

The book is marred by uneven exposition and the infelicities that bedevil expression in a second or third language. And, to this reader, the elements of social epistemology that Santos emphasizes are not integrated well enough into the discussion of individual experiments. We get a good discussion of the experiments, a good account of the gradual development of criteria of evaluation and of an understanding of what economic experiments might test, as well as the author's own insightful contribution to criteria for evaluating experiments. What is missing is a clear analysis of what social epistemology brings to our understanding of these developments. What does SEE, the social epistemology of experiment, make salient that is obscured by conventional (e.g. hypothetico-deductive) approaches to experiment? What is the three-way coherence sought for in the particular experiments discussed? Why should we see this coherence rather than truth (or confirmation) as the goal with respect to which experiments are to be assessed? How does a requirement of critical discursive interaction among diverse participants lead us to identify epistemologically relevant aspects of an experimental episode or series differently than nonsocial approaches would? Although we get accounts of resistance, criticism, and acceptance, these are not integrated with the social epistemology notions of critical scrutiny by researchers representing different points of view. And what is the relation between three-way coherence and the criteria proposed for experiments purporting to measure agent's motivations and preferences? It would have been nice to see the tenets of social epistemology themselves subjected to critical examination in light of the analysis of the episodes of experiment and integrated with the criteria for evaluating the probative valence of experiments. Social epistemology may well have enabled the author to identify features of experiment in economics that would otherwise be neglected, but we do not really see its working in the text. Nevertheless, *The Social Epistemology of Experimental Economics* is an often fascinating and insightful account of the emergence of experiment in this famously recalcitrant discipline.

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War, peace, and the birth of game theory

Von Neumann, Morgenstern, and the creation of game theory. From chess to social science, 1900–1960, by Robert Leonard, New York: Cambridge University Press, 2010, x+390 pp., US\$95 (hardback), ISBN 978-0-521-56266-9

In this long-awaited book, which has been awarded the 2011 Joseph J. Spengler Prize for the best book in the history of economics, Robert Leonard brings together the research on the origins of game theory that he has pursued since the early 1990s, and which already serves as a compulsory reference for all scholars working on the subject. The first 11 chapters of the book richly portray the cultural backgrounds, the social and political contexts, and the biographical developments that led John von Neumann and Oskar Morgenstern to write *Theory of Games and Economic Behaviour* (1944; henceforth

Theory of Games). In the last two chapters, Leonard describes the use of game theory during World War II in search, bombing and allocation problems, and the role played, between 1946 and 1960, by game-theoretic models in the array of social scientific practices at the RAND Corporation, America's quintessential Cold War institution.

Leonard's story is motivated by the conviction that without considering the cultural and political context of *fin-de-siècle* and interwar Europe 'the making of game theory cannot be understood' (p. 2; this and the following quotations are from Leonard's book); and, in fact, one of the main historical claims of the book is that von Neumann's return to game theory in 1939–1941 was only marginally prompted by his conversations with Morgenstern in Princeton, but very much motivated by his personal involvement with the European upheaval of the late 1930s and the descent into World War II.

Leonard's approach is purposely and consistently contextual, and his fascinating narrative draws on letters, diaries, interviews, and additional archival material (often new). One consequence of this approach is that the theoretical and analytical side of the history of game theory plays only a marginal role in the book. This, in turn, may render Leonard's work more exciting for historians of economics and more generally for historians of science, rather than economic methodologists and economists.

In Chapter 1, Leonard highlights the cultural and social importance of the game of chess in Central Europe during the first decades of the twentieth century. In this period, two main views of the game stood opposed. While the first, epitomized by German chess champion and mathematician Emanuel Lasker, stressed the psychological aspects of chess and denied the possibility of any scientific approach, the second considered the game in a formal way and tried to avoid any reference to psychology. This latter, logical approach to chess is most clearly expressed by another German mathematician and chess player, namely Ernest Zermelo, who in 1912 obtained some notable results as to the existence and characteristics of a 'winning position' in chess by applying set theory to the analysis of the game.

This first chapter on chess is original and intriguing, and the reader feels that Leonard enjoyed researching and writing it. However, I must confess that sometimes I found its content too remote from the main von Neumann–Morgenstern storyline.

