, \boldsymbol{y_0}) = \max_x K(\boldsymbol{x}, \boldsymbol{y_0})$;" when optimal strategies exists in such a sense, "the [payoff] matrix $\boldsymbol{A}$ is said to have a *saddle point* [. . . which] is an element of a matrix which is both a minimum of its row and a maximum of its column." Saddle-points, unlike in Williams's (1966) exposition, did not emerge from a concrete study; in Karlin's (1959a) presentation, saddle-points just *were*. Examples played reversed roles. In Karlin's (1959a) book (or even McKinsey's (1952)), numerical cases predominantly illustrated what a mathematical object is; that is, readers can make sense of them once they absorbed a mathematical concept.[2] Instead, in Williams's (1966) presentation examples are an integral component of reasoning and without them, there is no concept construction.

The main point here is that Karlin (1959a, 1959b) wrote a more formal book than Williams (1966) did. Neither McKinsey's (1952) nor Karlin's (1959a, 1959b) texts were purely terse, as they did allow for relatively more heuristic passages. For instance, McKinsey (1952, pp. 6-7) defined strategic-form games (or "rectangular games," as he called them) from a numerical case, a simple game in which two players chose among a finite set of natural numbers, and each received numerically-determined payoffs—that is, by an example, not by a formal definition. What matters in comparing Williams's (1966) presentation on one end, and McKinsey's (1952) and Karlin's (1959a, 1959b) on another, is that these more rigorous textbooks actually emulated how research papers presented game theory (as it is observable from, for example, Karlin's eleven papers spread in the *Contributions to the Theory of Games* series). By balancing out heuristics and realness in favor of formalism, McKinsey (1952) and Karlin (1959a, 1959b) adapted the *Theory of Game* for different purposes than those of Williams (1966), who sought a popular expression of game theory; instead, they presented game theory in a way suitable for a trained mathematician start pursuing to do research in games.

[2] Comments to Karlin's (1959a, pp. 21, 28) exposition also applies to McKinsey's (1952, pp. 8-11).

To make sense of such difference, it is helpful to note that Luce and Raiffa (1957) had a third approach. Truly, they highlighted theorems, but not as McKinsey (1952) and Karlin (1959a, 1959b) did: proofs were almost nonexistent—they were not demonstrating theorems, but frequently they justified them using logial reasoning. Instead of accentuating key statements of game theory and rigorously demonstrating them, Luce and Raiffa (1957) posed questions and existing answers covered by research in game theory. Their presentation was not just about what was most solid in game theory, but included discussions concerning its open endings. They weighted more heavily heuristics and reflections, leaving less room for reality and rigor. To illustrate, in discussing "equilibrium pairs" of two-person zero-sum games, Luce and Raiffa (1957, pp. 65-68) advanced and (verbally) discussed five questions, using examples, pointing difficulties in providing definitive answers, and qualifying possible ways to respond to them. Just for reference, they questioned:

> i. *Do all strictly competitive games have equilibrium pairs?*
>
> [. . .]
>
> ii. *If a game has an equilibrium pair, is this pair necessarily unique, or may there be several equilibrium pairs?*
>
> [. . .]
>
> iii. *Does the existence of several equilibrium pairs in a game cause any difficulty in the sense of creating a conflict of interest among them?*
>
> [. . .]
>
> iv. *Does an equilibrium strategy maximize a Player's security level?*
>
> [. . .]
>
> v. *If a strategy maximizes a player's security level, is it an equilibrium strategy?* (LUCE; RAIFFA, 1957, pp. 65-67)

As much as McKinsey (1952) and Karlin (1959a, 1959b) emulated research papers, Luce and Raiffa (1957) copycatted survey papers. This distinction helps to understand how come Luce and Raiffa (1957) went as far as to discuss Nash's (1951) equilibrium point concept, a possible answer to solve n-person games, while McKinsey (1952) spent nearly four fifths of his text in two-person zero-sum games. Game theory textbooks of 1950–1957 rebalanced the *Theory of Games* differently, providing more or less mathematical rigor, real-life applications, heuristic reasoning, or reflections on problems game theory had in varying proportions. This effectively amounts to slicing von Neumann and Morgenstern's ([1944] 2007) text accordingly to what each audience needed. The *Theory of Games'* book reviews agreed it was a difficult text to go through cover to cover. For a curious example, Nagel (1945, p. 551) suggested philosophers should read just its beginning; there was nothing more to them beyond page 200 (through 625). Textbooks, instead of suggesting page ranges, actively cut and reshaped the *Theory of Games*.

