

Game Theory and Cold War Rationality: A Review Essay

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This essay reviews new histories of the role of game theory and rational decision-making in shaping the social sciences, economics among them, in the post war period. The recent books The World the Game Theorists Made by Paul Erickson and How Reason Almost Lost Its Mind by Paul Erickson, Judy Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael Gordin raise a number of complex historical questions about the interconnections among game theory, utility theory, decision-theory, optimization theory, information theory and theories of rational choice. Moreover the contingencies of time, place, and person call into question the usefulness of economists' linear narratives about the autonomous and progressive development of modern economics. The essay finally reflects on the challenges that these issues present for historians of recent economics. (JEL: A11, A12, B2, C02, C6, C7, D01, D74)

In North America, historians of science have always had some interest in the social sciences although most disciplinary research on the contemporary period has focused on the traditional hard sciences of biology, chemistry, and physics. For complex reasons (Backhouse and Fontaine 2014), historians of social science have paid more attention to psychology, sociology, and anthropology than they have to economics. Earlier generations of historians of science did not much attend to those whom historians of economics considered canonical – Smith, Say, Malthus, Ricardo, the Mills, Marx, et al. It was left to intellectual historians to write about such individuals. But in the years following World War II, as economics has emerged as a discipline whose practices look familiar to historians of science, their attention has turned to economics in new ways. Historians of the social sciences, who had often been marginalized in history of science programs, have now found a rich mine of researchable topics in economics. Indeed, their incursions into the history of economics are quite changing the way such history is presented (Fontaine 2016).

Most of what has been written on the history of *recent* economics by American *economists* has been what Bruno Latour (1988, 218) called “Legends of the Saints”. Samuelson, Schumpeter, and Stigler and others fostered the notion that only those who were sophisticated economists have the authority to write about the history of modern economic science. As they themselves held their own work in high regard, they spent a lot of effort to read intellectual heroism into economics’ past. Samuelson was not speaking ironically when he described himself, like Newton, as “standing on the shoulders of giants.” Much as André Weil (1978) told the International Congress of Mathematicians in Tokyo that the history of mathematics should be written by eminent mathematicians, economists have spent most of their historiographic energy attending to the organized self-reflections of major economists. Contemporary interviews with famous economists by either famous or not-so-famous economists purport to provide a record of the development of modern economics even as they crowd out serious historical research.

Economists who are not historians have their own interest in the history of modern, defined roughly as post WWII, economics. They are consumers-scholars naturally interested in their own profession. Their interests, especially in their own subspecialty fields, often concern the appropriate awarding of credit, of who did what when, and how a well-known figure got to be well-known. Since the creators of new knowledge are rewarded with tenure, promotions, salary, prizes, offices, honors and such, it often is assumed that historians of economics are supposed to be in the business of adjudicating rival creation claims and then awarding gold stars (Düppe and Weintraub 2014, 204-229). That they are not so occupied is a source of some puzzlement to many

mainstream economists. If they are not in the crediting business, what is it that historians of economics do anyway, and why is it worth doing (see, e.g. Binmore 2012)?

But it is not as if North American economists are *trained* to do serious historical research. Unlike the American Mathematical Society, the American Chemical Society, and the American Physical Society, economists' professional organizations do not encourage research into their discipline's history. Consequently many economists writing the history of economic science tend to write what historians call under researched and over interpreted history. Until fairly recently, it was not the case that historians of modern economics even attended to archival materials, syllabi, correspondence, institutional records, and other staples of historical research. Instead they attempted to construct an account of the past using simply the published written materials that past economists left for others to study. The common idea, anathema to historians, that the works themselves tell their own story and do not depend on readers' understandings and interpretations, meant that context was usually ignored in the search for the "true meaning" of say Keynes' Theory of Liquidity Preference or Hayek's Spontaneous Order or Arrow's Impossibility Theorem. A predictable consequence of removing the study of history of economics from all major U.S. economics Ph.D. programs has been that a number of young European economists, some with a sophisticated understanding of the nature of research in the history of science – from France, The Netherlands, Austria, Germany, Italy, and Portugal – are shaping the field's future within the economics community.

Some historians of science have realized that the grail of "An Archive That No One Else Has Seen Before" can be realized in economics. Newly discovered material

can launch doctoral dissertations and then careers. Two recent books, *How Reason Almost Lost Its Mind* and *The World the Game Theorists Made*, are evidence of precisely this shift. The two volumes are different in their objectives, but not in their narrative richness and meticulous scholarship.