Chapter 2 studies the two communities to which von Neumann belonged, namely that of Hungarian Jewry and that of Hungarian mathematicians, many of whom were in fact of Jewish origin. Prior to World War I wealthy Jews had reached a high degree of assimilation and recognition in Hungarian society, but after the disintegration of the Austro-Hungarian empire the situation began to change, and in 1920 Hungary passed its first piece of antisemitic legislation. With regard to mathematics, since the late nineteenth century, and despite its small size, Hungary had produced a remarkable number of outstanding mathematicians, and by the early twentieth century a widespread and sophisticated mathematical culture was a defining feature of the country's intellectual landscape.

Although I agree with Leonard that discussing these Hungarian backgrounds is important for understanding von Neumann's personality and approach to mathematics and game theory, in reading Chapter 2 I did have the impression that this could have been done more briefly and I found myself wondering where to locate the point at which the search for contexts and backgrounds should stop. My feeling is that here, and indeed in other parts of the book, the author somehow went too far with this search, and ventured into exploring contexts of contexts and backgrounds of backgrounds too remote from the central topic of the book.

Chapter 3 nicely describes von Neumann's early studies in his native Budapest, and his later apprenticeship, between 1926 and 1931 and under the guidance of David Hilbert, in modern mathematics and axiomatic method at Göttingen University. In a synthetic but clear way, Leonard reviews some of the articles on set theory and the axiomatization of quantum mechanics that von Neumann wrote during this period, and he discusses von Neumann's stance in the debate about the foundations of mathematics that in the early decades of the twentieth century saw Hilbert's formalism opposed to Russell and Whitehead's logicism and Brouwer's intuitionism. Leonard convincingly argues that, although von Neumann gradually distanced himself from Hilbert's position in the foundations debate in the late 1920s, and then completely abandoned it after Kurt Gödel proved the impossibility of a formalist foundation of mathematics, he nevertheless remained 'faithful throughout, and beyond, to the Hilbertian, axiomatic style in mathematics proper and science in general' (p. 46).

Chapter 4 is devoted to the game-theoretic developments of the 1920s. Between 1921 and 1927 the French mathematician Émile Borel wrote a series of papers in which he presented 'the notion of a strategy in a precise form and the principle of random play, and investigated the range of two-person games in which they could be employed profitably' (p. 59). Like Lasker and despite his formal approach, Borel insisted on the importance of psychology in games, and considered mathematical calculation as, at best, a supplement to strategic cunning. Zermelo's anti-psychological approach to game theory, however, was in this period pursued further by Hungarian mathematicians Dénes König and László Kalmár, who between 1927 and 1929 sought to refine Zermelo's results, citing in their papers discussions with von Neumann.

In 1928, von Neumann himself published an article 'On the theory of parlor games,' which follows the anti-psychological approach to game theory and was apparently written without direct knowledge of Borel's papers. While the other early contributors to game theory referred to specific games such as chess and card games, von Neumann attempted to analyze a generic game of strategy played by n players. As is well known, much of the article is in fact devoted to two-person, zero-sum games, and the proof that these games have a minimax equilibrium, that is, an equilibrium in which both players play the strategy that minimizes their maximal possible loss, whereby this strategy may be probabilistic in nature. Appropriately, Leonard notes that von Neumann's 1928 treatment of games was certainly formal and mathematical, yet not axiomatic in the proper Hilbertian sense of the term (p. 68). In fact, in the 1928 article von Neumann does not postulate an explicit system of axioms, and this is a major difference between this article and the approach he later followed in *Theory of Games*.

This is one of the chapters I liked most in the book as I found it very clear and with the right mixture (at least for my tastes) of contexts, backgrounds, and analytical content. The only shortcoming I would point out concerns the fact that the mathematical formulas contained in von Neumann's 1928 article are presented too hastily, so that the reader not already familiar with game-theoretic technical language might find it difficult to understand them. Unfortunately, the same shortcoming recurs also in the following chapters when the mathematical details of *Theory of Games* are dealt with.