APPENDIX B – Linear Programming Textbooks, 1960–1965

After 1950–1959, a new generation of game theory textbooks would come only in 1966–1970, with Owen's (1968) and Rapoport's (1966, 1970) works. The lapse between 1960 and 1965 had a number of other books and textbooks which contained something of game theory, but were not precisely about just it, as documented in Table 8. Their titles display two types of publication were dominant: new volumes were either about linear programming and game theory (mostly textbooks), or they were about new ways of thinking about game theory (not textbooks). This last category comprises books from Anatol Rapoport and Thomas C. Schelling, being particularly prominent *Fights, Games, and Debates* and *The Strategy of Conflict*. Although such books were influential—Schelling's text was partly responsible for his Nobel prize alongside Robert J. Aumann, awarded in 2005—, they were not as relevant in shaping game theory textbooks. The same comment applies to linear programming books; while new titles continuously appeared throughout decades, they remained unchanged and cannot explain substantial changes going on with purely game theory textbooks.

Linear programming refers to optimization techniques concerning problems of linear objective functions and constraints, be them equalities or inequalities. In 1960, John R. Hicks, famous for his indifference curve approach, published a survey about it in the *Economic Journal*. This survey, named *Linear Theory*, comprised more than that: beyond linear programming, it covered activity analysis, input-output systems, and game theory. For Hicks (1960, pp. 671-672), all such recently developed techniques were closely related to one another and offered a "restatement" of "things of which we [economists] were (more or less) aware, but which one can now realise that we were putting rather badly." His survey suggested economists had something to learn with linear programming, even if they did not desire to become users of such tools. Hicks (1960, p. 672) himself, whose education in mathematics ended in 1923, admitted being somewhat discomfortable speaking about new mathematical methods. The survey in the *Economic Journal* reflected a certain success programming was achieving among mathematical economists. A first example of linear programming's dissemination is a Cowles Commission Monograph Tjalling C. Koopmans edited and published in 1951, named *Activity Analysis of Production and Allocation*, and a book written by Robert Dorfman, Paul A. Samuelson, and Robert M. Solow, published in 1958, *Linear Programming and Economic Analysis*. In particular, a quick rundown of Dorfman, Samuelson, and Solow's book shows a wide range of applications of linear programming in economics: transportation problems, firm

Table 8 – List of Game Theory Books, 1960–1965

Author	Title	Year	Rev.	Textb.
A. Rapoport	*Fights, Games, and Debates*	1960	16	
E. G. Bennion	*Elementary Mathematics of Linear Programming and Game Theory*	1960	4	✓
S. Vajda	*Introduction to Linear Programming and the Theory of Games*	1960	9	✓
T. Schelling	*The Strategy of Conflict*	1960	19	
D. Gale	*The Theory of Linear Economic Models*	1960	9	✓
P. Suppes and R. C. Atkinson	*Markov Learning Models for Multiperson Interactions*	1960	7	
M. Dresher	*Games of Strategy: Theory and Applications*	1961	0	✓
E. S. Ventzel	*Lectures on Game Theory*	1961	4	✓
A. M. Glicksman	*An Introduction to Linear Programming and the Theory of Games*	1963	4	✓
E. Burger	*Introduction to the Theory of Games*	1963	4	✓
E. S. Ventzel	*An Introduction to the Theory of Games*	1963	0	✓
A. Rapoport	*Strategy and Conscience*	1964	8	
M. Shubik	*Game Theory and Related Approaches to Social Behavior: Selections*	1964	9	
M. Dresher, L. S. Shapley, and A. W. Tucker (Eds.)	*Advances in Game Theory*	1964	2	
J. Talacko	*Introduction to Linear Programming and Games of Strategy*	1965	0	✓
R. Isaacs	*Differential Games: A Mathematical Theory With Applications to Warfare and Pursuit, Control And*	1965	5	
C. Berge and A. Ghouila-Houri	*Programming, Games and Transportation Networks*	1965	4	✓
A. Rapoport and A. M. Chammah	*Prisoner's Dilemma: A Study in Conflict and Cooperation*	1965	2	

Source: Each listed book appears in library catalogs from either the University of São Paulo, Duke University, or the Library of Congress. The number of book reviews comes from JSTOR's database and Google Scholar searches.

theory, capital accumulation, general equilibrium, welfare economics, and—what mostly matters here—game theory.

A mathematical relationship bounds together game theory and linear programming. In *Activity Analysis of Production and Allocation*, Dantzig (1951) formally demonstrated how solving any given two-person zero-sum game is equivalent to work out a linear programming problem. The history behind such a result is well-known, as Dantzig himself provided an account of it (see Dantzitg and Thapa (1997, pp. xxvi-xxvii)). On October 3, 1947, he met von Neumann at the Institute for Advanced Study of Princeton University, hoping von Neumann would help him solve a war-related problem he was struggling with. After briefly explaining his issue, von Neumann gave him a lecture on linear programs. More than that, von Neumann conjectured an equivalence relationship between two-person zero-sum games and linear programming. This relationship is what Dantzig (1951) proved later (already in 1948 actually, in an unpublished text). Noteworthy here, such an equivalence relation justified uniting two-person zero-sum game theory and linear programming theory in textbooks—especially considering how the Cold War made put such class of games in trend.

