

Introduction

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If one looks back to the 1930s from the present and reads in the major economic journals and examines the major treatises, one is struck by a sense of "the foreign." That past period, notwithstanding G. L. S. Shackle's (1967) claim that it was a decade of high theory, produced works that today require a gloss, a modernization of the text as it were. Keynes's work, that of Hayek, that of the business cycle theorists seem to be written in a primitive language particularly unsuited, we feel today, to the problems they addressed.

If, on the other hand, we read economics journal articles published in the 1950s, we are on comfortable terrain: the land is familiar, the language seems sensible and appropriate. Something happened in the decade of the 1940s; during those years economics was transformed from a "historical" discipline to a "mathematical" one. To be sure, there had been past calls for the mathematization of economics and attempts to do economics mathematically. From Jevons in England, Walras in France, and Pareto in Italy those calls were clear and forceful. From the American Irving Fisher they were more pointed still. Yet the mathematical movement was slow to take root in the mainstream economics discourse. The founding of the Econometric Society in 1930 was a signal that times were more hospitable to a mathematical approach to economics, but the mathematical economists of the 1930s were still, in Herbert Simon's words, "a sect" (Simon 1959, 493).

Although economists have been interested in this transformation of economics and have recorded personal memories of this period in autobiographical notes, Nobel Prize speeches, and so on, it is only recently that historians of economics have begun to take an interest in the matter. Perhaps this is because the period is a relatively recent one, or perhaps it is because the issues have a very technical context or are large and confusedly interdisciplinary. Yet the past several years have produced

some attempts to write the history of this period; individual scholars have taken on the challenge of tracking the mathematization of modern economics. Recent books by Philip Mirowski (1989) and Bruna Ingrao and Giorgio Israel (1990) have begun this work, telling the complex and interrelated tales of mathematics, physics, and economics in the first several decades of this century. This history is hard to write because it covers many disciplines and many countries. For example, a history of existence of equilibrium proofs over the period 1930 to 1954 (as in Weintraub 1985; Punzo 1991) must connect the Vienna of the mathematician Karl Menger, the "Mathematische Kolloquium," to John von Neumann (1936) and Abraham Wald and trace the movement of ideas and people from Europe to the United States in the 1930s. The story links the Cowles Foundation and the Econometric Society with Kenneth Arrow, Lionel McKenzie, and Gerard Debreu, and thereby links the developments in game theory and programming to the emerging general equilibrium literature.

This line of history is only loosely connected with the issues of how to model economic dynamics. These issues, which greatly concerned economists in the 1930s, found their interpreter in Paul Samuelson (1947). The confused and multilayered history of how "dynamics" circa 1930 was transformed into "stability theory" in the economics literature by the 1950s is likewise hard to tell in terms of "The Story of How Economist A Influences Economist B." Leaving economics aside, one excellent model for this kind of history can be found in Russett 1966.

In economics, the dynamics story connects the mathematical community—J. W. Gibbs, E. B. Wilson, Poincaré, Liapunov, Andronov, G. D. Birkhoff, Solomon Lefschetz—with Samuelson, Arrow, and Hurwicz. Specifically, the history is one of the reception in economics of certain themes, results, and approaches in the study of dissipative dynamical systems and the related qualitative theory of differential equations. The narrative line involves the "Harvard Pareto Circle" of physiologist L. J. Henderson (Henderson 1935; Heyl 1968) and the unusual configuration of skills and talents of the Princeton mathematician Solomon Lefschetz. The story line should indicate that the new ideas of equilibrium theory were not unique to economics but rather were "in the air" so to speak, becoming manifest in a variety of fields and disciplines from sociology to psychology, from naval ballistics to development economics (Weintraub 1991).

Of course there was another major transformer of economics in the

1940s, an intellectual upheaval in the sciences which has been termed "the Probabilistic Revolution" (Kruger et al. 1987; Kruger, Gigerenzer et al. 1987). In economics, this became the new econometrics signaled by Haavelmo's work and that of the Cowles Foundation econometricians, but linked to earlier works in statistical economics in England, Holland, and the United States. Mary Morgan (1990) presents a wonderfully rich history of this diffusion of statistics, economics, and mathematics.