Chapter 5 switches to interwar Vienna, and discusses the theories of three figures that in the 1920s had a significant influence on the young Morgenstern: Othmar Spann, an anti-rationalist and conservative economist and philosopher; Hans Mayer, an exponent of the Austrian School of Economics who criticized general equilibrium theory on methodological grounds; and Ludwig von Mises, another prominent Austrian economist who was a stubborn opponent of the use of mathematics in economics and an outspoken

classical liberal. Chapter 6 examines Morgenstern's early contributions to economics which, in accord with the main tenets of the Austrian School, were characterized by the critique of the notion of static equilibrium, the quest for a theory dealing with the temporal dimension of economic activity, and a suspicion of mathematics. In his *habilitation* thesis of 1928 he 'set out to show the impossibility of making any complete forecast of the state of the economy given the complexity of the mechanisms that shape economic events' and criticized 'attempts to understand the business cycle based on statistical considerations' (p. 101).

Although interesting and beautifully written, these two chapters deal with topics that are already well explored in the history of economics and are often, in my opinion, only very indirectly related to the creation of game theory. By contrast, an issue surely relevant to Leonard's story and almost uncharted in the literature, namely Morgenstern's possible antisemitism, is dealt with only in some footnotes and without any comment by the author. Leonard here reports entries from Morgenstern's diary of 1929 such as 'Monday there was "Mises Seminar." [...] Afterward an unpleasant discussion in this arrogant circle of Jews'; 'Friday was the Economics Association. [...] Lots of Jews'; and 'Yesterday there was also the *Geistkreis*. [...] I was the only pure Aryan (out of 8!), Hayek is probably only 1/2 or 2/3. This is uncomfortable.' It is not clear to me why Leonard quoted these sentences only in the footnotes and did not discuss Morgenstern's apparent antisemitism in more detail.

Chapter 7 constitutes a sort of aside from the main storyline of the book as it examines at length the mathematical and political views of Viennese mathematician Karl Menger, the son of the founder of the Austrian School, in the context of the political situation in Vienna in the 1920s and early 1930s. Menger is brought into the story because in the early 1930s he influenced Morgenstern, especially in prompting the latter to abandon his previous opposition to the use of mathematics in economic analysis.

Chapter 8 returns the focus to Morgenstern and the Viennese economists, examining now the decade leading to the March 1938 annexation of Austria by Nazi Germany. During this period Morgenstern distanced himself from Mises and other members of the Austrian School, criticizing among other things their constant mingling of economic analysis with liberal policy advice. Between 1931 and 1938 Morgenstern was also at the helm of the Austrian Institute for Business Cycle Research, and in this role he achieved considerable power in the Viennese economic community and served as policy advisor to the government. However, in late 1937 Morgenstern broke with the Austrian government over issues concerning the control of agricultural prices, and in early 1938 he flew to the USA and eventually accepted a position at Princeton University. By the same time, Mises, Menger, and many other scholars had also left Austria, so bringing to an end Morgenstern's Viennese world.

Chapter 9 covers the period 1930–1940 and reconstructs the biographical and scientific events that, according to Leonard, prompted von Neumann's return to game theory. In 1930, von Neumann knew that because he was Jewish his chances of obtaining a chair in mathematics in Germany or Hungary were negligible, and thus he readily accepted an offer of a professorship in Princeton, first in the Mathematics Department and then, from 1932 onwards, at the Institute for Advanced Studies. Von Neumann's correspondence of the 1930s, and especially his exchange with Hungarian physicist Rudolf Ortway, who had remained in Budapest, shows that while continuing his scientific work he became increasingly preoccupied by the political situation in Europe. His political interests were further stimulated by his return to Hungary and visit to Sweden and Poland between April and December 1938. During these months, Hungary passed new and harsh