A quick inspection of some of such linear programming-and-game theory textbooks, dated of 1960–1965, shows how unimportant they are in linking textbooks of 1950–1959 and of 1966–1970. For starters, note that two textbook authors mentioned in Table 8—S. Vajda and D. Gale—were already publishing materials on linear programming before (respectively, in 1956 and 1957; see Table 3). David Gale is not a stranger's name among economists: trained as a mathematician, receiving his PhD from Princeton University in 1949, he quickly moved his interests to linear programming and game theory (circa 1953), but possibly became known because of his work with Shapley, *College Admissions and the Stability of Marriage*, published in 1962 (see Gale and Shapley (1962)). In turn, S. Vajda was a Hungarian mathematician who, because of the Nazis, ended up in the United Kingdom, and there he worked on linear programming and operations research. While coming from substantially different social contexts, both transformed lecture they gave into textbooks.[1]

More specifically, Gale (1960, p. vi) wrote his book after "a set of notes from a course given to a group of graduate students in pure and applied mathematics" at Brown University during 1956–1957; hence, he suspected, "the average graduate

[1] Gale's (1957) book reads as a draft for his later book, *The Theory of Linear Economic Models*, of 1960. Both volumes do not show significant differences. In turn, Vajda (1956b) had already published a small volume before his textbook—a booklet of 100 pages not counting post-textual elements, printed in pages of around 15 × 10 cm. His presentation intertwined game theory and linear programming, as his contents page shows. For reference, Vajda's *The Theory of Games and Linear Programming* chapters were, in order: "An Outline of the Theory of Games," "Graphical Representation," "Algebra of the Theory of Games," "An Outline of Linear Programming," "Graphical Representation of L.P. (1)," "Algebra od the Simplex Method," "Degeneracy and Other Complications," "Duality," "The Solution of Games," "Graphical Representation of L.P. (2)," and "The Method of Leading Variables."

student in economics would have some difficulty going through the book on his own." This should not downplay its significance for economics students, Gale (1960, p. vi) contended: "Nevertheless, the theorems we prove are about economics, are used by economists, and in many cases where first discovered by economists." Vajda (1960, p. 7) also wrote his book after "courses of ten to twelve lectures, given to several audiences." Game theory and linear programming were "two mathematical techniques which are fundamental to it," Vajda (1960, p. 7) said in explaining his content choices. These two volumes, from Gale and Vajda, had different takes on game theory, as one would expect given their dissimilar origin. When Gale switched his presentation focus from linear programming to game theory, he stated:

> The remaining chapters of this book will be concerned mainly with models which are not of the simple optimization type, and the reader must at this point prepare himself for a definite change in the nature of our discussions. In the treatment of linear programming we presented a fairly definitive theory. The problems discussed made obvious sense. The theory of the problems was quite thoroughly developed, one even had efficient methods for finding solutions, and most important of all, it was clear from the outset exactly what questions had to be answered. <u>In nonoptimization situations, on the other hand, the first and usually the most difficult problem is to decide what questions to ask. Much of the remainder of this book will be concerned with some of the answers which have been given to this problem in various sorts of situations.</u> These answers will necessarily be more tentative than those given for simple maximum and minimum problems, and the reader must not expect to be presented with polished and complete theories. The subject of "multi-objective" models is still very young, and the best we can do at this point is to present the reader with some of the more promising attacks which have been made on it in some special cases. (GALE, 1960, pp. 189-181; added underline)

Transitioning from linear programming to game theory could lead to different paths, such quote suggests. Indeed, chapters about applications of game theory in Gale's (1960) and Vajda's (1960) were dissimilar, especially regarding how much economics they had. For example, Gale's (1960) last two chapters—"Linear Models of Exchange" and "Linear Models of Production"—concerned problems in economics such as von Neumann's (1937) general equilibrium model. Oppositely, Vajda (1960, pp. 7-8) relied on oversimplified and schematic examples only because "they serve their purpose better than elaborate case studies could do." In such a sense, his books reads as a short, hands-on manual on how to solve a game—either graphically, algebraically, or making it a linear programming problem (so G. B. Dantzig's simplex method would be employable). There was no space for economic models (or any other disciplinary crossover). Other linear programming books also sough connections with economics, as Gale (1960). Bennion (1960, pp. 126-127), for instance, decided to follow Dorfman, Samuelson and Solow (1987) in taking general equilibrium theory as "the most enlightening example (for our purpose) of the relationship between linear programming and economic analysis." This meant discussing what he called "the Walras-Cassel" model. Also, Kaufmann (1967, p. 169) had a section in his game theory exposition named "Use in Concrete Cases of Competition," in which he discussed a series of games referring to two rival companies. Regardless of how diverse applications could be, however, textbook authors portrayed game theory *per se* rather homogeneously.