How Reason Almost Lost Its Mind emerged from a several yearlong collaboration among members of a working group put together jointly by Lorraine Daston, Executive Director of the Max Planck Institute for the History of Science in Berlin, and visiting professor in the Committee on Social Thought at the University of Chicago, and Michael Gordin, a professor of history at Princeton. Daston is a distinguished historian of science, and her multiyear, multi-book career has provided historians and philosophers with new ways of looking at objectivity, the probabilistic revolution, scientific facts, scientific objects, and the moral authority of nature. From a variety of perspectives, she has explored the nature and boundaries of reason. The group she Gordin put together included one historian of economics, Judy Klein, two other historians of science, Paul Erickson from Wesleyan and Rebecca Lemov from Harvard and the philosopher of science at Barcelona Thomas Sturm. The book project developed with the individual contributors writing various chapters, then periodically meeting in Berlin and revising and rewriting all of their material collectively until it now appears as a seamless story. This book has several overlapping connections with Erickson's book on game theory, not least of which of course is that Erickson was member of the Berlin working group.

The book's subtitle, "The Strange Career of Cold War Rationality" suggests the book's narrative intent. The Berlin Institute's workshop was titled "The Strangelovian Sciences" and the authors state

The aim of this book is to make the label ‘Cold War rationality’ stick ... Cold War rationality in all its variants was summoned into being in order to tame the terrors of decisions too consequential to be left to human reason alone, traditionally understood as mindful deliberation (2).

Reminding us that “rationality” has been studied mostly within specific disciplinary histories, they want to provide a discussion that is “larger than [a disciplinary examination and whose] resolution [is] less fine-grained” (10). The group of actors they study, members of what they term the “silent club”, include individuals ranging from economist and strategist Thomas Schelling to psychologist Morton Deutsch, from economist Daniel Ellsberg and mathematician Edwin Paxson at RAND (who coined the term “systems analysis”) to mathematical psychologist Anatol Rapoport at the University of Michigan, to sociologist Erving Goffman at the University of California-Berkeley to game theorist Martin Shubik at Yale to political scientist Albert Wohlstetter at the University of Chicago and others. The sites of their work of course include RAND, the Center for Advanced Study in the Behavioral Sciences, the Office of Naval Research, Harvard’s Laboratory of Social Relations, the Cowles Commission, and so on. Other individuals who played a major role include von Neumann and Morgenstern, George Dantzig, Herbert Simon, Herman Kahn, Robert Freed Bales, and many others.

The early chapters 1 and 2 describe how, following World War II, questions of how to “reason” became salient: MIT computer scientist Joseph Weissman claimed that “the reason in question was restricted to ‘formal thinking, calculation, and systematic

rationality” (29). Rationality was to be captured by a “finite well-defined set of rules to be applied unambiguously in specified settings – without recourse to the faculty of judgment so fundamental to traditional ideas of reason and reasonableness” (29). The authors show that Cold War rationalists, the action intellectuals, were never as unified as something like a “school”. Nevertheless they shared the assumption usually unstated that “whatever rationality was, it could be stated in algorithmic rules – whether these were strategies in game theory, the consistency specifications of personal utilities, linear programming code, actuarial formulas for clinical decisions, or cognitive representations” (30).

Setting the stage then, the first chapter delineates this new way of thinking, and presents a condensed history of related ideas going back at least to Condorcet and Bernoulli. Chapter 2 is an exhaustive examination of the Berlin Air Lift, the military Operation Vittles whose programming framework grew over several years into algorithmic procedures. This history contextualizes Dantzig’s “formulation in June 1947 of a linear programming model, his construction in August 1947 of the simplex algorithm for solving linear programming problems, and the subsequent coding of the simplex algorithm for digital computers” (61). Some of the results of both those early developments were presented by Wood and Geisler in the Cowles 1949 Linear Programming Conference (Düppe and Weintraub 2014, 2014a). The story moves then through Herbert Simon, who was working simultaneously with Project SCOOP (Project for the Scientific Computation of Optimum Programs) and the Cowles/RAND resource allocation contract awarded to Carnegie’s Graduate School for Industrial Administration (GSIA). There Charnes and Cooper, employing programming techniques, became the

leaders in the movement to professionalize management science. Topics like minimization of quadratic cost functions, and the use of servo-mechanisms to derive inventory decision rules, shaped the new approaches and the *Naval Research Logistics Quarterly* became an important publication outlet for such work. Simon, at GSIA, argued that the “economists’ representation of rationality as an optimal *outcome* for a goal-oriented decision maker was unrealistic and incomplete. Simon’s emphasis on procedural rationality begged for a psychological focus on the *process* of problem solving” (80). This insight opened the door for psychology.

