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Essays in the history of economic thought: Theory and institutions in the mid-twentieth century

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Theory and Institutions in the mid-Twentieth Century

by
Robert J. Leonard
Department of Economics
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Date: Aug. 8, 1991

Approved:

Craufurd D. W. Goodwin, Supervisor

Dissertation submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in the Department
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ABSTRACT

(Economics)

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ABSTRACT

The history of economic thought is usually associated with the examination of contributions up to and including developments of the early twentieth century. With some notable exceptions, the changes in theory, methods and intellectual environment of the 1940's and after are not usually the object of historical study. In a partial attempt to reverse this pattern, this dissertation comprises three essays in the history of *modern* economic thought. Their collective purpose is to highlight some features of the evolution of mainstream, neoclassical economics and related institutions during the period centering on World War II. The approach to research reflected in these essays is eclectic, encompassing the use of published books and articles, archival material, and oral interviews. The latter two sources have been particularly useful in enriching, and indeed sometimes altering, the perspective gained from a familiarity with only the published record.

The first paper is a detailed examination of the evolution of game theory, focusing in particular on the sequence of formative contributions which preceeded its incorporation into economic theory. The work of Borel, Steinhaus, and von Neumann during the 1920's and 1930's is presented and given an intellectual and social context. We show how the isolation of these contributors forbade the construction of anything that might have been called a "theory" of games. This changed, however, during World War II when game theory, as a field of applied mathematics, became socially relevant for the first time. Von Neumann & Morgenstern (1944) acted as a signal in this regard: it helped spur theoretical discussion which centered, not on the content of the book itself, but on the earlier pre-war ideas.

The post-war milieu in which game theory was first nurtured also gave rise to another development which sheds light on the evolution of modern economics. This is the application of economic analysis to defense policy. Beginning in World War II, with the application of simple cost-benefit reasoning to bombing strategy, economics became increasingly important in defense decision-making. It formed the basis for the new discipline of operations research and provided the intellectual underpinning for the Pentagon budgetary reforms of the 1960's that

subsequently became known as the "McNamara Revolution". While defense became an area in which economists became very prominent, as a policy area it remained peripheral in the academic world. This is also explored.

The final paper is concerned with the relationship between the Ford Foundation and mainstream neoclassical economics. Setting out to direct economic research towards retarding the spread of communism at the beginning of the "Cold War", the Ford Foundation was guided initially by a mixed group of economists reflecting both institutional and neoclassical perspectives. As power in the discipline at large moved increasingly towards the adherents to the latter, the Foundation essentially became "captured" and found itself supporting all aspects of academic economics, even those far removed from its initial policy concerns. By the late 1960's, in the face of a new set of social problems, and with diminished confidence in the value of academic economic research, the Foundation scaled back tremendously on such support.

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Roy Weintraub has also helped shape both the letter and spirit of this thesis, and has influenced my views of the history of economic thought and scholarship in general. He is unique among intellectual historians interested in economics and and has set standards in his own scholarship to which I aspire. Neil de Marchi, too, has sustained, at a high level, discussion of historical and philosophical matters related to economics. In his reaction to my work he has always cut to the heart of the matter, and I attach great importance to his opinion. James Leitzel also, both directly and by example, has been a source of support and stimulus for which I am grateful. Looking to the broader academic circle, I have benefitted from the comments of the sometime participants in Duke's Economic Thought seminar, including, amongst others, Bruce Caldwell, Bob Coats, Cliff Gaddy, Daniel Hammond, Mary Morgan, Hervé Moulin, Urs Rellstab, Jeff Roggenbuck, and Margaret Schabas.

Less tangible than the above, but no less important, is the long-distance moral support and encouragement of my parents, Patrick and Joan Leonard, and sisters, Noelle and Miriam. They have, unwittingly perhaps, been crucial to my efforts and my debt to them goes beyond words. Finally, but by no means least, I thank Valérie Cauchemez. Not only has she made my work easier, she has made it, for me, worth while. Affectionately and spiritedly, she has encouraged me through "thick and thin" and has, above all, taught me to keep everything in perspective.

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Introduction

A casual perusal of the presidential addresses of the American Economic Association of the last fifty years conveys to the reader a sense of the considerable change which economics has undergone during that period. By "change" we mean not simply the evolution of the theoretical subject matter, but also transformations in the relationship between economics and other disciplines, in economists' conception of their own field, and in the role of the economist in policy-making. In 1952, Harvard institutionalist John H. Williams saw the profession's pretension to universality as the "inescapable bane of theorizing" and condemned theory for its own sake:

> "Economic theorizing seems to me pointless unless it is aimed at what to do. All the great theorists, I think, have had policy as their central interest, even if their policy was merely laissez-faire" (p. 10)

This move towards abstraction and away from policy concerns was regarded with similar alarm by Edmund Witte in 1957. Economists, when they deigned to consider applied questions, were often "somewhat apologetic in doing so" and seemed to "greatly prefer to deal with universal truths which lend themselves to model building and mathematical reasoning" (p. 12). While some were no doubt suited to theoretical work, the majority, said Witte, would be better used in tackling "concrete questions which appear not to be too large for their capabilities and resources" (p. 14). Similar concerns are expressed in other addresses (see Goldenweiser 1947, Stocking 1959).

By 1962, however, Samuelson, looking back upon the great theorists, lays emphasis on the extent to which they can be interpreted as preempting modern developments. For example, Marx's mention of harmonic analysis of economic cycles "can be construed as pointing towards modern periodogram analyses and Yule-Frisch stochastic dynamics" (p. 12). In the same spirit, three years later, George Stigler regards "the so-called theoretical revolutions of a Ricardo, a Jevons, or a Keynes to have been minor revisions compared to the vast implications of the growing insistence upon quantification". And the theoretical and quantitative progress of the last fifty years, he believed, would "inevitably and irresistibly enter into the subject of public policy, and [help] develop a body of knowledge essential to intelligent policy formulation" (1965, p. 17). By 1990, Gerard Debreu casts a retrospective glance over the process of "mathematization of economic theory" to which he had been party. While eloquently summarising the theoretical developments made possible by the use of mathematics --- more rigor, fewer logical errors, stronger conclusions, greater generality etc. --- he acknowledges the critcism to which the entire process has been subject (see Leontief 1971, Gordon 1976). He states further that assessment and criticism of the work of the last fifty years will be made possible only by detailed historical analysis, and his address concludes with a circumlocutive call for that history:

> "The quality of assessments of the phase that economic theory underwent [during the past five decades] and the effectiveness of attempts to alter the course of its evolution will gain from a detailed analysis of the processes that led to its present state" (p. 6)

The present set of essays might be regarded as a partial response to Debreu, and have as a common theme the process of disciplinary change in economics which took root during, and immediately after, World War II. They are variously concerned with theoretical development, with the evolution of a research community, and with that community's changing relationship to society. Two particular features of the historiography in this collection are worth noting.

First, they center on a time period which, in the history of economic thought literature to date, has received comparably little attention. With the exception of a few historians (see, for example, Weintraub 1985, Mirowski 1990, 1991), those writing about the evolution of economic thought have usually stopped short of the particularly prolific post-World War II period. Two reasons for this are immediately suggested. First, there is a natural tendency among historians to turn to topics only when a sufficient period of time has elapsed to permit a "detached historical perspective" (Coats 1990, p. 1). By this is presumably understood "historical" as opposed to "chronological" time, the detachment provided by historical flux rather than calendrial change. It is implicit in this collection of essays that we are now sufficiently distant, historically speaking, from the disciplinary changes of the period extending from the 1940's through the 1960's, so as to permit their legitimate and serious examination. The second reason why historians avoid this period has more to do with the actual nature of the change which economics has undergone during this time. In short, it became sufficiently abstruse so as to defy the comprehension of many in the discipline who lacked the proper training in mathematics and statistics. Like any linguistic change, it necessarily involved a shift in authority, the empowerment of some at a cost to others, and the earlier presidential remarks considered above partly reflect the exasperation of those who felt isolated by such change. What this has meant, for those historians of the period interested in the details of theoretical change, is that they must understand the new linguistic and rhetorical forms appropriated by economics. By this one means not simply an ability to read and comprehend mathematical proofs, but equally importantly, a sensitivity to the changing disciplinary priorities of which such a linguistic change is a sign. One must be able to break into the hermeneutic circle which links mathematical elegance and theoretical progress. To the historian's larger capabilities of synthesis and interpretation must be added a familiarity with at least the mathematical and statistical world.[1]

The collection's second feature is that it challenges the traditional internalist approach to historiography. There has long been a conception abroad in the history of economic thought that theoretical change is governed by *internal* dictates: theoretical innovations are caused by existing contradictions and impasses within the theory, rather than by extradisciplinary "shocks", or by political and social change. While this view is gradually becoming less dominant, with the forging of links between historiography in economics and that of mathematics and the other sciences, the vast majority of new contributions still

[1] The extent to which historians of economics are familiar with, and in some cases, actively involved in, the history of other fields has never been greater. For example, Gigerenzer et al 1989 features contributions by Mary Morgan on the history of econometrics, by Ted Porter on the history of statistics, and by Lorraine Daston on the history of mathematics. The central claim of Mirowski 1990 is that the evolution of economics cannot be understood without comprehending that of physics. What is particularly interesting is that this development is taking place at the same time as the history of economic thought is becoming increasingly marginalised within the economics discipline itself.

take an internalist stance. By contrast, these three essays lay considerable emphasis on external developments: they address, respectively, the relationship between economics and the mathematics community, this being the source of game theory's initial development; the strategic community, which assumed particular importance with the changed political geography following World War II; and the philanthropic community, in the shape of the Ford Foundation, which initially sought to marshal the resources of academic economics towards the conservation of the American political and social system. All three external influences affected the path taken by the economics discipline. Nor, however, can the influence be exclusively regarded as unidirectional: the historical development of the Rand Corporation and Ford Foundation were profoundly affected by the prominence they gave economists and the economic method.