Although linear programming books included game theory, von Neumann and Morgenstern's ([1944] 2007) field did not play a protagonist role; linear programming did. Suggestively, all such books presented linear programming before they presented game theory. The origin of such secondary role is related to applications, as textbook authors were verbal about what was wrong with game theory: it was unable to produce new applications linear programming could not handle. Put it differently, once one learned linear programming, he could tackle two-person zero-sum game problems without reference to game theory's techniques. And in situations in which linear programming was not suitable—more than two-person, or non-zero-sum games—game theory also was faulty for being overly complex. To illustrate, when picking "very simplified" game examples for his book, Kaufmann (1967, pp. 169-170) explained he would limit himself in "explaining how a certain form of reasoning can be used;" if he sought "cases which are more general and closer to reality," his reader would learn "the theory of games of strategy for the purpose of obtaining optimal strategies has often proved difficult, and even impossible." Bennion (1960) also shared his thoughts about economic applications of game theory:

> There is little disagreement as to the importance of game theory techniques to economic analysis. These techniques have some indirect value in those cases where there is something to be gained by turning a linear programming problem into a game for solution purposes. Beyond this there is, at least currently, not too much to be said for game theory techniques as an economic tool, and for fairly obvious reasons.
>
> [. . .] Unfortunately, few problems in economics take the form of zero-sum two-person games. Rather, such problems tend to run to *non*-zero-sum two-person games. [...] But the current state of game theory in these more complicated games is far from satisfactory. Bennion (1960, p. 135; added underline)

This difficulty in handling games other than two-person zero-sum explains a critical characteristic of how linear programming books of 1960–1965 addressed game theory, which justifies why they are not central in understanding later texts, such as Owen's (1968) and Rapoport's (1966, 1970). Authors of 1960–1965 presented game theory within boundaries, limiting themselves to two-person zero-sum games. That is, in such books game theory *was* two-person zero-sum game theory. There is one exception: Vajda (1960, pp. 65-67) distinguished himself for writing two and a half pages on "non-zero-sum" and "more-person" games. He rapidly stated how non-zero-sum games made it unreasonable to think of conflict in terms of maximizing and minimizing payoffs, as von Neumann's (1928) did. Citing Nash (1951), he mentioned reasoning in terms of equilibrium points—in which every player maximizes his own payoffs—could be a "more interesting" approach, but he delved no further into it. Importantly here, any surge in publishing linear programming books did not imply fresh presentations of game theory. They emphasized two-person zero-sum games as did earlier books, as McKinsey's (1952) and Karlin's (1959a, 1959b), neglecting new possibilities published for non-zero-sum games.

In perspective, linear programming textbooks including game theory appeared continuously through time: a more recent example of such "type" is Brickman's (1989) text, which exemplified how such kind of material remained frozen in time—there is not many differences between it and other texts of 1960–1969. Truly, Dantzig's (1951) equivalence result made game theory more applicable, and it should come as no surprise internal discussion papers of RAND Corporation include many inquiries on linear programming. But mathematicians working in game theory were aware that, beyond Cold War problems, two-person zero-sum game theory was not precisely fruitful. Economic problems, for instance, are often of a non-zero-sum nature. The link with linear programming had its relevance for that game theory developed at places such as RAND, but it does not explain later textbooks of 1966–1970, whose focus was n-person games, for which linear programming could add nothing. Consequently, these linear programming books of 1960–1965 are not critical in explaining how modern game theory textbooks emerged; they are just distant relatives.

APPENDIX C – Rapoport's and Schelling's Books

Two authors in Table 8, Anatol Rapoport and Thomas C. Schelling, published books pushing game theory's boundaries; or, to put it differently, suggesting changes. Specifically, Rapoport published *Fights, Games, and Debates*, whereas Schelling wrote *The Strategy of Conflict*.[1] These books were about new ideas, meaning Rapoport (1960) and Schelling ([1960] 1980) were not expecting to *teach* game theory to anyone, but to *develop* it. Morgenstern (1961b, p. 103), who jointly reviewed both volumes, thought what connected them was "their concerns for human conflicts and the attempt to find methods for analyzing and resolving them;" and mentioned they offered game theory "various comments and suggestions." This does not mean they did not explain game theory. Someone unacquainted could still learn something about games by reading either *Fights, Games and Debates* or *The Strategy of Conflict*. To illustrate, Wegner (1963, 425), who reviewed Rapoport's (1960) book, thought it "introduces with great lucidity the standard concepts of game theory, such as move strategy, mixed strategy, outcome (payoff) and solution." Yet, it would be hard to categorize them as textbooks.[2]