Early in Chapter Two we see the difference in how historians of science approach the topics like the history of Cold War rationality and the way economists have traditionally structured their narratives. From the detailed schematic pictures of the mechanics of the Berlin Airlift, to the photographs of the plotting board and the airlift terminal at the British base at Fassberg, Germany, to the contemporaneous photographs of the planes and runways in operation, this is a very different and richer history than economists are used to reading. A detailed discussion about the costs of Operation Vittles (14% of the entire USAF budget) “validated the Air Force Comptroller’s plan initiated in 1947 to increase the cost-effectiveness of Air Force operations through the mechanization of the planning process” (57). The full analysis of these moves showed up in Wood and Geisler’s paper for the Koopmans *Activity Analysis* volume. Most of the histories of that conference have focused on the connections of the high theoretical work, the use of fixed point theorems developed from von Neumann’s 1936 paper, and their input into work done by Arrow, McKenzie and Debreu on competitive equilibrium (Düppe and Weintraub 2014a). But the Wood and Geisler paper is precisely a report of

Project SCOOP and the resource requirements “for quantitative goals in the Berlin Airlift” (69). These kinds of considerations are a long way from those more traditionally engaged by historians of economics.

Chapter 3 uses the Cuban Missile crisis of October 1962 as the lens through which we see how the Cold War community linked reason to questions of use and nonuse of nuclear weapons. Thinking about nuclear war was intertwined with ideas about the game theoretic scenario, or game, of chicken. Herman Kahn’s book *On Thermonuclear War* was an attempt to explore rational decision making under thermonuclear scenarios. Kahn, of the Hudson Institute, “believed that everything about nuclear war could be understood using the core principles of rational-choice theory” (87). The details of the Cuban Missile Crisis were framed in terms of rational models of individual decision making. Those case studies of Chicken developed by the Hudson Institute and the challenges to that way of thinking received public exposure in Stanley Kubrick’s 1964 film *Dr. Strangelove or: How I Learned to Stop Worrying and Love the Bomb*. This was Cold War rationality at its showiest. Nevertheless many could agree with Bertrand Russell that “no nuclear war” was the only rational outcome of the strategic hypotheses developed by the US and the USSR. “In July 1955 [Russell] released a manifesto coauthored with the [late] physicist Albert Einstein calling for disarmament, and their eponymous statement sparked intense international activism, not least prompting the formation of the Pugwash Conferences” (85). Russell’s argument thus prepared the ground for conflict analyses of how to avoid playing Chicken in the first place.

One such was psychologist Charles Osgood’s strategy of disarmament dubbed GRIT (Graduated and Reciprocated Initiatives in Tension-Reduction) (84). The authors

take up a number of studies in psychology where the Prisoner's Dilemma provided an experimental operating procedure to investigate nuclear escalation and de-escalation scenarios. The chapter finally engages problems of decision-making in environments prone to "group think" and "cognitive dissonance". The authors point out that GRIT "was intended to be the functional opposite of Herman Kahn's theory of escalation. If for Kahn the central problem was deterrence's credibility, for Osgood it lay much deeper: the inability of the human mind to function according to the seamless rationality Kahn posited" (93). Over time what came to the fore was the complex array of emotions that formed the background of such Cold War rational decision-making. The problem became clear: critiques of mechanical rationality were easy to make. But how was one to evaluate alternative critiques to develop a more coherent theory of decision making? How were the social sciences to structure an approach to reason?

Chapter 4 explores work in the social sciences not generally well understood by economists.

Across rapidly unifying fields and subfields in sociology, cultural anthropology, social psychology, economics, early cybernetics, and operations research, among others, specialists mark out a set of laboratory-like scenarios and quasi-laboratory situations...common to many such sites was a term of art -- the "situation"— that emerges the multifarious and nearly all-purpose construct among groups of allied and cross-disciplinary social scientists...thus the situation was a methodologically defined space for

exploring how Cold War rationality...could be found in something *resembling* real life and (potentially) made to operate in other situations (112).

From discussions of the Micronesia projects managed by sociologists and cultural anthropologists, to the examination of the nature and impact of Harvard's Laboratory of Social Relations which supported Talcott Parsons' Department of Social Relations, the authors go into rich detail about the attempts to ground research empirically with experiments about decision-making. Bales, of Harvard and periodically at RAND, in analyzing the interactions in small groups through observation using hidden investigators, claimed to have found an ideal method "to control the entire group, as a conductor of an orchestra and each musician his instrument. Bales had found that all groups worked as systems, but some were better systems than others. With his method, he could discover which factors made groups able to reach a 'stable and successful' equilibrium state and others unable" (123). The "situation" emerged as an attempt to focus attention on small-scale controlled environments using tools and methodologies from sociology, cultural anthropology, social psychology, etc., in "laboratory like scenarios in quasi laboratory situations" (112) where observation and intervention could be observed close at hand. result of this kind of work suggests

[That] there was to some degree a shared imperative, shared set of assumptions about the *scientific process as it applied to human and social relations*, and, as a result, a shared product that consisted of a newly reworked form of

stripped-down rationality... All sorts of situations, or perhaps *the situation* itself, had the potential to become mobile, detachable, on-the-ground test sites where all kinds of phenomena could be better examined and new methods could flourish. Special powers of seeing into and penetrating the otherwise mysterious phenomena of human behavior resulted: this was the advantage of building a microcosmic world (130-131).