The first essay examines the early evolution of game theory, beginning with the work of Borel, Steinhaus and von Neumann in the 1920's. While the period until 1944 can be retrospectively regarded as being creative from the perspective of game theory, not until World War II did there emerge an intellectual environment in which the ideas could mature. The salient features of this era were the appearance of von Neumann & Morgenstern's Theory of Games and Economic Behavior, of course, and the impetus given to mathematical economics and applied mathematics by academic involvement in the war. Particular attention is given to this changing environment and to the early postwar developments in game theory which preceeded its incorporation into economics.

The second paper, in a related context, examines the evolution of the relationship between economics and the study of conflict. Beginning in World War II, with the application of simple cost-benefit rules to the choice of bombing targets, economic analysis became an increasingly important tool in defense-related decision-making. Maturing in the same milieu that nurtured the initial postwar work on game theory, elementary economic thinking formed the basis for the new discipline of operations research, and provided the intellectual underpinning for the Pentagon budgetary reforms of the 1960's that subsequently became known as the "McNamara Revolution". Their prominence in this area contributed to the increased authority of neoclassical economics in policy circles in the three decades after World War II. Ironically, however, defense economics never became a significant field of study within academic circles. It is argued here that this reflects the extent to which the whole area of defense policy became an increasingly treachorous moral terrain in which few intellectuals felt comfortable, least of all "value-neutral" economists. Second, those dominant in the field explicitly eschewed the use of sophisticated mathematical techniques and thus, consciously or otherwise, did little to attract those enamored of high formalism. From its earlier tentative application to strategic bombing in Germany, the perceived relevance of cost-benefit analysis to weapons decisions grew steadily: ultimately, the entire process of defense-budgeting was shaped by the application of marginal analysis. This sequence, the topic of the second paper, succinctly illustrates how, in one particular area, neoclassical economics gained authority in the broader, non-academic, policy-making community. Further, the above postwar milieu, typified by the RAND Corporation, in which both game theory and the economics of defense were nurtured, also played a significant role in the creation of other techniques now standard in economic theory, including linear programming, decision theory, and mathematical economics. For example, Debreu's Theory of Value was written in part at RAND (see 1954, p. xi).

The question of the authority of neoclassical economics is also the subject of the third essay. As a very large supporter of economic research beginning in the early 1950's, the Ford Foundation was important to the discipline. Advised at the outset by a mixed group, in which institutionalists featured as strongly as neoclassicals, the latter soon became dominant and the foundation's strategy in supporting economics slowly moved from an emphasis on applied questions to the sustenance of the discipline itself in *all* its aspects, education and theoretical research, as well as policy questions. This lasted until the Foundation lost confidence in the value of such widespread support, concluding that an increasingly narrow discipline, no matter how well sustained, would not yield solutions to the societal dilemmas of the day.

The contrasting experiences of economic research in the defense area and at the Ford Foundation yield complementary insights into the evolution of neoclassical academic economics. In both cases, the authority of economists was, at the outset, unquestioned. At Project RAND, the architects of the new technocracy advising on defense policy, and at the Ford Foundation, those concerned with marshalling academic resources towards the preservation of the political and social fabric, both regarded the field of economics if not as a policy panacea then at least as a rich source of advice, a wellspring of sensible approaches to pressing questions. In time, however, as the academic discipline grew more abstract and less directly concerned with policy issues, these relationships changed. In the defense policy area, it became increasingly difficult to attract economists. One reason for this, it is argued here, is that such policy questions did not call upon the skills then being developed in the study of economics at the graduate level. There was little room for sophisticated techniques, and thus defense remained of peripheral academic interest. In the policy areas of concern to the Ford Foundation, on the other hand, it became increasingly difficult to attract problem-solvers. The funding institution had slowly become a "rich uncle", following the neoclassical community's advice that neoclassical economics needed long-term, all-round, substantial support. As the discipline evolved, however, economists poured increasing amounts of intellectual energy into the sort of abstract work revered by Samuelson and Stigler, and less into the policy issues of concern to institutionalists like Williams and Witte. In addition, neoclassical economics' technique-driven path had isolated it increasingly from the other social sciences: in the face of the new social problems of the 1960's, it appeared noncooperative and relatively impotent.

Chapter 1: Creating a Context for Game Theory

Introduction:

After decades of hesitation, the theory of games now plays a central role in economic theory. New textbooks on microeconomics no longer relegate Nash equilibrium to a section on "other topics", but now place the formalization of interaction at the theory's very heart (see Kreps (1990)). Nor has this been confined to the theoretical core: the "new" industrial organisation, for example, differs from the older "structure-conduct-performance" paradigm in its emphasis on the game theoretic aspect of firms' decisions (see Jacquemin 1987, Tirole 1988). Regardless of how one views the worth of these developments --- and in this regard there is considerable debate (see Fisher 1989)--- the ideas of game theory have permeated economics in a circuitous manner. While the stylized historical précis locates the seminal ideas with von Neumann & Morgenstern (1944), their purification and refinement by Nash, Shapley, Aumann and others in the intervening period, all cumulatively contributing to the position in which we find ourselves today, the adoption of the game paradigm by economic theorists has not been a smooth process. Nor were the "new" ideas of 1944 appropriated and developed primarily by economists. Game theory initially provoked curiosity as a mathematical construct; this interest was sustained and nurtured by those who saw the potential military application of that mathematics, and by the illustration of theoretical links between theoretical games and related constructs in mathematics and statistics. These links, furthermore, did not emphasise those aspects of games given most attention in von Neumann & Morgenstern (1944), but initially reached back to the former's earlier work and that of some French mathematicians he had essentially ignored. Game theory's initial development rested, ironically, not on the material in the Theory of Games, but in the disparate papers which preceded it. Part I of this paper discusses the disconnected contributions to mathematical games of the pre-1944 period, showing how, in the absence of any discursive mathematical community, game "theory" remains something of a misnomer. Part II examines the social, institutional and mathematical transformations brought about by World War II, which created a context in which game theory became significant.

After all the initial noise died down, these mathematicians took to exploring primarily pre-Morgenstern game theory, i.e., two-person, noncooperative games. And even though von Neumann alone had developed the mathematics of cooperative games and devoted the bulk of the Theory of Games to that topic, seeing the analysis of n-person games as the theory's crowning achievement, this postwar work was done with his sanction and encouragement.

The history of game theory has only recently begun to receive significant academic attention. Rives (1975) provides a useful introduction but does not explore any particular aspect in great detail. Mirowski (1991) treats of some of the subject matter of this paper arguing that the military influence on games was not only significant, but retarding, in that it prevented the theory's proper or "logical" development. A forthcoming volume edited by E. Roy Weintraub will gather further recollections and reconstructions and promises to provide the beginnings of a multi-faceted history. This paper's unique contribution is that it explores in considerable detail the seminal work by European mathematicians in the first half of this century, and then links this to the seminal work after the appearance of Von Neumann & Morgenstern (1944). The latter work is shown to be significant not in that it *itself* became the object of direct mathematical attention, but in that it focused the attention of mathematicians and their patrons on an inchoate body of applied mathematics which World War II had made relevant. The paper may be essentially regarded as a prelude to a history of the incorporation of game theory into neoclassical economics. The research has been informed by multiple sources: if published articles and books have provided the warp, then archival material and oral interviews have added the weft. Particularly useful have been the Morgenstern papers at Duke, the Von Neumann papers at the Library of Congress, and extensive discussions with some figures involved in the early mathematical work, including Samuel Karlin and Edward Harris, both formerly of RAND.

Part I Exploration: the Theory of Games to 1945

In 1953, there appeared in the columns of Econometrica (see Fréchet 1953) a restrained but firm debate on the legitimacy of John von Neumann's position as "initiator" of game theory. The challenge, posed by French mathematician Maurice Fréchet, implied that due credit had not been paid to his senior colleague Émile Borel. The latter, Fréchet pointed out, had written on game theory in the early 1920's *prior* to von Neumann, and while he had not proved the central minimax theorem, he *had* raised the question of its validity, and had speculated on the ultimate application of such ideas to economic and military problems. Von Neumann wrote a stiff rejoinder, claiming that until his 1928 proof of the theorem, "there was nothing worth publishing" (p. 125). Fréchet remained committed to his claim, countering that Borel's early speculations provided "an open door", through which von Neumann could walk.

We take this debate as point of entry into the history of game theory for the following reasons. First, it is a sign that something of theoretical significance has happened. There had emerged a set of ideas in which several parties had different, often conflicting, interests: there was something worth arguing about. Second, to the extent that it indicates what historical aspects the participants themselves found interesting, it helps us cast an interpretative net back over the period in question. What follows in Part I begins with an exploration of the period referred to by Fréchet. In particular, we examine the relevant work of Borel, von Neumann and, also, the Polish mathematician Hugo Steinhaus. We also carry the inquiry through the 1930's, covering the passage of game theory to the U.S., up to the publication of von Neumann & Morgenstern (1944). Our concern, it must be emphasised, is *not* to reopen the above priority debate. Rather, we illustrate with equal emphasis that, for the most part, there was negligible interaction between the individual mathematicians concerned. To the extent that a mathematical theory is given life by a discursive community, arguing and contributing to the set of ideas in question, the mathematical analysis of games before World War II was a particularly lifeless affair. Not only was the number of interested people involved very small but, for all intents and purposes, they remained *incommunicado*. Only with the appearance of von Neumann & Morgenstern (1944) did this state of affairs begin to change. Throughout, to the greatest extent practicable, we describe the intellectual context of the ideas in question.