Schelling's ([1960] 1980) *The Strategy of Conflict* became particularly influential through time—he became a Distinguished Fellow of the American Economic Association, and textbook author Myerson (2009, p. 1109) ranked Schelling's work as "a masterpiece that should be recognized as one of the most important and influential books in social theory."[3] Regardless of Schelling's ([1960] 1980) later effect on economists, his *The Strategy of Conflict* was not part of textbook expositions of game theory; neither Owen (1968), Rapoport (1970), Telser (1972, 1978), nor Friedman (1977) mentioned his name or works. The first modern textbook mentioning Schelling ([1960] 1980) was that of Tirole (1988, pp. 276, 458). Differently than *The Strategy of Conflict*, Rapoport's (1960) book never attained some influence. Both texts, regardless of how sound their criticism was, materialized little to no change at their time. However, their criticism says something about how game theory grew from circa 1960 until recently.

The relationship of Schelling with game theory dates back to RAND Corporation

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1 A. Rapoport published other such books, such as *Strategy and Conscience*. The focus here is on his *Fights, Games, and Debates*, however.

2 As Schelling ([1960] 1980, p. vii) himself declared, his book contained reprints of previously published papers, what further marks it as a non-textbook. These appeared in the *American Economic Review*, the *Journal of Conflict Resolution*, the *Review of Economics and Statistics*, and in a book Klaus Knorr edited, named *NATO and American Security*.

3 See Zeckhauser's (1989) paper, which brought reflections on T. C. Schelling's work in light of his becoming a Distinguished Fellow.

and the Center for International Affairs, both of which he joined in 1956 and 1958, respectively.[4] In his Nobel autobiography, Schelling associated his 1960 book with his work at RAND and Harvard, mostly. As Schelling (2005) recollects, he learned game theory from Luce and Raiffa's (1957) book: "it was my professional introduction to game theory, and I spent at least a hundred, maybe two hundred, hours with it." At RAND, Schelling (1960, p. vi) received "stimulation, provocation, advice, comment, disagreement, encouragement, and education." While RAND produced an "powerful and persistent" intellectual impact on him, Schelling (1960, p. vi) argued RAND was "not responsible for the shape" his "ideas have taken." This is expressed in his book, as its chapters effectively reprinted his previously published research, only differing from it by minor changes to integrate them into a single volume. Also in his autobiography, Schelling commented on his 62-page piece *Prospectus for a Reorientation of Game Theory*, which would turn into chapters 4 through 6 in *The Strategy of Conflict*:

> In the spring and summer of 1958 I took my family to London, where I pursued what I considered my concept of game theory in a manuscript—typed by the woman on Charing Cross Road who did all of Agatha Christie's books and plays—and submitted it to the Journal of Conflict Resolution. It was so long that that Journal decided to make it a whole issue. I persuaded the editor that a smart way to publicize the new journal would be to give me, without charge, instead of reprints three hundred copies of the journal to send to everyone I could think of. I called my article, "Prospectus for a Reorientation of Game Theory." I was trying to get some theorists to pay more attention to strategic activities, things like promises and threats, tacit bargaining, the role of communication, the design of enforceable contracts and rules, the use of agents, and all the tactics by which individuals or firms or governments committed themselves credibly. I don't think I had any noticeable influence on game theorists, but I did read sociologists, political scientists, and some economists. (SCHELLING, 2005, no page numbering; added underline)

This quote captures much of Rapoport's (1960) and Schelling's ([1960] 1980) intentions: they wanted to make game theorists pay attention to specific problems, and both called for such an attention by criticizing game theory. A short account of their challenge follows. In *Fights, Games, and Debates*, Rapoport (1960, p. vii) addressed "any serious student of human conflict on the intrapersonal, interpersonal, organizational, social, or international level," dividing his text in three parts. In his text, "fights" referred to behavioral models based on differential equations: as if people reacted mechanically to any events, and not strategically.[5] This approach was naturally much different from that of game theory, and Rapoport (1960, p. 359) discussed both because, he thought, it would

[4] Schelling obtained his Ph.D. from Harvard in 1948 but, before joining Yale University in 1953, he assumed a series of governmental jobs. This step in his career was not surprising as his early papers and book reviews from 1946–1956 concerned macroeconomic development and policy.