Bales' work eventually moved to the RAND Corporation in the early 1950s where it connected explicitly to investigations of Cold War rationality.

Chapter 5 of the book concerns the connection of game theory, especially the Prisoner's Dilemma game, with strategic thinking during the Cold War period. In large measure this chapter was based on Erickson's work on game theory and reprises some of the material in Erickson's own *Game Theory* volume, so we can be brief. It concludes:

[In] the eyes of many, humans in this era [the 1970's and the 1980's] fell far short of the standards presumed by Cold War rationality. This growing recognition, emerging in tandem with the breakup of the Soviet Union and the dissolution of the national security consensus in the United States, ultimately brought about the privileging of "irrationality" as the prime characteristic of human decision making and the splintering of Cold War rationality" (158).

The final chapter 6 takes the story closer to our present day. With the end of the Cold War, the tight imperatives of nuclear decision-making gave way to more textured analyses. The psychology of making decisions moved along two paths. The first more or less followed the cold war rationality project as it sought to understand deductive thinking. The second focused on decision-making under uncertainty, and eventually employed a methodology of heuristics-and-bias based on the work of Daniel Kahneman and Amos Tversky done in the 1970s and 1980s (163).

The Epilogue reflects back on the entire story. An intellectual history of cold war rationality uncovered a rather dramatic confluence of “ideas, methods, and practices ...[that] have genealogies; they can and should be traced. But the debate that once fit them all together has no such continuity” (188). From our present time, it is difficult for an economist to see how the peculiar times of the Cold War period have led to present paths of inquiry. Just as the critique of macro forecasting models had its roots in the socialist calculation debates of the 1920s and 1930s, so too today’s behavioral financial economics has an ancestor in war gaming at RAND.

[The] mainstays of the debate over Cold War rationality have fragmented into academic specialties, more or less respectable and more or less robust – and quite a bit less connected to one another...[As] the Cold War tensions eased and the lavish fountains of money for the human sciences dried up, Cold War rationality ceased to be a single battlefield of contestation. The kinds of academics and intellectuals who had been part of the original disputes

discussed in this book, who hailed from a wide variety of disciplines, now talk mostly to their peers in their own discrete fields, no longer to one another nor to the high echelons in the Pentagon on a quest to remake the world.

(185-186)

As those of us who were taught that to survive a nuclear blast one must line up to duck and cover in the hallways of our elementary school pass on memories of that time now to our grandchildren, the imperatives of Cold War rationality fade in economics' past. Since so much of modern post World War II economics grew out of the Cold War period, and that so much of modern economics is rooted in controversies and problems long forgotten, it is left to historians and intellectual archeologists to tell compelling stories of how we arrived at our place today. Economists tell their own stories of the present state of X at (T) having arisen from the state $(T - 1)$ which $(T - 2)$ begat and so on. That is indeed the function of a "survey of the literature", a feature of almost all economics doctoral dissertations. But historians write context. And for much of modern economics the context is not simply earlier economics. "*Veni, vidi, vici*" accounts by Nobel Laureates hardly ever show the complexity and richness of scientific practice as performed by real economists working with real colleagues in real institutions. Surveys of the literature present an impoverished view of the intellectual traditions that have created economics as exemplary among the social sciences.

Today's concern across a number of disciplines with rational choice

[is] a far cry from the Cold War rationality debate, whose participants took for granted a shared starting point for

formulating a new, improved rationality equal to the challenges of preserving the world from nuclear immolation. It is easy to find fragments of the debate in this or that sub discipline; it is all but impossible to find anything like the debate itself – its depth, its breadth, its exhilarating and terrifying sense of urgency...If in retrospect, from a comfortable distance and safe in the knowledge that the Cold War did not in fact erupt into the hottest war in human history, the drama looks more like melodrama -- at times even rendered as farce – that is simply proof of how extraordinary and extraordinarily strange, those times were. (188)

A second book also showcases the interest that historians of science have recently taken in the history of contemporary economics. Paul Erickson, trained in Wisconsin's History and Philosophy of Science program, offers a comprehensive history of game theory. The historiography of game theory begins rather late in that theory's history. Aside from a few first person accounts, like Morgenstern's *JEL* paper (1976) on collaborating with von Neumann, economists first turned to the history through a conference held at Duke University in 1991 titled 'Toward a History of Game Theory.' The resulting conference volume (Weintraub 1992) had several pieces by economists who were historians of economics, a couple of papers by historians or sociologists of science, and several papers by second generation game theorists like Martin Shubik,

Howard Raiffa, William Riker, and Vernon Smith. A number of those papers drew on materials that had become available when Oskar Morgenstern's widow donated his papers to the Duke Economists' Papers Project. Among those papers was a diary he had kept from his student days in Vienna to his collaboration with von Neumann through his later years at Princeton. Subsequently several scholars who participated in that conference began writing about the history of game theory. Primary among them was Robert Leonard whose (2010) history of game theory was titled *Von Neumann, Morgenstern, and the Creation of Game Theory*¹. Another book focusing primarily on von Neumann was written by Giorgio Israel and Ana Gasca Millán (2009), and a detailed and sometimes polemical analysis of game theory's role in the Cold War was written by Philip Mirowski (2002). Sonja Amadae's (2003) critique of rational choice theory as a Cold War creation, overlapping to some degree with the Erickson et al. book discussed above, addressed game theory as well.