"Borel's Wager":

Like so many of the protagonists in our short historical excursion, Borel led a life which was, to put it mildly, fuller than that of many of his contemporaries (see Collingwood (1959), Fréchet (1965)). His brilliance was recognized early when, at age 18, he won first prize in the Concours Général and achieved first place in the admission lists for both the École Normale and the École Polytechnique. For the rest of his career, he was prodigiously active in the spheres of both academics and politics.

Borel was directly influenced by mathematician Gaston Darboux in his decision to become a *Normalien* and pursue a career in research. His 1894 doctoral thesis on the theory of functions stemmed from a theme derived from Darboux, and contained many of the seminal ideas which Borel would soon develop. During the period till 1905, he made several contributions of great significance, including the theory of measure, later developed by Lebesgue, the theory of divergent series, and an elementary proof of Picard's Theorem, which mathematicians had sought for over 17 years. The culmination of this period was the beginning of a voluminous series on the theory of functions, edited and directed by Borel, to which he himself contributed 5 volumes. Under his directorship, 50 volumes of these "Borel Tracts" would appear.

His interests broadened during the first decade of this century to encompass probability theory, where he introduced the notion of enumerable probability[1] and his strong law of large numbers. In addition, with money he had received in academic awards, in 1906, he founded the Revue du Mois, a popular monthly, to which he contributed articles of scientific, philosophical and sociological interest. He also undertook the editorship of a series of books intended to

[1]The issue of enumerable probability is concerned with the probability of events which depend upon the totality of an infinite sequence of random variables. While Bernoulli and others had examined this problem using the asymptotic properties of the probabilities, Borel was the first to consider the totality. His theorem showed that the probability for the totality depended on the convergence or divergence of the sum of probability sequences (see Collingwood 1959).

popularize scientific ideas, to which he himself contributed l'Aviation (1910, with Painlevé) and le Hasard (1914). After World War I, he entered public life, becoming a member of Parliament for 12 years, occasionally holding various positions of higher office, and all the while writing prolifically in mathematics. In 1921, having exchanged his Chair of Theory of Functions, at the École Normale, for that of Theory of Probability and Mathematical Physics, formerly held by Poincaré, he began to edit and contribute to the monumental Traité du Calcul des Probabilités et de ses Applications, a series of monographs intended to "organize and expound the whole mathematical theory of probability and its applications as it had developed up to that time" (Collingwood 1959, p. 488). This undertaking occupied the next 15 years till World War II, after which the 74-year old Borel would begin to write another 50 notes, papers and books, mainly presenting mathematical and physical ideas to non-specialist readers.

His first published work on the mathematics of games dates from 1921, by which point he had become absorbed in probability theory. In a series of notes written throughout the 1920's, Borel gives a reasonably systematic, if ultimately speculative, treatment of what would later be known as two-person, symmetric, zero-sum games. He clearly considered this work important enough for presentation to the august Académie des Sciences, to which he had just been elected in 1921: three of the five notes appear in the Academy's proceedings, the Comptes Rendus. As some of his observations are repeated from one paper to the next, we shall confine ourselves to the three most important (1921, 1924, 1927) in an attempt to capture both the letter and spirit of Borel's inquiry. To render this work tractable, we make a distinction between Borel's *specific* speculation on the possibility of a deeper mathematical theorem, which we examine first, and his *general* thoughts, by which is intended his illustration of concrete examples of games, and his musings on the broader applicability of the general framework.

In Borel (1921), the author defines certain concepts and creates the framework upon which the subsequent notes would build, albeit with frequent changes in notation.[2] First, he suggests that we consider a game "in which the winnings depend both on chance and the skill of the players" , unlike such games as dice where skill does not influence the outcome. Defining a "method of play" as "a code that determines for every possible circumstance . . . . what the person should do", Borel asks "whether it is possible to determine a method of play better than all others" (Fréchet 1953, p. 97).

Consider a game with two players *A* and *B*, who choose strategy ("method") C_i and C_k, respectively. Each have the same set of *n* strategies available. Given the strategies chosen, the entries in the matrix represent A's probability of winning the game: no winnings are transferred between players.[3] The numbers α_{ik} and α_{ki} are contained between $-\frac{1}{2}$ and $+\frac{1}{2}$, and satisfy $\alpha_{ik} + \alpha_{ki} = 0$. Also, $\alpha_{ii} = 0$. The game is symmetric and fair.

$$
\begin{array}{cc|cccc}
 & & & \underline{\textit{Player B}} & & \\
 & & C_1 & C_2 & & C_n \\
 & C_1 & \frac{1}{2}+\alpha_{11} & \frac{1}{2}+\alpha_{12} & \cdots\cdots & \frac{1}{2}+\alpha_{1n} \\
\underline{\textit{Player A}} & C_2 & \frac{1}{2}+\alpha_{21} & \frac{1}{2}+\alpha_{22} & \cdots\cdots & \frac{1}{2}+\alpha_{2n} \\
 & & & & & \\
 & C_n & \frac{1}{2}+\alpha_{n1} & \frac{1}{2}+\alpha_{n2} & \cdots\cdots & \frac{1}{2}+\alpha_{nn}
\end{array}
$$

Players are assumed to automatically cast aside "bad" strategies, i.e., methods of play which guarantee a probability of winning of less that half.[4] Having done this, the question is how the remaining strategies might be employed in the best manner possible. Borel suggests that a player can act "in an advantageous

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[2] As far as possible, we use consistent notation. We also use matrices to a greater extent than Borel, in order to portray his ideas more easily.

[3] In his later work, Borel uses payoffs in the matrix, rather than probabilities, in the now conventional manner.

[4] Borel suggests that strategies offering a probability of only $\frac{1}{2}$ should also be rejected, but it is difficult to see why a player would do this and then proceed to seek a guarantee of $\frac{1}{2}$ with the remaining strategies.

manner by varying his play", i.e., play a mixed strategy. C_k is played with probability x_k by A and y_k by B, where

$$\Sigma_1^n x_k = 1 = \Sigma_1^n y_k$$

Given this, A's expected probability of winning is

$$\Sigma_1^n \Sigma_1^n (\tfrac{1}{2} + \alpha_{ik}) x_i y_k = \tfrac{1}{2} + \alpha$$

$$\text{where } \alpha = \Sigma_1^n \Sigma_1^n \alpha_{ik} x_i y_k$$

and B's probability of winning is thus

$$\tfrac{1}{2} - \alpha$$

Borel now considers the case where $n = 3$, given by the following matrix:

		Player B		
		C_1	C_2	C_3
Player A	C_1	$\frac{1}{2} + \alpha_{11}$	$\frac{1}{2} + \alpha_{12}$	$\frac{1}{2} + \alpha_{13}$
	C_2	$\frac{1}{2} + \alpha_{21}$	$\frac{1}{2} + \alpha_{22}$	$\frac{1}{2} + \alpha_{23}$
	C_n	$\frac{1}{2} + \alpha_{31}$	$\frac{1}{2} + \alpha_{32}$	$\frac{1}{2} + \alpha_{33}$

Since no strategy is "bad", it must be the case that α_{23}, α_{31}, and α_{12} are all the same sign. He asks: is there any way that A can choose x_i in such a manner as to ensure $\alpha \geq 0$, for any vector y_k? Alternatively put, if A knows the probabilities that B is going to employ, can he or she find a mixed strategy which will ensure an even chance of winning? Such positive numbers, x_1, x_2, x_3, Borel claims, are always easy to find: in a 2-person, symmetric game, with 3 strategies available to each player, each player can ensure an even chance of victory.

In 1924, Borel extends this analysis, slightly modified, to the case of $n = 5$, i.e., where each player can choose from 5 strategies. Here, the entries in the matrix are the payoffs to player *A*. The same assumptions hold:

$$\alpha_{ij} = -\alpha_{ji} \text{ and } \alpha_{ij} = 0, \text{ if } i=j.$$

$$\begin{array}{cc} & \underline{\text{Player } B} \\ \underline{\text{Player } A} & \begin{array}{cccccc} & C_1 & C_2 & C_3 & C_4 & C_5 \\ C_1 & 0 & \alpha_{12} & \alpha_{13} & \alpha_{14} & \alpha_{15} \\ C_2 & -\alpha_{12} & 0 & \alpha_{23} & \alpha_{24} & \alpha_{25} \\ C_3 & -\alpha_{13} & -\alpha_{23} & 0 & \alpha_{34} & \alpha_{35} \\ C_4 & -\alpha_{14} & -\alpha_{24} & -\alpha_{34} & 0 & \alpha_{45} \\ C_5 & -\alpha_{15} & -\alpha_{25} & -\alpha_{35} & -\alpha_{45} & 0 \end{array} \end{array}$$