[5] Put briefly, "fights" referred to Lewis F. Richardson's arms races models as systems of differential equations—they may embed variables out of one's hands, but any adjustment to them is automatic, not strategic.

make come to light what deficiencies they had.[6] Turning to Schelling's ([1960] 1980) work, *The Strategy of Conflict* was about establishing an interdisciplinary field—whose label could be "theory of bargaining," "theory of conflict," or "theory of strategy"—, Schelling ([1960] 1980, pp. v-vi) suggested; in his words, he wanted to show "that some elementary theory, cutting across economics, sociology and political science, even law and philosophy and perhaps anthropology, could be useful not only to formal theorists but also to people concerned with practical problems."[7] In his first edition preface, Schelling ([1960] 1980, p. v) said his book's subject fell "within the *theory of games*, but within the part of game theory in which the least satisfactory progress has been made." In sum, both book authors had some trouble or another with game theory—and they all proceeded from a common source.

The problem came down to who was making game theory. Rapoport (1960, p. 226) stated it "was conceived by mathematicians" and had been "developed almost exclusively by mathematicians." This dominance implied a particular disciplinary view which emphasized a prescriptive nature—saying what a theoretical player should do—, lacking in descriptive power—not describing what players actually do. This is where *Fights, Games, and Debates* challenged game theory. For Rapoport (1960, p. 227), its general failure "can be traced to the exclusion of certain psychological concepts from its axiomatic base." This is why, he thought, deviations from two-person zero-sum games frequently ended in ambiguity and paradox. The situation was notably worse for n-person games, Rapoport (1960, p. 227) concluded, as "the theory cannot predict what will happen if everyone 'does his best.'" While Rapoport (1960, p. 227) stressed problems in moving from 2- to n-person problems, Schelling's ([1960] 1980, p. 83) directed his criticism toward zero-sum restrictions which, again, was a question of who did game theory and how.

The Strategy of Conflict primal criticism of game theory concerned how mathematicians excessively focused on zero-sum situations. In all fairness, Schelling ([1960] 1980, p. 83) acknowledged zero-sum games "had yielded important insight and advice." However, he could not say so of non-zero-sum games, which mattered, for instance, "in wars and threats of war, strikes, negotiations, criminal deterrence, class war, race war, price war, and blackmail; maneuvering in a bureaucracy or in a traffic jam; and coercion of one's own children." Schelling ([1960] 1980, p. 83) aspired "to enlarge the scope of game theory, taking the zero-sum game to be a limiting case rather than a point of departure." For starters, he reasoned, while zero-sum games were a case of extreme competition, it was worth exploring a diametrically opposed situation of

[6] Part III of Rapoport's (1960) book, about "debates," concerns an attempt to develop a new non-mathematical model of cooperation in which agents pursue a common interest using persuasion, in response to failures he saw in game theory and that approach of differential equations.

[7] The *Journal of Conflict Resolution*, established in 1957, would be central for such a field, Schelling ([1960] 1980, vi) stated.

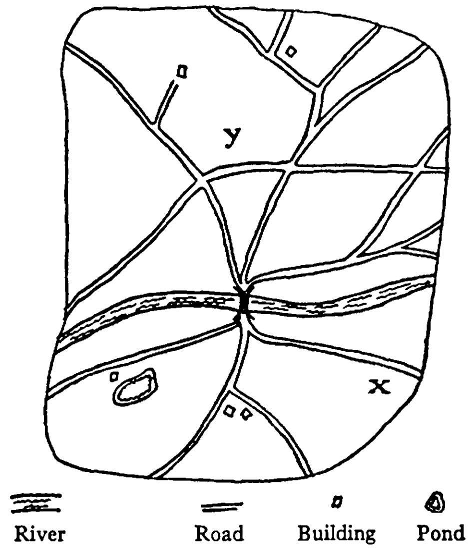
"pure-collaboration," in which players had perfectly correlated interests. These were not as trivial as they might initially sound, Schelling ([1960] 1980, pp. 84-85) pointed, because players might have trouble in understanding each other; a relatable example he mentioned of it concerns "two people dancing together to unfamiliar music." Although they have identical preferences—they both want to have a good time—, they do not behave as a single individual and, consequentially, feet could get hurt. All in all, *The Strategy of Conflict* proposed to discuss games falling between such cases of pure competition and collaboration.