Most explorations of game theory's history have focused on economics. Erickson sees matters differently. Erikson argues that the view from economics constrains our understanding of the development of game theory itself: "when viewed up close, game theory comes across less as a coherent entity with a well-defined character, message, or ideological content, and more as a cluster of conceptually pregnant metaphors, notations, axioms, and techniques that hang together not because of their internal structure but as a result of accidents of history" (9). He suggests that this perspective, and the history that it produces, "[by] emphasizing the internal *diversity* of the game-theoretic corpus avoids the temptation to essentialize game theory, equating it with Nash equilibrium concept or

¹ Some material in Sylvia Nasar's (1998) short volume on John Nash developed from conversations she had with Leonard.

granting it some essential character, and embracing the fact that game theory's identity was a constant point of contestation throughout this [Cold War] era and afterwards" (9).

Erickson's intellectual stance is important but unfamiliar and possibly uncongenial to those economists who believe that game theory, though it developed *outside* the economics community, is now *properly* associated *with* the economics community. It has a major role in economic analysis and even economic policy work, as testified to by Nobel prizes to John C. Harsanyi, John F. Nash Jr., Reinhard Selten, Robert J. Aumann, Thomas C. Schelling, Alvin E. Roth and Lloyd S. Shapley. In contrast, the historian will always emphasize the contingency of a particular historical process (like the development of game theory) and the particular context in which that process occurred. To write history is to write context, and attention to both the personal and institutional lend weight to a narrative of scientific change. Game theory was not the same "thing" in 1927 as it was in 1944, as it was in 1950, or 1960, or today. Telling a story of the stabilization of a concept, even a scientific one, is always a suspense tale, one whose final outcome could have been different.

Erickson's story begins with a chapter "explicating Theory of Games itself by grounding aspects of the text in the history of the work's creation" (27). He recounts in good detail the origins of von Neumann and Morgenstern's work on questions that arose primarily in Vienna in the interwar period. He points out the connection between the Viennese economics tradition and a central idea in N person cooperative game theory: "The imputation problem (*Zurechnungsproblem*) ... concerned how to divide up the value of the product between the different factors of production — much as the players in the game sought to divide the fruits of their collaboration or collusion among themselves"

(56). Going through the text of the 1944 *Theory of Games in Economic Behavior*, Erickson argues that “it was two other closely related innovations, found in the closing chapters of *Theory of Games*, that most directly brought von Neumann’s mathematics into contact with existing economic theory: the dropping of the zero-sum condition on games and the development of the mathematical theory of utility, the economically meaningful unit for measuring the winnings and losses experienced by the game players”

(66). He goes on to suggest that “whether embraced or panned for its descriptive or predictive inadequacy, the theory of utility would constitute a key point of entry for economists engaging with *Theory of Games*” (70). This was so in part because the question of whether utility was cardinal or ordinal was still live and contested among economists in the 1940s. The axioms of von Neumann-Morgenstern expected utility led to *measurable* utility. How was this idea, seemingly well-based on axioms of rational behavior in risky situations, related to the ordinal-cardinal controversy? Summing up this first phase of its history, Erickson concludes “*Theory of Games* in 1944 thus was less a secure achievement than a promissory note. Its survival depended on acceptance of its broader agenda and style of analysis, if not particular results... [one could argue] that *Theory of Games*’s appeal lay precisely *not* in providing a complete system or definitive piece of work, but in its heterogeneous collection of notations, metaphors, terminology, and results that could be appropriated, reinterpreted, and put to work in a variety of contexts” (73).

Erickson’s next examines how game theory came to be employed in assessing “military worth”. In WWII the Applied Mathematics Panel (AMP) was directed by the mathematician Warren Weaver and staffed with nontraditional mathematicians,

specifically applied mathematicians very different from the usual university scholar. Statistics, computation, and so on were important. As the war ended the concept of military worth, an index capturing gains and losses associated with a particular operation, “would form the centerpiece of Warren Weaver’s ‘Analytical Studies in Aerial Warfare’ which appeared [in 1945]” (86). Weaver’s own account stressed that “Military worth, as the phrase is here used, is closely related to the general concept of *utility* in economic theory. And the reader is warmly encouraged to read the discussion of a numerical theory of utility given... in the ‘Theory of Games in Economic Behavior’” (88).