antisemitic legislation, and British Prime Minister Chamberlain and Hitler signed the Munich Non-Aggression Treaty that allowed Germany to occupy the Sudetenland, a part of Czechoslovakia. Although von Neumann's European journey was motivated by personal reasons (after his divorce from Mariette Kovèsi in late 1937, he went to Budapest to marry Klára Dán and bring her back to the USA), it forced him to 'confront the political situation directly and witness its contamination of Hungarian and von Neumann family life' (p. 196). According to Leonard, this confrontation played a decisive role in bringing von Neumann back to game theory in 1939–1941, which he now saw no more as a formal theory of parlor games, but rather as 'a new social mathematics, [...] providing analytical insight into the exercise of power and discrimination' (p. 223). Although claims about the psychological motivations that might have led a scientist or an artist to produce a certain piece of work are always questionable, and despite the fact that von Neumann seems not to have declared explicitly that his return to game theory was in fact motivated by the European tumult of the late 1930s, the letters and the other archival materials provided by Leonard make his interpretation convincing. However, and as in the case of Morgenstern's possible antisemitism, in reading this chapter I again felt that Leonard was treating the two main characters of his story somehow too deferentially, and wondered whether less noble motivations might also have played a role in von Neumann's return to game theory. For instance, given the fact that 'his output of papers, normally volcanic, collapsed to one in 1938 and none the year after' (p. 197), von Neumann might have chosen to return to game theory in the anticipation that it would become a hot topic during the war.

Leonard also suggests that even the explicitly Hilbertian axiomatic approach that von Neumann used in *Theory of games* might have had a political motivation. During the 1930s the German mathematician and Nazi sympathizer Ludwig Bieberbach sought to connect different mathematical styles with different races, and claimed that Jewish mathematicians favored formalism and axiomatization, which he criticized as a form of autistic thinking completely unrelated to reality. For Leonard, 'in light of Bieberbach's extremism [...] it is possible to see von Neumann's commitment to Göttingen-style "mathematical modernism" as no longer merely an intellectual or scientific preference,' but as a symbolic and politically laden 're-affirmation of loyalty to a world now lost' (pp. 194, 244). Although this reading of the axiomatic style of *Theory of Games* is suggestive, it seems to me less plausible than it first appears. In fact, according to Leonard's interpretation one would expect von Neumann to have taken a public stance on Bieberbach or to have referred explicitly to him in his writings, but apparently von Neumann did neither of these things.

Chapter 10 analyzes in detail the collaboration between von Neumann and Morgenstern that resulted in the publication of *Theory of Games*. The two met at the beginning of 1939, but it was only in the spring of 1940 that the relationship between Morgenstern's reflections as to the difficulties of assuming perfect foresight in economic theory, and the formal analysis of games that von Neumann was already and independently pursuing, became clear to them. In October 1940, von Neumann produced a manuscript titled 'Theory of Games I (General Foundations),' in which he explored 'games of three and more players and, in a decisive move beyond his paper of 1928, [he created] the new concepts of coalitional equilibrium and stability' (p. 217), that is, the notion of a 'stable set,' which will be the main solution concept used in *Theory of Games*. By January 1941, von Neumann had completed the second part of his manuscript, 'Theory of Games II.' Here he extended the theory 'to cover non-zero-sum situations and, adopting a more formal set-theoretic notation, he prove[d] some short theorems on stability and discusse[d] decomposition of games' (p. 231). These two manuscripts already contain the

backbone of *Theory of Games* and were written without Morgenstern's contribution. In the first part of 1941, Morgenstern was in fact busy writing a critical review of Hicks' 1939 *Value and Capital*, as well as a paper, largely inspired by the work of Karl Menger, on the formal aspects of the maxims governing individual behavior. Morgenstern appears to have begun studying von Neumann's first manuscript only in July 1941, when he also started writing the introduction to *Theory of Games*, and the second manuscript in October 1941. On 14 April 1942, von Neumann constructed (in but a couple of hours) his celebrated axiomatization of cardinally measurable utility. Yet from the second part of 1942 onwards, von Neumann became increasingly involved in military consulting and for him the research project with Morgenstern became one activity among several. The 600-page manuscript went to Princeton University Press in April 1943 and was published the following year.

This is a beautiful chapter that largely draws on Leonard's classic article 'From Parlor Games to Social Science,' published in the *Journal of Economic Literature* in 1995. The conclusion of the chapter (pp. 242–245) is also notable because it summarizes some of the main interpretative claims of the book, namely that von Neumann's return to game theory in 1939–1941 was stimulated 'not by Gödel's work, but by the very concrete personal and historical circumstances of the late 1930s' (p. 243); that the axiomatic style of *Theory of Games* was partially motivated by von Neumann's reaction to Bieberbach's antisemitic dismissal of axiomatization (pp. 243–244); that in 1939–1941 von Neumann no longer saw game theory as a formal theory of parlor games, but rather as a 'new social theory' (p. 243), and that the 'volcanic energy and the extraordinary degree of obsession' with which he turned to the creation of this new social theory contained 'an element of perhaps subconscious resistance to the conditions of the time; an almost defiant willingness to see order beyond disorder, equilibrium beyond the confusion, an inevitable return to normality once the present transition, with its "abnormal spiritual tensions", was over' (pp. 243–244).