Schelling ([1960] 1980, pp. 88-89) called such in-between games as "mixed-motive game" or "bargaining situation" for a lack of better terms, since players were neither opponents nor partners. What matters here is how dividing games between zero-sum and non-zero-sum—the classic mathematical division when two-person games were in trend—was insufficient to successfully model conflict situations (a non-zero-sum game could be either of pure coordination or of mixed-motive). To reorient game theory, *The Strategy of Conflict* sought insight on pure collaboration games. Drawing on experimental data, Schelling ([1960] 1980, pp. 55-57) found that even without means of communication, people employ tacit procedures of behavior coordination. To illustrate, consider Figure 8, which represents a game of coordinating a meeting point. The story goes: two people fell from parachutes in a foreign region and should somehow meet; that is, they should chose equal meeting points without being able to communicate to one another. Seven out of eight subjects, Schelling ([1960] 1980, p. 56) found, managed to meet at the bridge. The mental process underlying coordination was "not a matter of guessing what the 'average man' will do," Schelling ([1960] 1980, pp. 92-93) defended; what happened is that "one is trying to guess what the other will guess one's self to guess the other to guess, and so on ad infinitum." The bridge stood out in Figure 8, serving as a natural focal point for subjects to coordinate their beliefs and expectations.[8]

Naturally, game theorists had already thought of collaboration games. Imagine, for example, two players picking numbers among 1, 3, 7, 13, and 100—a "name a positive number" game, as Schelling ([1960] 1980, pp. 94-95) described. Players earn a unitary payoff whenever they pick matching numbers and win nothing otherwise. A traditional game theory model would portray such a situation by an identity matrix: payoffs are zero everywhere, except in its diagonal. If players behave rationally as game theory suggested, they would randomly choose any number following a uniform distribution, earning 1 in payoffs with a probability equal to 1/25. Schelling ([1960] 1980, p. 94) analyzed his subjects' success in coordinating in playing 1: "If one then asks what number, among all positive numbers, is most clearly unique, or *what rule of selection would lead to unambiguous results*, one may be struck with that fact that the universe of all

[8] This discussion originally appeared in one of Schelling's (1957, pp. 20-21) papers, published in the *Journal of Conflict Resolution*.

Figure 8 – Schelling's Map for a Tacit Coordination Experiment



Source: Schelling ([1960] 1980, p. 55).

positive numbers has a 'first' or a 'smallest' number." Schelling ([1960] 1980, pp. 94-95) explained axiomatic game theory assumed labels in rows and columns of a strategic form games should not matter in determining outcomes, but precisely because they *do* have labels, "players can rise above and 'win' these games." As Schelling ([1960] 1980, p. 97) suggested, using labels could put players in a better situation, "and how to use it may depend more on imagination than logic, more in poetry or humor than mathematics." This reading of how players used labels had far-reaching implications: for example, it supported Schelling ([1960] 1980, p. 99) in advocating games in extensive form could not be equivalent to games in strategic form. Until then, game theorists emphasized strategic form games because von Neumann and Morgenstern ([1944] 2007, pp. 79-84), among other reasons, had already shown an equivalence between extensive and strategic forms.

Schelling's ([1960] 1980, p. 111) reasoning about collaboration games would lead him to think of his "focal point" solution, a concept through which players would conspicuously find a way out of otherwise hopeless games (such as that of Figure 8). By insisting on focal points as an answer of how players play games, he opposed mathematicians who made game theory assuming players perceived games as if they were mathematicians themselves. Schelling's ([1960] 1980, p. 114) argument did not mean players could not be exceptional at calculations, since "a rational player must be presumed to know as much mathematics as he ever has need for." But a theory about "best strategy determination" could not simply dismiss non-mathematical features of a conflict situation. If two mathematicians played, it would be reasonable for them to focus on mathematical properties to make their expectations converge—game theory would apply well. If players were *not* mathematicians, however, they could rely on aesthetic, historical, legal, moral, and cultural properties to focus on and make their expectations converge. Thus, Schelling criticized how mathematicians sough new solution concepts by digging deeper and deeper into mathematical features of games. This criticism is especially applicable for research in n-person games (which appeared in textbooks around 1968–1970).

Aware of and motivated by Schelling's (1958) criticism, Rapoport (1960, 227) argued game theory failed in transitioning from zero-sum to non-zero-sum games because it "retained the basic framework of thought into which the zero-sum game has been cast." While Schelling claimed for an appendage of new concepts to reorient game theory (for example, tacit communication, commitment, and threats and promises), Rapoport (1960, pp. 229-231) disagreed they were necessary: when players make commitments, threats, and promises, a "switch from one game to another" could happen. To illustrate his argument, he reinterpreted "the Battle of the Sexes," normally seen as a strategic form game in which players simultaneously make a decision, as an extensive form game, so he could add a commitment move to it. As long as players believed in commitments, threats, and promises, such moves would be amenable to regular game theoretical representation. This was not an argument in defense of what mathematicians had done with game theory, still. They failed in not effectively exploring such possibilities of including communication in games. As Rapoport (1960, p. 232) put it, "what is essentially missing from game theory proper is a rigorous analysis of situations where communicative acts are moves of the game, *and where effective communication may change the game*."