From here the story moves to RAND, which was constructed along the same lines as the AMP. Erickson points out that “With mathematicians such a significant presence inside Project RAND, game theory per se became institutionalized as an active area of research, so that even though many of the resulting papers bore the mark of Weaver’s “military worth” agenda, the internal logic of the mathematicians’ discipline also began to assert itself... [indeed RAND’s first hire, and chief mathematician John D.] Williams pushed the mathematicians under his direction towards research in game theory that might be relevant to the development of mathematical theories of air warfare” (93). In particular, the mathematicians developed game theoretic models of aerial duels, and that led to dueling problems more generally, like silent duels and three person duels. They also began working on pursuit games like those of fighter planes versus bombers, and attack versus killer submarines.

When wartime planning, with seemingly unlimited budgets, faced postwar realities of financial constraint, the military faced real challenges to make policy decisions about which projects to support and which to abandon. Developing analyses of

competing programs turned into programming, a field of formal mathematical inquiry. The theory of programming engaged the individuals at RAND and, making connection with the research group at the Cowles Commission in Chicago, they jointly put together symposia devoted to game theory initially at the summer meetings of The Econometric Society in 1948 and subsequently at the Cowles conference, sponsored by RAND, on Activity Analysis of Production and Allocation (Düppe and Weintraub 2014, 2014a). Erickson proceeds to go into one of the less well known stories related to these matters through examining the role of the Office of Naval Research (ONR) which developed funding relationships with a number of mathematicians who would prove important in sustaining academic research into game theory. Mathematician Mina Rees, the architect of the ONR program, brought Solomon Lefschetz and his student Richard Bellman in on the project together with Albert Tucker. Tucker's seminar at Princeton "would serve as the institutional focal point of the up-and-coming generation of game theorists: David Gale, Harold Kuhn, Lloyd Shapley, John Isbell, Martin Shubik, and John Nash" (105). As a result of these kinds of moves and these kinds of inter-relationships, Erikson argues that "the theory of the 2-person zero-sum game...became a theory of maximization or optimization – a fundamental transformation in the meaning of the theory, since von Neumann and Morgenstern had imagined game theory as an alternative to the naïve optimization theories they saw in the mathematical economics of their day" (107).

This point is both important and unappreciated by most economists, especially game theorists. From his Austrian perspective, Morgenstern was contemptuous of neoclassical economics. Von Neumann was contemptuous of economists' mathematics, remarking that while the calculus was developed in the 17th century, calculus based

(neoclassical) optimization theory was too narrow, too ignorant of individual and group conflict, to be a basis for solving any nontrivial economic problem (von Neumann and Morgenstern 1944, 1-8). Von Neumann believed, (and confided to Morgenstern who wrote about it in his diary) that Samuelson “has murky ideas about stability. He is no mathematician and one should not credit him with analysis. And even in 30 years he won’t absorb game theory [October 8, 1947]” (Mirowski 2002, 138-139n41). Von Neumann’s cooperative game theory, as Leonard (2010) showed, was an attempt to grapple with the conflicts between individual behavior and group behavior at the root, he believed, of the collapse of European civilization that produced WWII.

The next marker in this complex story was John Nash’s equilibrium theorem for N person games. But beyond that paper, in thinking through what had been the cooperative game analysis launched by von Neumann and Morgenstern, Nash was able to suggest “that any cooperative game could be transformed into a noncooperative game” (112). This move seemed natural at the time, “but pursuing this agenda ultimately led Nash into intellectual territory that was a more problematic fit with the context of military worth analysis or the study of logistics” (113). What was troubling about this reduction of cooperative to non-cooperative games was that ideas about power, trust, and knowledge in social situations kept reappearing in the military’s multi-player games. Finessing such issues by dumping them into “pre-play communication” games made the resulting non-cooperative game less interesting to military planners: understanding how real players made real decisions was the paramount Cold War strategy concern. The Nash equilibrium, so distant in concept and purpose from von Neumann and Morgenstern’s solutions to cooperative games, opened the way to a kind of comment that Erickson got

from Martin Shubik that “one’s choice of solution concept almost constitutes an act of projection of self onto society” (116).

By the 1960’s systems analysis had come to dominate RAND’s interests. Operations research emerged as a favored professional discourse, supplanting game theoretic approaches to solving optimization problems. Consequently different facets of the theory of games survived in different communities. Utility theory remained of interest to economists and behavioral scientists but the theory of multiplayer games fell into disfavor. Despite the connection of games and programming, “the essential mathematics of the theory survived, becoming a special case in the general theory of optimization... [while] interest in programming rapidly came to overshadow that given to games” (120). Game theory had helped to frame and solve a number of outstanding problems in operational planning and program evaluation through the 1950s. The mathematics was only loosely linked to the 1944 book though. Although different ways of thinking about decision-making and information-processing seemed *prima facie* tractable to game theoretic analyses, it would not be until the 1960s that such work “[emerged] in the context of ‘conflict resolution’ and ‘peace research’” (121).