Now A's expectation becomes

$$\Sigma_1^5 \Sigma_1^5 x_i y_k \alpha_{ik} = \Sigma_1^5 x_i Y_i$$

where

$$Y_i = \Sigma_1^5 y_k \alpha_{ik}$$

Taking, for analytical convenience, the skew-symmetric matrix of the payoffs to be the following:

$$\begin{array}{cc} & \underline{\text{Player } B} \\ \underline{\text{Player } A} & \begin{array}{ccccc} 0 & 1 & a_1 & -a_4 & -1 \\ -1 & 0 & 1 & a_2 & -a_5 \\ -a_1 & -1 & 0 & 1 & a_3 \\ a_4 & -a_2 & -1 & 0 & 1 \\ 1 & a_5 & -a_3 & -1 & 0 \end{array} \end{array}$$

the Y_i, player B's expected payoff given that A plays strategy i, can be written:

$$Y_1 = y_2 + a_1 y_3 - a_4 y_4 - y_5$$
$$Y_2 = -y_1 + y_3 + a_2 y_4 - a_5 y_5$$
$$Y_3 = -a_1 y_1 - y_2 + y_4 + a_3 y_5$$
$$Y_4 = -a_4 y_1 - a_2 y_2 - y_2 + y_5$$
$$Y_5 = y_1 + a_5 y_5 - a_3 y_3 - y_5$$

Borel now asks: is it possible for player B to choose the vector of y_i such that each of the Y_i is no less than zero? Borel devotes the rest of the paper to showing that appropriate probabilities y_i can always be found. Depending on the values of the a_i in the payoff matrix, B can keep all the Y_i to zero, thus ensuring no advantage to A. Borel concludes that with n = 5, "nothing essentially new happens compared to the case where there are three manners of playing" (p. 114), i.e., each player can ensure an expected payoff of zero. But speculating as to whether this is likely to hold for n arbitrarily large, Borel is pessimistic and suggests that it will not always hold. However, three years later, in another note (Borel (1927)) presented to the Académie, he reports an extension of his analysis giving rise to greater optimism: what has held for 3 and 5 strategies seems also to hold for 7, and it would thus "be interesting either to demonstrate that it is unsolvable in general or to give a particular solution" (p. 117).[5]

While Borel may have concluded his search for a deeper theorem on a speculative note, his work of the 1920's is notable in certain other respects, especially in the provision of general concepts and examples. First, introducing the infinite game, where strategies are drawn from a continuum, he shows how the continuous analogue of player A's expected payoff is expressible as a Stieltjes integral:

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[5] Borel's analysis is confined to games with odd numbers of strategies because of his use of determinants of skew symmetric matrices to calculate the optimal mixed strategy. Using this approach, he could never have proved the theorem's validity for all games.

$$\alpha = \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} f(C_A, C_B)\, d\phi_A (C_A)\, d\varnothing_B (C_B)$$

where $f(\,)$ is the function relating A's payoff to the strategies chosen, and $\varnothing_A(\,)$ and $\varnothing_B(\,)$ are A's and B's respective cumulative distribution functions over strategy space. The example offered of such a game is what later was to become known as a game on the unit square: in this case, each player chooses three real numbers summing to 1, the winner being the one with two choices of greater value than the opponent's. While Borel simply describes the game here, a decade or so later, in 1938, his student Jean Ville would extend the minimax theorem to such games.

Second, Borel looks at specific examples of finite games such as "Paper, Scissors, Stone" (p. 102) and shows in detail how the calculation of the optimal mixed strategies depends on relative payoffs. For example, if the payoff to A for a particular strategy is relatively large, then the probability attached to it in the optimal mixed strategy will be correspondingly low: otherwise, B could gain by anticipating A's emphasis on the favored strategy.

Third, he considers the broader application of these ideas to the non-mathematical realm:

> "The problems of probability and analysis that one might raise concerning the art of war or of economic and financial speculation are not without analogy to the problems concerning games" (p. 100)

He cautions restraint in this regard, however, saying that such matters are highly complex, and that mathematical calculation can at best be a supplement to strategic cunning. As we shall see below, this trace of scepticism in Borel would grow even stronger with time.

In his final related communication to the Académie, in May 1928, Borel supplied the answer to the question that had occupied him in the notes discussed above, i.e., concerning the existence of a "best" way to play. However, this clarification came not as a result of further work by Borel himself, but in the shape of a note from somebody who claimed to have proved the minimax theorem in Gottingen two years previously. This, of course, was John von Neumann, then in his mid-twenties and 30 years Borel's junior (see von Neumann (1928a)). Before turning to von Neumann, however, we should consider the related contemporaneous work of Hugo Steinhaus, at Lwów in Poland. While Borel and von Neumann were at least aware of each other's work and common interest, Steinhaus, it appears, labored in total isolation.

A Pole Apart:

The period between the two world wars was one during which mathematics flourished in Poland (see Kuratowski 1980). Under the tutelage of such figures as *Zaremba and Sierpinski, there emerged from the universities in Lwów and* Wroclaw a number of capable young mathematicians including Banach, Ulam and Steinhaus. Born in 1887 in Jaslo, Poland, Steinhaus spent one year studying at the university in Lwów and then took off to Gottingen, where he completed his doctorate in *1911 under David Hilbert. Following this,* he lived as an independent scholar in Jaslo, until the outbreak of war, upon which he joined the Legions. In 1916, he joined the faculty at Lwów, became full professor by 1923, and stayed until further interrupted by war in 1941. Following World War II, *in 1945, he moved to the scientific center at* Wroclaw, where he remained until his death in 1972. In both Lwów and Wroclaw, Steinhaus was a leader among the groups engaged in mathematical research (see Kac 1985).

Steinhaus's mathematical interests were wide-ranging in both theoretical and applied areas. In the former, he was active in the theory of trigonometrical series, functional analysis, orthogonal series, the theory of real functions and, perhaps most famously, in sequences of linear operations, for which he is remembered as a collaborator of Banach. In applied areas, he published on probability theory and on the application of mathematics to questions in medicine, electricity, biology, geology and anthropology. It was, no doubt, this taste for the mathematics of applied problems that led Steinhaus to games.

The work in question is a single article, "Definitions for a Theory of Games and Pursuit", which appeared in 1925 in the first issue of "an ephemeral pamphlet called 'Mysl Akademicka'" (Steinhaus 1960, p.108). A short-lived periodical edited by Lwów students, its first issue was also its second-last! Here, Steinhaus claims that he is concerned with the construction of mathematical definitions for a group of problems which lie "beyond the strict boundaries of mathematics" (p. 106). The problems discussed are chess, naval pursuit and card-playing, and the thread which binds them together is their use by Steinhaus to motivate the notion of minimax play (without actually terming it such). He introduces the concept of a "mode of play" which connotes, for either player, "a list of all possible circumstances with a preferred move for each" (p. 106). Considering chess, he modifies the game by placing a limit, known to both players, on the total number of moves permitted. Should White not win before this limit is reached, then Black wins. Black's aim, therefore, is to adopt the strategy which prolongs his defence, while White's is to keep the length of play as short as possible by winning as quickly as possible. Given that Black chooses a strategy to maximise the duration, White chooses the strategy which keeps this maximum to a minimum. In exactly the same manner, Steinhaus discusses two ships in pursuit: one chasing, the other fleeing, both at a given speed. The pursuer's aim is to close the angle between its line of steering and line of sight, thereby minimising the time in pursuit. The evader's aim is the opposite. Each ship's strategy is a function of both ships's coordinates. At any moment, given that the evader has chosen the time-maximising strategy, the hunter will respond by attempting to minimise it. Finally, Steinhaus notes that in card games a similar tussle is involved, as each player tries to reduce the expected gain of their opponent.

While conceding that only definitions have been provided, Steinhaus notes that these are essential for the next stage, the calculation of best play. However, actually finding the "best move", "best pursuit" or "best way of playing", involves "enormous difficulties". Pursuit, for example, would require us "to use the calculus of variations on a very difficult problem of mathematical analysis", while even the simplest card games "lead to very involved combinatorial calculations" (p. 107). Had Steinhaus learnt the following year that the mathematical consideration of games was the subject of discussion among the

Gottingen group, centered on his doctoral supervisor Hilbert, his paper might not have been forgotten. As it was, it gathered dust for decades.[6]

New Boy at Göttingen:

Born to a wealthy, Jewish, banking family in Budapest in 1903, "Jansci" Neumann was the eldest of the three brothers.[7] Among these he was "the most aggressive one, the least sentimental, the most thoughtful", staying away from childish games, preferring to read, study or calculate or, during World War I, play "elaborate battles with toy soldiers" (Heims 1980, p.41). The young von Neumann was precocious and shortly after entering the Lutheran Gymnasium at age 10, was recognized by his mathematics teacher as a child prodigy. At the teacher's suggestion, the child's father arranged to have him receive additional tutoring from a mathematics lecturer at the University of Budapest and, by the time he left secondary school, he had won the nationwide Eotvos prize for "excellence in mathematics and scientific reasoning" (p. 44).

In 1921, at 17, von Neumann enrolled in the University of Budapest but promptly left the country for Berlin, thereafter returning to Budapest to take the necessary university exams or visit his family.[8] Spending 1921-1923 at the University of Berlin, he was influenced by David Hilbert's former student,

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[6] Steinhaus's paper was not translated into English until 1960, when it was published in the Naval Research Logistics Quarterly. The paper is introduced there by Harold Kuhn who explains that the Polish version was secured by Stan Ulam. An early reference to the paper appears, however, in a letter from Oskar Morgerstern to Olaf Helmer at RAND in Oct. 1952, in which he states that a friend was writing to Steinhaus for a copy of the paper (see OMPD, Box 14, File RAND). In an letter accompanying the published paper, Steinhaus offers some background to his search for the paper. Not until 1957 did he retrieve the paper, and then only through a colleague who secured a copy of the journal in Lwów, which was by then part of the Soviet Union.