Thus there are a number of perspectives that one could have and that could shape one's writing of a history of the 1940s. By noting the various changes that occurred in the way economists presented their analyses and formed themselves into a scientific community in the postwar period, one becomes interested in reconstructing the transformation of discursive practice in economics in the 1940s. But from all these points of view one is led again, and most seriously, to von Neumann and Morgenstern's *Theory of Games and Economic Behavior* (1944) which had a most profound and complex effect on the mathematization of economics in the postwar period.

Moreover, in the 1980s and now in the 1990s, game theory is perhaps the most lively field of economic theory, and game theorists are currently rewriting entire parts of what had been considered settled fields (e.g., Kreps 1990). This fact, too, piques the interest of a historian of economics in the background and development of the theory and provides a second reason for wishing to have a history of the development of game theory.

Finally, the history of the mathematics community has been more concerned with traditional mathematics, which mostly means the various subdisciplines of algebra, analysis, and geometry. More recently, historiographic changes have been driving new approaches to the history of mathematics, as that subdiscipline has shared the movement to social and contextual history of science.

This historiography measures events of the past against the standards of their time, not against the mathematical practices of today. The focus is on understanding the thought of the period, independent of whether it is right or wrong by today's account. The historiography is more philosophically sensitive in its understanding of the nature of mathematical truth and rigor, and it recognizes that these concepts have not remained invariant over time. This new historiography requires an investigation of a richer body of published and unpublished

sources. It does not focus so exclusively on the great mathematicians of an era, but considers the work produced by the journeymen of mathematics and related scientific disciplines. It also investigates the social roots of mathematics: the research programs of institutions and nations; the impact of mathematical patronage; professionalization through societies, journals, education, and employment; and how these and other social factors shape the form and content of mathematical ideas. (Aspray and Kitchner 1988, 24–25) *→ qualitative*

But this reemphasis in historical studies of mathematics has not so far been directed outside traditional mathematical subject matter: “Applied mathematics, non-Euclidean and projective geometry, operations research, probability, and statistics have received little attention” (30). Thus an additional reason to focus the attention of historians on game theory is that this applied field allows the new historians of mathematics to have access to a scholarly gold field, outside the usual mathematical domain, in which many of the new historiographic themes may be employed to yield riches.

As is not uncommon in historical research, the local and contingent played a role in developing the history of game theory project, the results of which are collected in this volume. First, there is the simple fact that we have access today to the history of game theory through the memories of some quite active researchers such as Howard Raiffa and Martin Shubik, and that Shubik particularly was interested in having an examination of the historical issues. Second, through the generosity of Dorothy Thomas Morgenstern, the Economist Papers Project, Perkins Library, Duke University received the papers of her late husband Oskar Morgenstern. This meant that, together with the Library of Congress’s holding of the von Neumann Papers, archival material of importance to the historians of game theory was available for study. *→ through*

As a result, the economics department of Duke University, with some financial support from both the Duke Endowment and the John M. Olin Foundation, was able to host a conference on the history of game theory in October 1990. The papers presented at that conference were both invited and submitted, with the invitations generated by my conversations with Robert Bates, James Friedman, Alvin Roth, William Aspray, and Martin Shubik. During a year as a Fellow at the National Humanities Center in 1988–89 I spoke with and corresponded with a number of people who “signed on” to this project and whose papers are contained

in this collection. But it quickly became apparent that the history that would emerge would be less a finished product, more a prolegomena to any future history. This was so for several reasons.