Erickson then steps back to explore the role that game theory played in the postwar period’s expansion of scientific research in the human sciences. In Erickson’s view, game theory found its place among the human sciences in the increased attention, military attention, to gun control servomechanisms, anti-submarine warfare, etc. “that were capable of mimicking and extending the calculational and information-processing faculties of the human mind” (133). The intent was to formalize human decision making processes and game theory appeared to hold some promise in this area. Interdisciplinary

research, fostered for example by the Macy Foundation Conferences of 1946 to 1953, saw meetings of engineers, mathematicians, neurophysiologists, anthropologists, sociologists, etc. connecting as well to the MIT cybernetics group around Norbert Wiener. Cybernetics, or the responsiveness via feedback processes to particular moves or behaviors, was not very consistent with notion of decision making by algorithm that lay behind von Neumann's formalisms. It remained though for the Ford Foundation, newly created following Henry Ford's death in 1947, to galvanize work in the behavioral sciences. One of its primary early programs was titled "Individual Behavior and Human Relations". With RAND's receipt of a couple of million dollars from Ford around 1950, the behavioral sciences had a new institutional home.

Through RAND,

[One] major series of grants, initiated in 1953, ...would prove significant in establishing channels of cross-disciplinary contact involving game theorists at a number of universities, especially the University of Chicago and the University of Michigan. The founding of the Center for the Advanced Study in the Behavioral Sciences at Stanford University with Foundation money would provide an institutional waypoint for many of the practitioners of the new, mathematical theory-oriented social science. ...[The Foundation also sponsored] conferences and summer training sessions [to] directly promote the adoption and testing of mathematical theories like game theory in a range

of disciplines, especially social psychology and economics
(130).

Erickson links this work with the efforts of Duncan Luce, who had been a mathematics graduate student and who had some training with MIT's Kurt Lewin in small group dynamics, to use game theory to study the information and communication issues associated with coalition formation in von Neumann and Morgenstern's theory of cooperative games. That work was also connected with Paul Lazarsfeld's group at Columbia which explored "the problem of measuring attitudes and opinions among members of the public" (146).

In 1952 Howard Raiffa, a recent mathematics Ph.D., came to Columbia's Department of Statistics to replace the deceased Abraham Wald. Raiffa's background in solving nonzero sum games led him to visualize a successful career "at the intersection of operations research, mathematics, and statistics" (147). A postdoctoral year at Michigan connected Raiffa with several behavioral scientists there whose explorations of group behavior, based on individual behavior plus aggregation, ultimately led to the analyses that Luce and Raiffa highlighted in their *Games and Decisions* (1957). That book was extremely influential. Accessible to nonmathematicians, it moved away from von Neumann and Morgenstern's quite formal treatment of games. The book provided a broader image of game theory's worth as a vocabulary and as a set of metaphors, analogues, and images that illuminated questions that arose in many different disciplines.

In Chapter 5, Erickson examines the context "in which the idea became widespread that the Cold War between the two superpowers was a *game* in the technical

sense of game *theory* and in which the problem of how to choose rationally in this situation became perhaps *the* central problem of the age” (164). Erickson focuses on the behavioral scientists at Michigan’s Mental Health Research Institute in the mid-1950’s, particularly those involved in the construction of conflict resolution theory. He tells the story of Nicholas Rashevsky and Jacob Marschak in Chicago and how, after some of their group moved to Michigan with Anatol Rapoport, they began examining topics like team performance under stress. The Prisoners Dilemma began to be used as an experimental game, one of several such used in psychological research through the 1960s. Teamwork studies evolved over time into conflict resolution studies: how one was to reason in an arms race? Thomas Schelling’s work attracted Rapoport’s attention and Rapoport, who had employed game theory in his previous work, became an antagonist of Herman Kahn and the nuclear war scenario community². Game theory eventually became embroiled in questions of arms control and militarization in the latter part of the 1960s. At the same time a number of the Michigan theorists were moved to political activism; members of this community, and economist Kenneth Boulding, led the Vietnam War teach-in at Michigan. The question remained: What was game theory’s status as the theory of rational decision-making? As mentioned in the earlier discussion of the Erickson et al. volume above, the kind of rationality associated with algorithmic processing – if strategies are specified, a referee can “play” the game absent the players – can lead to horrific outcomes like Mutually Assured Destruction, the Doomsday Machine, and nuclear “Chicken”. Was strategic reasoning an answer to the arms race or

² Martin Shubik recalled that “The semi-humorous comment in some of the bigger war games was if you want the game to go nuclear cut down on the generals and diplomats and increase the numbers and positions of power for the 20-25 year olds” (Shubik 2016).

did one need to go beyond a formal axiom-generated concept of rational behavior? In any event game theory was no longer seen as the formal mathematical object that was born in 1944:

Game theory's connection with behavioral science and with debates over arms control shifted its center of gravity, making it less a mathematical theory of optimization or decision-making (as it had been for military-funded mathematicians and operations researchers) and more an open-ended framework for investigating the interplay between individuals self-interest and cooperation in a variety of settings" (205).