[7] The family name Neumann became von Neumann when John's father, Max, was ennobled by Emperor Franz Joseph in 1913 "for his contributions to the economic development of Hungary" (see Asprey 1990, p.254 n.4; Heims 1980, pp. 29-32).

[8] In Fermi (1968), Eugene Wigner, Hungarian contemporary of von Neumann and later atomic physicist in the U.S., comments on the tendency for young Hungarians to leave early:

> "[While] Hungarian high schools were excellent, . . .the universities were very poor. . . .It was symbolic of the bureaucratic influence . . . that professors at universities kept their libraries locked up and a student who needed to consult a book was often obliged to borrow the key from his professor" (p. 54).

Erhard Schmidt, and by Albert Einstein from whom he took lectures on statistical mechanics. He kept company with fellow Hungarian émigres including Eugene Wigner, Leo Szilard and Dennis Gabor, all part of the "Hungarian Phenomenon", which was later to have an enormous impact on American physics and mathematics. During this time, von Neumann also made contact with Hilbert at Göttingen, the "mecca of German mathematics", beginning a collaboration which would last several years, and influence von Neumann for life. From Berlin, he went to the Swiss Federal Institute of Technology at Zurich where he took a degree in chemical engineering in 1925. There he fell in with George Polya, another Hungarian mathematician who would later come to Stanford University, and Hermann Weyl, a German mathematician who was to be one of the first residents at Princeton's Institute for Advanced Study (IAS). In 1926, von Neumann received his doctorate in mathematics from Budapest and spent the rest of the year in Gottingen, and the following year was appointed Privatdozent at the University of Berlin. Throughout this entire period, he maintained his close contact with Hilbert.

Given the importance of von Neumann in our story, it is worth portraying the historical developments in mathematics in which his mentors at Göttingen had crucial roles.[9] Over the nearly 50 years from 1895, during which he reigned at Göttingen, Hilbert made mathematical contributions which arguably, among mathematicians, exalted him to the rank of an Archimedes, Newton or Gauss. By 1902, he had made his mark on, *inter alia* , invariant theory, the calculus of variations and the foundations of geometry. In the latter, his axiomatization of Euclidean geometry (The Foundations of Geometry, 1899) signalled the beginning of a lifelong preoccupation with the way in which proofs in mathematics are related to the axioms on which they are based. True rigor, for Hilbert, required that axioms be complete, in the sense that all theorems be derivable from them; independent, in that the removal of any axiom would make it impossible to prove at least some of the theorems; and consistent, so that no contradictory theorems could be established using such axioms. In this, he was further galvanized by

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9 For a fine account of this period, the reader is referred to Constance Reid's (1970) biography of Hilbert.

Zermelo's and Russell's independent observation of a fundamental antimony, or paradox, in set theory, and called for the mathematical investigation of proof itself.[10] This became known as metamathematics, or proof theory. Just as Hilbert was concerned with the logical rigor of mathematics, so too was he disturbed by the apparent lack of order in the constructions of the physicists, which at this time were growing rapidly. The turn of the century saw Hertz's proof of the existence of electromagnetic waves; Roentgen's discovery of X-rays; the Curies' radioactivity; J.J. Thomson's electron; Einstein's special theory of relativity; and Max Planck's quantum theory. Hilbert "perceived the pressing necessity for investigation to determine whether these diverse principles were compatible with one another and in what relation they stood" (quoted in Reid (1970), p. 127). Thus, beginning in 1912, he turned his attention to the mathematics underlying purely physical phenomena, beginning with kinetic gas theory and then elementary radiation theory, in each case constructing, from the axiomatic base up, a mathematical theory consistent with the physics.

After World War I, Hilbert's attention reverted to the exploration of foundations in response to the view of mathematics known as Intuitionism, then being propagated by Dutch mathematician L. Brouwer. Briefly stated, this rejected mathematical objects the proof of whose existence depended on an infinite number of steps. Any existence proof which implicitly depended on a greater than finite number of steps could not be regarded as constructive since the existent, even in principle, was unattainable.[11] Logically, this meant the rejection of much that classical mathematics took for granted, including the Principle of the Excluded

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[10] Russell's Paradox was put forward in 1902. Some sets are not members of themselves, e.g. the set of all women. Other sets are members of themselves, e.g. the set of all things that are not women. If one considers the set consisting of all sets who are not members of themselves, one finds the paradox that if it is a member of itself, then it is not, and vice versa. Antinomies such as these gave rise to restrictions on the use of general properties to define sets.

[11] Reid (p. 149) offers the following example: consider the statement A "There is a member of the set S having the property P". If S is infinite, each member of the set cannot, even in principle, be examined to verify the statement. Brouwer rejected such existential statements for infinite sets. Now the Principle of the Excluded Middle says that if A holds then ~A does not hold, and vice versa: there is nothing in between. Brouwer argued as follows: if in S above one finds one element showing property P, then the first alternative is substantiated. If however, one does not, then the second alternative is still not substantiated, and nor has the middle been excluded.

Middle, Cantor's theory of infinite sets, and many existence proofs. To Hilbert, who considered such concepts central, the denial of all this to mathematics was akin to "prohibiting the boxer the use of his fists" (Reid, p. 149). By 1922, his agitation was heightened further by the degree to which Intuitionism seemed to be taking hold among younger mathematicians, such as Weyl, and, turning from physics, he threw himself at his work on axiomatics with renewed vigor. Mathematics, if it were to remain intact, had to establish deductions with the same certitude that existed for the arithmetic of whole numbers "where contradictions and paradoxes arise only through our own carelessness" (quoted in Reid, p. 176).

Meanwhile, Hilbert's colleagues were contending with the proliferation in physics, which by 1926 had become even more notable. Heisenberg's new theory of quantum mechanics was shown to be explicable in terms of matrix methods by Max Born. Then Schrodinger, at Zurich, constructed his wave mechanics which, while it led to the same results as Heisenberg, proceeded from an entirely different base. The two were soon mathematically reconciled by Courant, using, to a great extent, Hilbert's earlier work on integral equations and infinitely many variables (Hilbert Space). These radical developments in physics challenged prevailing views in mathematics. The theory of relativity deeply questioned many concepts that were central to classical mechanics, such as absolute space and time, simultaneity, etc. Quantum theory, more importantly, threw mechanistic determinism into disarray by demonstrating the impossibility of knowing simultaneously both the position and velocity of a particle, without which its future evolution could not be predicted. Such basic contradictions called for fundamental changes in the mathematics used, to the chagrin of classical mathematicians such as Poincaré and Volterra, who believed in the underlying continuity of physical events and the possibility of their representation by the infinitesimal calculus and the theory of differential equations. As Ingrao & Israel (1990) point out, the emphasis shifted from *mechanical* analogy to *mathematical* analogy. Mathematics was to be used for its form, to provide a language unifying theories, rather than to mechanically describe physical processes.

If firm tremors, therefore, were being felt at this time throughout the mathematical world, the Göttingen to which von Neumann was first directly introduced in 1923 was at the epicenter. His contributions for the next few years reflected the concerns of the time and the place. Hilbert's twin concerns of the foundations of mathematics and the axiomatisation of mathematical physics greatly influenced him (see Heims 1980, Goldstine 1972). Working with Lothar Nordheim, Hilbert's assistant in physics, von Neumann undertook the axiomatisation of Heisenberg's work, and then proceeded to develop further the notion of the Hilbert Space to provide a fuller mathematical basis for quantum mechanics. His seminal work on the axiomatisation of physics appeared in the form of three articles in 1927, three in 1929, and their condensation into a book, Mathematical Foundations of Quantum Mechanics (1932). In 1927 also, influenced by Hilbert's concern with the foundations of mathematics, he published a paper conjecturing that all analysis could be proved consistent. At some point in 1926, von Neumann produced his proof of the minimax theorem which, not surprisingly, was overshadowed by his contemporaneous work (see Heims p. 56). The source of his interest in games thus remains something of a mystery. However, his use of the axiomatic approach is entirely in keeping with the Hilbertian ethic with which he was fully imbued, and the notion of chance, made central through probabilistic play, is consonant with the indeterminism at the basis of quantum mechanics:

> "chance . . . is such an intrinsic part of the game itself (*if not of the world*) that there is no need to introduce it artificially by way of the rules of the game: . . . it still will assert itself".
>
> Von Neumann (1928b, p. 26, emphasis added)

As noted above, there are two publications by von Neumann dealing with the minimax proof (1928a, 1928b), the first of these being the communication to Borel presented by the latter to the Académie. In it, von Neumann refers to Borel's work in this vein since 1924 and claims he has solved the problem of the existence of a best way to play the 2-person, zero-sum game. He points out that he reached these results independently: "*M'étant occupé indépendamment avec le même problème*" (p. 1689), and that the full proof is forthcoming soon as "Zur

Theorie der Gesellschaftsspiele" in Mathematische Annalen, the Göttingen journal (which by this time had accepted it for publication).