Nevertheless, even if *Fights, Games and Debates* relativized Schelling's (1958) arguments, it agreed on how problematic it was to apply game theory to real problems. Rapoport (1960, p. 234) decided to "illustrate the conceptual and practical difficulties of game theory by applying its techniques to a particularly human decision" after William Shakespeare's tragedy *Othello*. The situation is seemingly straightforward. Othello asked Desdemorra: "Did you or did you not give yourself to Cassio?" While she could either reply "Yes" or "No," Othello could "Believe Desdemona guilty" or "Believe her innocent." Here, Rapoport (1960, pp. 235-236) first stumbled: in translating Shakespeare's playwriting into game theory, should one think of strategic or extensive games? That is, did Desdemorra's answer matter to Othello at all? This thought led Rapoport (1960, p. 238) to conclude: "here the problem of choosing a strategy is intertwined with the problem of choice to believe or not to believe." This choice should "be guided by entirely different considerations," Rapoport (1960, p. 238) continued. Pushing his questions on how to map a real problem into moves in a game, Rapoport reached a dead end; he obtained an extensive form game which was overly complex from a mathematical standpoint, and yet it was not an accurate model for what happens in *Othello*.

Criticisms addressed more than two-person problems; in developing n-person games, both authors suggested game theory should embrace experimentalism. Rapoport (1960, p. 214) argued terms such as "trust and suspicion, power of bargaining, balance of bargaining advantage, and equity (all social-psychological and even ethical concepts)" appeared as residuals in normative game theory, "*ad hoc* explanations of why people do not behave as the normative theory prescribes." Rapoport (1960, p. 224) argued that in moving beyond two-person zero-sum games, it was almost "impossible" to extend normative game theory because "the criteria for rationality become confused by the clash between individual and group norms and because 'equilibria' are either nonexistent or not very relevant to the interests of the players." In opposition, a different game theory built upon an empirical, descriptive approach seemed promising. This shift would entail posing different questions for game theory. For example, for a three-person game, instead of asking which coalitions would players form and how could they distribute payoffs—which, under von Neumann and Morgenstern's ([1944] 2007) approach, yielded no definitive answer as a myriad of configurations is rational—Rapoport (1960, p. 224) asked "what is the frequency distributions of coalitions AB, AC, and BC respectively?" and "Given coalition AB [. . .] what is the frequency distribution of the way the winnings are split between them, and so on for other coalitions?" That is, questions would be more about statistical properties of actual plays than theorems about abstract games.

This change, Rapoport (1960, p. 225) concluded, would solve a critical problem of game theory which, unable to provide definitive answers, resorted to arbitrary behavioral norms; consequently, one had not *a* n-person game theory, but *many* different n-person game theories, each based on different assumptions. Schelling ([1960] 1980, p. 163) maintained a similar position: for him, "the principles relevant to successful play, the strategic principles, the proposition of a normative theory, cannot be derived by purely analytical means from a priori considerations." This followed because one mathematician could reproduce a single decision process, if he knew which criteria guided such decision process; but when "two centers of consciousness" met, formal modeling was a vain approach; a single analyst would be unable to model how each of such "centers of consciousness" took hints from one another.

The criticisms and suggestions in *Fights, Games and Debates* and *The Strategy of Conflict* were reactions to what game theorists did from their newborn discipline until circa 1960. Their opinions echoed through time, as publishers printed multiple re-editions.[9] To understand them, it is important to consider how game theory was changing since 1944. The initial research surge focused two-person problems and slowly moved toward n-person cases, as demonstrated in papers collections Kuhn and Tucker (1950), Kuhn and Tucker (1953) and Tucker and Luce (1959a) edited. The early methods

[9] According to WorldCat, between revisions and reprints, Schelling's ([1960] 1980) book had sixteen re-editions after its initial release until 1990 (1963, 1965, 1966, 1968, 1969, 1970, 1971, 1973, 1975, 1976, 1977, 1979, 1980, 1981, 1986, 1990). Rapoport's (1960), in turn, had six (1961, 1967, 1970, 1974, 1989).

endured, however. Put simply, game theory still restricted itself as a logical inquiry over a strictly formal structure, for which it was especially convenient to use characteristic function form games. Rapoport (1960) and Schelling ([1960] 1980) offered alternatives routes. Even if some modern commentators ascribe a great influence to Schelling's ([1960] 1980) work—for instance, Myerson (2009, p. 1110) sees *The Strategy of Conflict* as what connected Nash equilibria and later critical contributions due to John C. Harsanyi and Reinhard Selten—, such an influence does not come up in textbooks (not, at least, until textbooks of the late 1970s).