Although game theory developed from questions about the nature of rationality, the next phase of game theory's evolution did not involve rationality at all. "The Logic of Animal Conflict" by J. Maynard Smith and George Price, which appeared in *Science* in 1973, opened a new series of connections for game theory and behavior. Among biologists, particularly population biologists and evolutionary biologists, "game theory provided a framework for reconciling what neo-Darwinian and evolutionary theorists saw as the inherent 'selfishness' of genes with a number of apparently cooperative adaptations observed in nature, rather than simply accepting these adaptations as normal features of life" (205).

In his Chapter 6, Erickson shows how "one of the most remarkable legacies of game theory's encounter with biology was the creation of a new 'game theory without

rationality' that, ironically, a number of economists and political scientists would subsequently adopt to free their models from unrealistically demanding assumptions about the cognitive abilities of *human* social actors" (206). Erickson tells the story, not well known by either economists or historians of economics, of William D. Hamilton who would become one of the premiere evolutionary biologists of the 20th Century. In a series of papers from the mid-1960's Hamilton used his interest in game theory to suggest that animal behaviors could be thought of, in certain circumstances, as the selection of strategies. Developing the notion proposed by Maynard Smith on evolutionarily stable strategies (ESS), "in the hands of evolutionary biologists, game theory continued its movement away from the networks of military patronage that had sustained it in the immediate postwar years. Biologists encountered game theory in a range of contexts, most notably in connection with conflict resolution ... Ironically, the end result of game theory's foray into biology thus was a theory of rational choice stripped of anything resembling 'rationality' or 'choice'" (237).

The seemingly disparate interests of economists exploring the implications of Nash equilibrium in rational choice problems, and biologists in ESS analyses of population dynamics, were brought together in the mid-1970s. Peter Hammerstein had been a doctoral student of Reinhard Selten's who took up a position in Maynard Smith's Sussex (UK) research group. Smith's ESS was actually a Nash equilibrium, though Smith knew nothing of Nash's work until Hammerstein called it to his attention. Erickson's final chapter opens with Hammerstein's and Selten's demonstrating "the close connection between the ESS and the Nash equilibrium, showing that the ESS was a particular kind of Nash equilibrium...or a particular equilibrium refinement" (240). This led to the never

clearly articulated hope “that a final theory of games encompassing the economies of nature and society alike was just around the corner. (241)

Economists of course are comfortable with the history from this point on. Erickson explores this explosion of interest in the Nash equilibrium and suggests that “the proliferation of ‘game theories’ has gone hand in hand with the further disintegration of any straightforward conception of ‘rationality’ and the problematization of game theory’s rationality postulates more generally...Straightforward and generally applicable rules of rationality will not necessarily get you where you want to go” (244).

Erickson refuses to provide a simple closure with any last word. Instead, he tells us “game theory, as it has been bequeathed to its latter-day practitioners, provides a heterogeneous collection of tools for notating, speaking, and reasoning within the human sciences. But while these can prove exceptionally useful and ergonomic, if the debates...are any guide, they are unlikely to be the only possible ones. They are thus to some extent traditional and conventional – an outgrowth of a particular history and set of practices for the study of reasoning and social interaction” (271).

Economists most likely will be dissatisfied with this conclusion. The result of a long analysis or narrative somehow should, an economist expects, lead to an appraisal of the enterprise described or a solution to a problem posed. Historians though are not economists. A historian writing a history of the Office of Naval Research would be unlikely to conclude that it “worked” or “didn’t work” (Sapolsky 1990) but would describe instead how it worked, and how it developed in the context of competing interests and complexly intertwined communities. So too with a history of the world in which game theory was born and developed.

As North American economists who specialize in writing the history of economics are increasingly scarce on the ground, histories of economics will more frequently construct economists as “them” not “us”. Historians of science will more frequently treat economics as a wider set of practices and communities than economists do themselves. In an ironic twist, historians of science who simply treat economics as an appropriate subject of their research have laid to rest the hoary controversy about whether economics is or is not a science. This move, manifest in the two books discussed here, makes economics part of a richly interesting conversation about the growth of knowledge in and about the modern world. Even better, the authors of these volumes set a high standard for historical research and writing about our discipline.

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