The latter, von Neumann (1928b), appeared later that year, primarily containing a long and difficult proof of the existence of an equilibrium value for the two-person, discrete game, based on functional calculus and topology. The paper reveals little about whose work, if anybody's, von Neumann draws upon and there is no reference to the past, except in two footnotes. The first of these says that the paper had been presented in shorter form in December 1926 to the Göttingen Mathematical Society.[12] The second states that while finalising the current paper he "learned of the note of E. Borel in the Comptes Rendus of Jan. 10, 1927" (p. 25). In the paper, the concept of a game is completely axiomatised and two examples are offered of zero-sum games with solutions only in mixed strategies.[13] Among the situations which can be regarded as games of strategy are roulette, chess, baccarat, bridge, and "the principal problem of classical economics: how is the absolutely selfish 'homo economicus' going to act under given external circumstances?" (p. 13).[14] Von Neumann also treats the 3-person, zero-sum game, showing how the possibility of coalition formation introduces a measure of indeterminacy, or "struggle" into such games. In preliminary remarks on games with more than 3 players, he broaches the topic to which von Neumann & Morgenstern (1944) would later devote much space. Without calling it such, he introduces the characteristic function; a "system of

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12 This was undoubtedly the Gottingen Mathematical Club, "the highpoint of the mathematical week . . . during the 1920's" (Reid 1970, p. 168). This was an informal gathering where faculty and students would give talks on their recent work.

13 The two games illustrated are Matching Pennies (here the payoffs are given but the name is not used) and Morra (also called Paper, Stone, Scissors). The latter is one of the games considered by Borel in his 1924 note.

14 This is the first indication of von Neumann's interest in economic matters. Nicholas Kaldor (in Dore 1989) recalls von Neumann expressing such an interest in 1927, not long after his Berlin appointment. They met while returning on holiday to Budapest as young scholars working abroad. Kaldor suggested he read Wicksell's Value, Capital and Rent, which provided an introduction to Walras and utilised Bohm-Bawerk's capital theory. Von Neumann, on reading this, criticised the Walrasian system, saying that it permitted negative prices. Kaldor speculates that this may have prompted von Neumann to write his 1937 paper on economic growth, and that furthermore, von Neumann's interest in economics may well have grown out of his interest in game theory.

constants" describing "the sum per play which [each] coalition of the players . . . is able to obtain from the coalition of the other players", and conjectures "*that the complex of valuations and coalitions in a game of strategy is determined by these. . . constants alone*" (p. 40-41, emphasis added). If this is the case, then "we have brought all games of strategy into a natural and final normal form". In conclusion, von Neumann adds that a later publication will contain numerical calculations of a simplified Poker and Baccarat, the results of which are corroborated by the well-known necessity to "bluff" in poker.

The French Connection:

We observed above that von Neumann's communication with Borel was minimal, peremptory, and more in the nature of a rebuff than an invitation to exchange ideas. Under these circumstances, it is interesting to enquire as to what subsequently happened in the 1930's to the latter's work on games. Did he, for example, incorporate von Neumann's result into his analysis? Or did he take up the framework suggested in the 1928 paper? However even these questions suggest too smooth and seamless a conception of the history of ideas: what happened, briefly, was that Borel seems to have gradually lost interest; his student, Jean Ville, took up the cudgel, doing some further original work; and one of his contemporaries, René de Possel, brought the good news in popular form to the French intelligentsia. *And all this went by, it appears, unbeknownst to von Neumann.*

In 1936, mathematician René de Possel wrote what might be regarded as the first popularisation of von Neumann's minimax theorem. This came as part of a series of monographs on original contributions to science, philosophy, literature and art, produced by the Centre Universitaire Méditerranéen de Nice, and edited by the poet Paul Valéry. These were clearly intended for the well-educated, but non-specialist, reading public. De Possel's booklet is a forty-page description of the analysis of popular games, in which von Neumann's theorem is presented as the culminating achievement to date. Following Borel (1924), games are divided into those based on pure strategy, those based solely on chance and those involving "ruse", or bluff, where a player can gain an advantage by knowing the opponent's intentions. De Possel discusses various examples. For example, in the game of "batonnets" each player draws a number of matches from a pile until it is depleted. No more than a certain number may be drawn each time and the last person to draw wins the game. Based on the number of matches in the pile, an optimal strategy exists for at least one of the players, demonstrable by backward induction. Various versions of this purely strategic game are discussed. Roulette is presented as a game of pure chance. In this context, he explains the idea of the martingale, or how best to spread one's bets across several rounds, given the capital at one's disposal and the unfolding pattern of wins and losses. The ubiquitous "Baccarat du Bagne", or "Scissors, Paper, Stone", is presented as an example of a "social game", which combines strategy, chance and "ruse", and von Neumann (1928) is invoked to show how minimax play is optimal, in that it eliminates the risk of the opponent guessing one's intentions. While the theorem itself is "too technical to be reproduced here" (p. 39, author's translation), it is clearly the booklet's central feature, and von Neumann is honored as "the first to seek to penetrate the mechanism of play from such a general perspective" (p. 5, author's trans.).

However, no such credit is afforded von Neumann in Borel's work of the same period. In 1938, as part of his extended treatise on probability, Borel contributed a volume on "Applications to Games of Chance". This is a version of his course on the topic at the École Normale in 1936-37, written and edited by his student Jean Ville.[15] Following his by now standard approach, Borel initally examines, at length, dice and simple card games, where chance is the dominant feature and the analysis is confined to combinatorial probabilities. Then, in a chapter on "Games where psychology plays a fundamental role", he synthesizes and extends his work of the 1920's. Again, both finite and infinite games are analysed. In the case of the former, the usual suspects are featured, i.e., "Heads-or-Tails" and "Baccarat du Bagne", and the determination of optimal mixed strategies discussed in detail. Remarkably, however, *no* mention is made of what had become, for de Possel at least, the "theoréme fondamental", i.e., von Neumann (1928). This can only be regarded as an act of deliberate omission by Borel.

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[15] It was, and to some extent still is, quite common for some French professors to have a *protégè* take careful class notes for later publication.

Indeed, he devotes most attention to continuous games. For example, in the game where two players must each choose a point on a circle, some fair criterion determining the winner[16], simple integration is used to construct the distribution that each should optimally apply in selecting a point: the density is uniform so all points are afforded the same probability. This is extended to the choice of three points from a continuum by each player and the same mathematical justification is provided for the intuition of random play. Once again, Borel considers situations to which these principles are connected, in both the military and economic arenas. For example, he suggests that the analysis might be applied to the allocation, by opposing armies, of their respective forces to a limited number of common strategic points, or to the problem of how two merchants, wishing to sell equal stocks of similar goods, should distribute their available discounts across the goods, given that they are competing for customers.[17] In a final chapter, a simplified poker is analysed, showing the importance of bluffing. It is possible that this was prompted by von Neumann's earlier indication to Borel that he had done such work.

Following the above, and occupying but 9 of the book's 120 pages, is a note by Borel's student Jean Ville, which, in the purely mathematical sense, was to have a greater subsequent impact on the consolidation of the theory than the rest of the book. This, of course, is Ville's construction of the first elementary proof of the von Neumann theorem for finite games, and his own extension of this to show that a simple infinite game also has a value. Ville's proof is partly topological and rests on a theorem on linear forms in non-negative variables. Compared to von Neumann's earlier contortions, it is positively elegant.

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[16] On a fixed circle, a chosen point wins if the other choice lies within an arc of length p, in the counter-clockwise direction. Otherwise, by definition, the other choice wins.

[17] It is this military example that would later feature as the Colonel Blotto game, first appearing in Morse & Kimball (1946) and then developed at RAND in the late 1940's, ostensibly to consider the global allocation by the U. S. and Soviet Union of their respective armaments. See Dresher 1961.

Consider p linear forms in n variables:

$$f_j\,(x) = \Sigma\ a_{ij}\ x_i \qquad (j=1\ldots p;\ i = 1\ldots n)$$

with the property that, whatever the non-negative values of x_i, there exists among the f_j at least one which is non-negative. Then, there exists at least one system of non-negative coefficients $Y_1 \ldots Y_p$, $(\Sigma Y = 1)$, such that $\Sigma Y_j f_j$ is non-negative for all non-negative values of variables x_i. Having proved this, Ville then establishes a corollary: in the same system, if, whatever the non-negative values of x_i, there exists at least one f_j no less than φ, itself a linear form in the same variables, then there exists a linear combination:

$$\psi = Y_1 f_1 + Y_2 f_2 + \ldots + Y_p f_p \qquad \text{with} \quad Y_j \geq 0, \quad \Sigma Y_j = 1,$$

such that

$$\psi \geq \varphi \qquad \text{at every point } x, \quad (\text{where } x_i \geq 0,\ i = 1 \ldots n)$$

Having defined a game, and shown each player's expectation, conditional on the opponent's strategy, to be a system of linear forms, Ville simply invokes the above theorem and corollary to show the existence of a value, which represents at once an assured upper limit on the minimizing player's expectation and a lower bound on that of the maximizing player. He then, for the first time, draws the infinite game into the compass of the minimax concept. Considering a simple game where each player chooses a point from the unit interval, Player A's expectation is given by

$$\int_0^1 \int_0^1 K(x,y)\ dF\ d\Phi$$

$K(x,y)$ being A's payoff given choice of points x and y; F and Φ being the probability distribution functions applied to the strategies chosen by A and B, respectively. Ville shows that if $K(x,y)$ is continuous in x and y, in the closed domain $0 \leq x \leq 1$, $0 \leq y \leq 1$, then this game too has a value. This he does by establishing this infinite game as a limiting case of a finite game with very many strategies.