First, note that the conventional view of the history of game theory in economics is relatively simple to narrate. It was that von Neumann wrote a paper in the late 1920s on two-person games and minimax. Borel claimed priority but this claim was rejected as mistaken. Then von Neumann and Morgenstern got together in Princeton, wrote their book in 1944, and the word went forth. The story goes on to tell us that, unfortunately, economists were slow to see the importance of the theory. Thus although two-person theory was solved early on, the interesting issues became those of n -person cooperative theory. These problems took a long time to solve, but finally the core emerged as a good solution idea for economics and helped to unify topics in general equilibrium theory. Subsequently information problems emerged in microeconomic theory which could be studied by attention to the nature of the Nash equilibrium theory of noncooperative games, and this is the area of current work in game theory in economics. As the papers in this volume will make clear, this potted history is misleading in all its details.

Second, the conference showed that there was a great deal of confusion about the origins of the theory. Not only is there a real “priority” debate, but there is a great deal of misunderstanding about the nature and issues of the von Neumann collaboration with Morgenstern. From the several sessions it became clear that there was a variety of possible interpretations that could be convincingly presented, and defended, about the pre-1944 history.

Third, there are problems that the conference papers turned up about the fact that game theory was not very well received among economists in the 1940s. In the immediate postwar period game theory was viewed with some suspicion: it was not really economics. It received more attention in other disciplines, and thus the conference revealed that the history of its reception required attention to a more generalized social science perspective, as well as to the issues of the development of a new theory in “applied” mathematics.

And finally, the conference participants were acutely aware that the present importance of the theory in economics leads to reading current concerns into the past record, a natural Whiggishness which, because the theory seems to a modern game theorist to have always meant what it means today, confuses the past with the present. Although there is no

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record of this theme in this volume, it was reflected in the comments that enlivened the various sessions from those in attendance who were game theorists in departments of economics, mathematics, and political science and in schools of business.

As a consequence of these problems with constructing a full and coherent account of the history of game theory, the project evolved into this volume with its more modest claim to take those interested "toward a history of game theory." The papers are thus not definitive, but are rather ^{then} more preliminary. With few exceptions, they were not extensively rewritten to take account of the other papers in the collection, and thus author A's narrative reconstruction may not take full cognizance of author B's archival, or interview, "find."

The first set of papers presents the most interesting historiographic problems for the reconstruction of the history of the theory of games. They overlap in different ways in their concerns and emphases, yet they all take different approaches, and they reach conclusions that are not mutually consistent.

The paper by Robert Dimand and Mary Ann Dimand is, on the surface, a traditional paper in the history of economic thought. The authors backtrack from the theory of games circa 1944 to the von Neumann paper of 1928 to the Borel papers of 1921-27. Their argument concerns the priority claim asserted by, and generally "awarded" to, John von Neumann. The issues are fairly clear in this presentation, namely that Borel's contributions are underappreciated by historians and that von Neumann's priority must be reassessed. Against this particular claim we have Robert Leonard's paper which argues, from a perspective at least in part shaped by a view of scientific work as knowledge creation within a community, that there was no community in which game theory could have taken root before the 1940s, and thus there was no possibility that the theory could have been developed before that time. In linking the mathematical ideas with the communities of mathematicians and scientists, Leonard contextualizes a set of ideas in ways congenial to the best of the new historians of mathematics (Mehrtens et al. 1981). I note further that Leonard's paper, connected to a companion piece he has written on the RAND corporation (1991), uses a set of interviews that he conducted with a number of the original members of the game theory community.

Urs Rellstab's paper was written in response to some of the papers given at the Duke conference. Rellstab used the Morgenstern diaries to

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reconstruct the von Neumann-Morgenstern collaboration, and his paper corrects a number of misunderstandings about the nature and details of that joint endeavor. It is less a reapportionment of credit than it is an examination of the separate and distinct interests of the two authors of The Theory of Games and Economic Behavior. His story of the evolution of their joint project and the chronology he presents are reminders that later reconstructions, based on published retrospectives, may be less useful than contemporary records when they are available.