Chapter 11 reviews the main tenets of Morgenstern's introduction to *Theory of Games*, describes the transformation in Morgenstern's ideas as a result of his collaboration with von Neumann, and briefly reviews the early reception of the book. In doing so, the chapter concludes Leonard's reconstruction of the Tolstoyan historical drama that led von Neumann and Morgenstern to the creation of game theory.

The last two chapters of the book appear to me as a coda to this main narrative. Chapter 12 describes the use of a small part of game theory (basically the simple two-person, zero-sum, minimax part of it created by von Neumann in 1928) in problems of submarine search and fighter–bomber pursuit during World War II. Although the chapter also deals with von Neumann's wartime involvements with the military, he is now only one character among the many populating these pages.

Chapter 13 portrays the disparate social and scientific activities taking place at RAND Corporation in Santa Monica between 1946 and 1960, and the key role that game theory played in them. Among other things, game-theoretic models were used at RAND to analyze problems of strategic bombing and fighter pursuit in the event of a nuclear war against the Soviet Union, and game-theoretic experiments were performed to study bargaining and coalition formation. Leonard mentions the titles of several RAND papers in game theory, but unfortunately he seldom analyzes their content in detail. A multitude of new characters related to RAND enter the scene while von Neumann and Morgenstern almost disappear from the stage. I must confess that I found it difficult to follow Leonard in his pyrotechnical tour of RAND: too many disparate individuals, activities, researches, and approaches in too few pages.

Personally, I would have preferred that the final part of the book continued to remain focused on von Neumann and Morgenstern, now after the publication of *Theory of Games*. Indeed, here are some issues that I would have liked to know more about: Why did von Neumann so promptly abandon his ‘new social mathematics, providing analytical insight into the exercise of power and discrimination,’ first for military consultancy and later for computer science? Why did Morgenstern rather quickly give up the promotion of game theory among economists? What perception did von Neumann and Morgenstern have of the significance of their book 5 or 10 years after its publication? What processes led to the second (1947) and third (1953) editions of *Theory of Games*? What was, exactly, the relationship between von Neumann and Nash? Although Leonard discusses some of these issues, in my opinion this is not done with the same degree of care and depth that characterized the first 11 chapters of the book.

But I do not want to conclude this review on a negative note. Despite what seems to me a few imperfections, Leonard’s book is masterfully documented and brilliantly written, and constitutes the true pinnacle of his long and exemplary research on the founders of game theory.

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Noxious markets, inequality and social meanings

Why some things should not be for sale: the moral limits of markets, by Debra Satz, New York: Oxford University Press, 2010, xi + 252 pp., US\$35.00 (hardback), ISBN 978-0-19-531159-4

In this thoughtful and timely book, Debra Satz provides a convincing justificatory framework for our ongoing discomfort at the intrusion of markets into many areas of our lives that hitherto had been free from commercial influence. Her central problem is the commodification of everyday life. We inhabit social worlds which are highly commodified and in which the market is often prescribed as a universal panacea for any social problem we confront. Yet despite such overt marketisation in the culture at large, nonetheless there remains the widespread belief that there are some things that should not be for sale: the very thought of selling certain goods remains repugnant. Satz’s task in this book is to explain why such misgivings are correct. How might we best account for the intuition that some things should not be for sale? At the heart of her tale is a concern with the inequality that markets often bring in their wake.

Satz is what we might label a ‘moral limits theorist’. She approves of markets in general, acknowledging the great benefits that markets bring, while also maintaining that there are limits on what kinds of things might be bought and sold. This is a view that has been in recent years defended most prominently by Michael Walzer (1983) and Elizabeth Anderson (1993), as well as being explored by Amartya Sen (1987).

Satz traces the lineage of her position back to classical political economists, such as Adam Smith, who stressed the embeddedness of markets in social structures and who