Following this, Borel offers some observations on his student's contribution which unambiguously reveal his doubts about the entire theory. Having thanked Ville for illustrating von Neumann's "important theorem", he says:

> "It appears essential for me to indicate, however, to prevent all misunderstanding, that the practical applications of this theorem to the actual playing of games of chance is, for a long time, unlikely to become a reality"
> (p. 115)

Actual games are exceedingly complicated, he says, containing many coefficients and equations. Even if one could simplify a game sufficiently to the point where such calculations were possible, the advantage of playing according to the above theorem are only attainable on average, after a great number of rounds. And even taking account of the experienced recommendations of players to locate what might be regarded as reasonable strategies,

> "there still remains such a great number of variables that even the task of writing the equations, not to mention that of solving them, appears absolutely insurmountable" (p. 115)

In Borel, one senses a respect for games, many of which are old and have been played for generations. In many such games, a consensus may emerge on what constitutes good play, and thus what the novice should be taught. However, no sooner has such agreement been reached than the better players take advantage of it to introduce newer, more successful, ways of playing. This is what makes games interesting. Indeed, it often goes full circle, with today's innovators reviving ways of playing that were once considered revolutionary but then abandoned. Can it ever be hoped, he asks, that this natural evolution will approximate the impracticable solution of a system of equations which completely describes the game?

> "This, I must admit, seems highly doubtful to me, and, anyway, if it happened for a particular game, it is almost certain that the game would soon be abandoned for a more complicated one" (p. 116)

Furthermore, even where ideal play involves a probabilistic element, it is very difficult not to follow some regularity when actually playing. This is particularly important in bridge, for example, where probabilistic play intended to defeat one's opponents may well mislead one's partner also!

> "All these remarks. . . . are obvious to anyone with some experience in games. Perhaps they will make clear, to those uninterested in games, how enjoyable games are as leisurely distraction, at the same time showing to those who would wish to turn games into an occupation, how futile is the search for a perfect formula which is forever likely to elude us." (p. 117)

These remarks effectively signal the end of Émile Borel's active contribution to game theory.[18] They both constitute the first criticisms of the minimax idea, and suggest his unwillingness to sacrifice the mystery and delight of games for an elegant but inapplicable mathematics. His is a refusal to take it all too seriously.

Von Neumann in the 1930's:

After a one-semester visit to Princeton in 1929 to lecture on quantum theory, at the invitation of geometer and topologist Oswald Veblen, von Neumann alternated between there and Berlin, continuing his work towards the Mathematical Foundations of Quantum Mechanics (1932). During this period, his interest in economics was further stimulated and, in fact, the minimax idea resurfaced in the context of his 1937 growth model. At Menger's seminar at the University of Vienna, a sequence of papers presented between 1932 and 1937 dealt with equilibrium and growth in Cassel-type models (see Weintraub 1985, pp. 72-78). These were usually published a year later in the Ergebnisse (trans. Proceedings), edited by Menger. Karl Schlesinger (1932) offered a reformulation of Cassel using inequalities and set forth a model, without mathematical analysis. Following this, Wald, Menger's student, proved the existence of a solution to Schlesinger's model (the first existence proof) and modified this further in 1936. Von Neumann (1937) removed the distinction between primary factors and outputs: all goods are produced. Rather than

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[18] Subsequent publications merely reiterated his earlier work, e.g. Borel 1950, Appendix 1

emphasise production of single goods, he uses processes: one process may produce multiple outputs and each output may result from different processes. He then characterises the equilibrium rate of growth as the saddle point of a function relating the input and output matrices, its existence proved using Brouwer's fixed point theorem.[19] Apparently, von Neumann had presented the paper at the Princeton Mathematical Society in 1932 and, unlike the other papers in the series, did not present it to the Vienna seminar.

The 1930's, therefore, saw the affirmation of von Neumann's interest in theoretical economics and, among economists, it is for his contribution on the growth model during this period that he is remembered. It is also true, however, that the "pure" mathematics of games was on von Neumann's mind during this time. In April 1937, in its mathematics section, Science Letter News reported a talk by von Neumann at Princeton on what for him was "a mere recreation", his analysis of games and gambling. Apparently, he spoke about "stone-paper-scissors", showing that by "making each play the same number of times, but at random, ... your opponent will lose in the long run". Also parsimoniously reported are his observations on the probabilities of making particular plays in both dice and a simplified poker. Two and a half years later, in November 1939, von Neumann was planning a visit to the University of Washington, Seattle, where he was to spend part of the upcoming Summer semester as Walker-Ames professor in mathematics. In a letter to the department, he suggests possible topics for his lectures, including quantum theory, operator theory, groups, and the "Theory of Games". On the latter he says:

> "I wrote a paper on this subject in the Mathematische Annalen 1928, and I have a lot of unpublished material on poker in particular. These lectures would give a general idea of the problem of defining a rational way of playing" [20]

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[19] Despite apparently taking Cassel's work as a starting point, the paper contains no reference to the related work of Schlesinger or Wald. The latter and Menger, however, accepted the paper for publication without excercising their editorial preregative. Arrow 1989 notes that "von Neumann's lack of references is in general a source of difficulty in reconstructing the evolution of ideas" (p. 17).

[20] Von Neumann papers, Library of Congress, Container 4, File 3, Personal Correspondence 1939-40.

Finalising matters four months later, in March 1940, he indicated that he would give 3 evening lectures on games:

> 1. The general problem. The case of chess.
> 2. The notion of the "best strategy".
> 3. Problems in games of three or more players. General remarks.[21]

The extent to which Seattle's mathematicians were stirred by von Neumann's still quirky ideas is unknown. What is clear, however, is that just prior to this sojourn he had captured the imagination of one distinctly non-mathematical economist, who had moved to Princeton from Vienna two years previously, Oskar Morgenstern.

Morgenstern's early career:[22]

Born in Silesia, Germany, in 1902, Oskar Morgenstern moved at twelve years of age with his family to Vienna where, in 1925, he obtained his doctorate with a thesis focusing on marginal productivity. Following three years visiting London, Columbia and Harvard universities, Paris and Rome, as a Rockefeller Fellow, he was appointed Privatdozent at the University of Vienna in 1929. Morgenstern's habilitation thesis and first book, Wirtschaftprognose (trans. Economic Prediction) (1928), focused, in the Austrian tradition, on epistemological difficulties in economic forecasting: the difficulties of "knowing" when "other wills", other "economic acts" may interfere with, or enhance, one's own plans" (Morgenstern 1976, p. 806). During the 1930's, his work concerned such issues as the business cycle, methodology and the treatment of time in economic theory. He also had some work published in the U.S. and both George Stigler and Frank Knight were familiar with his work. His main professional activity was

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21 Loc. cit.

22 For the fullest extant treatment of the life and work of Oskar Morgenstern, the reader is referred to Rellstab (1991a, 1991b), both of which are part of a larger project. In gaining an understanding of Morgenstern, I have benefitted tremendously from conversations with, and the generosity of, Urs Rellstab.

the directorship of the Vienna Institute of Business Cycle Research and he also acted as editor of the Zeitschrift fur Nationalokonomie.

It is easy to look back at his work and pick out those elements which are most congruent with what later appeared in the Theory of Games. However, I believe his position as co-author with von Neumann is better understood by focusing not on particular incipiently "game-theoretic" concepts he may have alluded to in his earlier career, but rather on his general position as an arbiter of ideas, an intermediary between theorists in disparate fields, and one ultimately most capable of giving his energy to penetrating criticism rather than alternative theoretical construction. First of all, Morgenstern was primarily an outsider: neither intellectually nor psychologically did he fit comfortably into any particular group or school of thought. He attended the meetings of the Vienna Circle and was much taken by the philosophical flux centered on Schlick and Carnap, but he was not a philosopher, and he discussed these developments at a distance. Neither was he a mathematician, and while he also attended the Menger colloquia and debated issues in mathematics and their relationship to the social sciences, his relationship to mathematics remained akin to that of the *impresario* to the music.[23] Second, Morgenstern's intellectual interests, while varying in depth, were certainly broad, and he consistently attempted to relate disparate developments in philosophy and mathematics to economic theory. In "The Time Moment and Economic Theory" (Morgenstern 1935b), the work of Karl Menger and Moritz Schlick in logic form the background for his critique of the treatment of time in neoclassical economic theory. Similarly, in " Perfect Foresight and Economic Equilibrium" (1935a) and "Logistics and the Social Sciences" (1936), he contends that only by incorporating recent developments in logic can economics achieve what he saw as the necessary level of rigor. The need for mathematical rigor he emphasised incessantly in both his public remarks and personal reflections:

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[23] I believe his authorship of the Theory of Games created a false impression about his capabilities in this regard. In presentations on the subject for years afterwards, Morgenstern felt obliged to preface his remarks with the observation that they would be non-technical. Only by 1964, as the dusk of his career approached, did he say bluntly: "Mathematical intuition is a very important thing. I wish I had some." (Mensch 1966, p. 100).

> " When I ask myself what I consider to be my main duty working on economic problems, it is the introduction of truly exact reasoning and truly exact methods."
>
> (OMPD, Diary, Box 13, April 19, 1936)[24]

Broadly familiar with the developments in mathematics of Russell, Hilbert and their contemporaries, he felt that the implications for economic theory were enormous. Indeed, he sought to overcome his deficient training in mathematics by taking private tutorials with Abraham Wald, whom he appointed as researcher at his Institute, but he remained personally incapable of taking the theoretical steps that he himself envisioned.[25] Indeed, if there is an irony which characterises Morgenstern's career, it is that, in his continuous agitation for mathematical rigor in economics, he was ultimately calling for a theoretical approach in which thinkers of his own kind would have increasingly little place.