Andrew Schotter, who edited Morgenstern's *Collected Papers*, and who has a real claim to be Morgenstern's last "student," presents a word-picture of Morgenstern and his intellectual development. He is concerned with the issues of the von Neumann collaboration as well, and his conclusions differ somewhat from those of Rellstab. Although Schotter presents Morgenstern's Austrian background as context for his game theory work, he is more concerned with tracking the issues that concerned Morgenstern through later (post-1944) work in the theory of games, linking concerns that appeared to motivate the theory's creation with what has emerged in the various literatures.

The provocatively titled paper by Philip Mirowski connects the various themes of this first part. It may be best to read it together with its companion paper (1991) on the military connection with the theory of games in the 1940s and 1950s, already published, to see the large issues to which Mirowski wishes us to attend. His paper presents an unusually convincing argument which suggests that the interests of von Neumann, and Morgenstern, were connected to themes in their own intellectual histories. He suggests that von Neumann's response to Gödel's work led away from a concern with axiomatization, the "Hilbert Program" as it is termed, and toward the problems of what would become automata theory, and thus proof-strategies, or computational strategies, or strategic behavior more generally. Thus for Mirowski, the von Neumann of 1928 and the von Neumann of 1944 were doing two different kinds of work in game theory and cannot be well presented as having been engaged in a continuous line of thought. This argument clearly problematizes the issues raised by Dimand and Dimand about priority, and connects with Leonard's effort to track the relevant scientific communities. Mirowski's presentation of Morgenstern as an antineoclassical economist highlights the other deviant strand in the 1944 book and suggests that the revolutionary message of that work, coming from not one but two real critics of orthodox economics, made acceptance of the book

RAND LABORATÓRIO DE IDEIAS QUE FORMA A LÓGICA
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a real problem for economists. An implication, of course, is that until the message of the book could be domesticated, as it were, the theory could not, or better, did not, receive a fair hearing.

Part 2 contains exemplars of what historians of mathematics call "reception theory." That is, the three papers all recount the particular ways that the new theory of games made its way into the literatures of the time and the particular mechanisms by which game theory became known among the relevant communities of scholars; they jointly tell the story of the transition of the theory from the first to the second generation. Angela O'Rand, a sociologist of science, tracks the networks and connections among the early social science users of the theory. Her study of the diffusion of ideas connects the game theory movement to the larger theme of the mathematization of the social sciences in the postwar period and shows how the theory became a wedge in opening up social science to mathematical tools. This, of course, is particularly ironic history since the 1944 book itself was written with a thinly disguised abhorrence of the mathematization of economics going on at the time.

The two papers by Howard Raiffa and Martin Shubik are important documents of the "second generation" of game theorists. Based on personal recollections of Michigan and Princeton in the late 1940s, they show how the message of the theory of games grew into consciousness among the new generation of social scientists trained in the postwar period. The new theory was carried to those places, and those intellectual communities gave a hearing to the new ideas and of course transformed them as well. As personal testaments they will be useful to scholars interested in knowing how game theory captivated those whom we now consider to be the theory's "giants."

Finally, the three papers in part 3 concern the transmission of the new game-theoretic ideas into areas, and communities, apart from mathematics and economics. How ideas permeate disciplinary boundaries and reshape disciplines is itself fascinating for those seeking knowledge about the origins of particular intellectual communities. The stories told here are connected of course, but it is their differences that are most interesting to a historian of ideas. William Riker, one of the founders of modern analytical political science, writes of the ways in which game theory came into political thought in the 1950s. Similarly Vernon Smith, one of the originators of the field of experimental economics, writes of the development of that field and the particular direction the discipline took as a result of the then new theory of games. The history is presented

at length as a case study of the development of a field in which game theory had some real impact on the underlying problematic and on the subsequent research agenda. The final paper, by the historian of science Robin Rider, treats the interconnection between game theory and operations research, where the latter field was likewise shaped and recast by the nature and limitations of the theory of games.

To close this brief prologue, I would like to acknowledge the help of Jeff Roggenbuck, whose index will be invaluable to readers of the volume, as well as that of Beth Eastlick, whose efforts on behalf of the *History of Political Economy* have made my work as editor of this volume much less a burden, much more a scholarly enterprise.

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