In 1938, he visited the U.S. with the support of the Carnegie Endowment for International Peace and soon found himself unable to return, having been dismissed by the Nazis in his absence. When offered a three-year appointment in Political Economy at Princeton, he accepted. His decision to stay in the U.S. was not one reached suddenly, however, for two years earlier he had written to Frank Knight:

> "The idea of being a professor in a large and reputable American University appeals to me very much. You know very well, that I have a high regard for research in social sciences in the States and I have tried to transplant a good deal of the American method to Vienna. . . . Anyway, I wanted to make clear to you that my reaction to your question whether I would like to come to the States is possitive [sic] and I wish you would let me know if there are any further developments."
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> (OMPD, Box 6, Corresp. 1928-1939, Knight, F., April 6, 1936)

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[24] OMPD: Oskar Morgenstern Papers, Duke University

[25] *Morgenstern hired Wald at the suggestion of Karl Menger, the latter's teacher.*

One should note, however, that even though he had been made aware of von Neumann's 1928 paper on games by the Czech mathematician Eduard Cech, he did not read the work until he met the former in Princeton at the end of 1930's.

Collaboration at Princeton:

By 1938, therefore, both Morgenstern and von Neumann were at Princeton. One an economist, the other primarily a mathematician, they differed in many ways, but their common situation likely eclipsed differences that back in Vienna or Gottingen might have been more significant. Thus the two always spoke German together, even when writing in English, and there were dinners and conversation with Einstein, Bohr and Weyl (Morgenstern 1976). A glowing account of their collaboration on the Theory of Games and Economic Behavior (1944) is given in Morgenstern (1976). This reminiscence, while it may capture Morgenstern's nostalgia as he looked back at the high point of his career, is at odds with the version of events actually recorded in his personal journal at the time of his work with von Neumann during World War II.

Our concern here is to understand how their book reflects the different interests of the two authors and how it relates to the explorations discussed above. As mentioned above, von Neumann and Morgenstern met in the Fall of 1938, shortly after the latter had arrived at Princeton. Despite several further meetings in the interim, not until April 1940 did they actually discuss games and economics, by which time, we have noted, von Neumann had already arranged his lectures on games for Seattle. During these discussions, von Neumann, we are told, read and praised Morgenstern's "The Time Moment in Value Theory". While away, von Neumann did further work on games, moving on to consider 4-person games. On his return, in the Fall of 1940, von Neumann began to write a paper in two parts synthesising his work on game theory to date (1940, 1941). This constitutes the theoretical framework of what became the Theory of Games. From the beginning of the first part, "General Foundations", the emphasis is clearly on games with more than two players: 2-person theory is given short shrift, being quickly used to motivate the notion of the [characteristic] set function v(S). The concepts of strategic equivalence and (in)essentiality are defined, and the 3-person game is illustrated in detail, showing how its solution is a system of possible "apportionments" [imputations]. The stability evident in this case is considered in the general n-person game and a complete definition is given: no two valuations in the solution dominate each other and every external valuation is dominated by at least one solution member. This, of course, became known as the "Von Neumann-Morgenstern Stable Set" after 1944. In addition, the concept of (in)essentiality is discussed and a graphic illustration of the 3-person game provided. All the aforementioned is seen as a prelude to "our ultimate goal, i.e., to find the solutions of all games with n≥4" (p. 25). The second part extends the coverage to include non zero-sum situations, showing how the analysis remains essentially the same, and, adopting a more formal set-theoretic notation, proves some simple theorems on stability and discusses decomposition of games (the properties of games when considered together versus separately). Throughout, the presentation is dense and rigorous, and without discussion of economic or any other applications.

While von Neumann was writing this extended paper, however, he was listening to criticisms of conventional economic theory by Morgenstern, who was then preparing his scathing (1941b) critique of Hicks's (1939) Value and Capital. Provoked by this, in May 1941, he asked Morgenstern to write a paper illustrating his basic thoughts on economic theory. Within a month, this yielded "Quantitative Implications of Maxims of Behavior" (Morgenstern 1941).

If von Neumann's work above is impressive for its relentless mathematical rigor, the paper by Morgenstern is equally impressive for different reasons. Devoid of mathematics, it offers a skeletal social theory that is both methodologically individualistic and recognises the importance of social interaction. His concern, he states at the outset, is to construct a "theory of society", but we will limit ourselves to economics since "there alone the beginnings of a theory of social behavior are discernible" (p. 1). To this extent, a "maxim" may be regarded as a complex plan. Morgenstern distinguishes between two types of maxim. Unrestricted are those whose success or failure is independent of their adoption by other individuals, while restricted are those who are so dependent. A successful economic theory, i.e., one that enables prediction, will have to recognize that individuals make decisions whose outcome depends on whether or not similar decisions have been made by others. This is the core of Morgenstern's message.

In the process, Morgenstern conveys to the reader an idea of how he views knowledge, the individual, society, and economic theory. There exists an objective mechanism which underlies the evolution of economic phenomena. Individuals, having less than perfect foresight, have incomplete knowledge of "the facts", and thus act with subjective, rather than objective, rationality. In addition, because all decisions take place through time, and because time changes maxims from unrestricted into restricted ones, its introduction into any theory is "essential" (p. 2). This is emphasised repeatedly by Morgenstern, viz. "restricted maxims will be applied in succession and have therefore a dynamic character" (p. 21a). The quantitative effects of other individuals' behavior on one's application of restricted maxims may be positive or negative. For example, one's decision to withdraw deposits in order to protect them from bank failure will depend on the number of others making a similar decision. In cases such as this, the rift between subjective and objective rationality may be very large indeed. This raises the importance of the "institutional setting" in which economic decisions are made: regulation may help overcome information deficiencies thereby reducing these interaction effects. Interestingly, Morgenstern claims that the case for government intervention is made stronger when it serves to enhance inadequate private information, making easier the pursuit of restricted maxims. This, he says, is something not recognized by the laissez-faire doctrine. In general, however, "social progress [might]. . . . be presented as a gradual shift from restricted to unrestricted maxims" (p. 6a).

The document is manifestly Positivistic: the entire theory is predicated on the gap between the true mechanism of the economic world and our knowledge of it, both of which are completely quantifiable. The only suggestive mathematics in existence is "the theory of games by J. von Neumann", referring to the paper discussed above, but, perhaps in an attempt to provoke the latter to further efforts, he adds that even this does "not take the problems into consideration which have been described above" (p. 22).

In July 1941, Morgenstern began to write an introduction to von Neumann's work on games. By September, the two decided that it should all become a small volume. In an October letter to Frank Ayledotte, director of the IAS, von Neumann wrote:

> "We hesitated for some time whether to publish it as a paper (in one of the economics periodicals) or as a book. We are now inclined to do the latter since this would free us of limitations in space which would be rather troublesome"[26]

It ended up, three years later, as the 635-page tome with which we are now familiar: "a big book, because they wrote it twice, once in symbols for mathematicians and once in prose for economists".[27] In the introductory chapter, "Formulation of the Economic Problem", written by Morgenstern, he offers both a critique of accepted neoclassical theory and a reworked version of the alternative conceptual approach broached in his "maxims" paper. While the wings of Morgenstern's imagination have been clipped this time by the need to have his theoretical hopes conform to the "actually existing mathematics", the grandiloquence remains. The importance of interdependence is argued: when we move from the Robinson Crusoe world to a social group, qualitatively new features enter the picture, which require a mathematics better adapted than the traditional differential calculus. Throughout, physics is presented as a benchmark in scientific progress which economics should strive to attain. Gone is the emphasis on time and dynamics: "our theory is thoroughly static" but, as the experience in physics has shown, "it is futile to try to build [a dynamic theory] as long as the static side is not thoroughly understood" (p. 44). The rest of the text is a lengthy elaboration of the theory, centering largely on games of 3

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[26] Dated October 6, 1941: In Von Neumann papers, Library of Congress, Container 32, File 90, Theory of Games and Economic Behavior. Ayledotte responded: "I was keenly interested in your suggestion that the mathematical theory of games might demand the development of new branches of mathematics running parallel to the impulse which physics gave to mathematics in the seventeenth century. If you can get the mathematicians to take as much interest in that side of your paper as the economists are sure to take in the economic aspects, you would indeed be starting something of first-rate importance." (Oct. 9, 1941, Loc. Cit.). As we shall see below, for the next 12 years or so, game theory was a distinctly mathematical, rather than economic, affair.

[27] David Blackwell, quoted in Albers & Alexanderson (1985), p. 27.

or more persons, with Morgenstern's economic examples appearing in Chapter 11.

How do von Neumann & Morgenstern situate their book with regard to the earlier work considered since the beginning of this paper? While Steinhaus, as we know, remained *incognito*, and therefore could not have been taken into account, even in principle, it is remarkable that none of Borel's work before 1938 is mentioned. The latter volume, furthermore, is referred to in just two brief contexts. The first concerns Poker, where Borel's related work is characterized in a footnote as "very instructive, but . . . without a systematic use of any underlying general theory of games" (p. 186, n. 2). The second mentions the first elementary proof of the minimax theorem, by Ville in the same volume. The proof used by von Neumann, based on the theory of convex sets, is offered as a further, simplifying step in this process of elementarization: in general, the text presents itself as a revolutionary one, whose relationship to earlier mathematical and economic ideas is one of contrast rather than continuity.

The Period to 1944: Overview:

The period leading up to the publication of the Theory of Games displays historical features against which the subsequent development of game theory may be juxtaposed and without which it cannot be understood. If one term can possibly capture the twenty-five or so years considered above, it is: "fragmentation". From 1921 until the collaboration of von Neumann and Morgenstern, the three interested parties worked in relative isolation. In Poland, Hugo Steinhaus seem to have remained disconnected from and unaware of the contemporaneous writings of both Borel and von Neumann.[28] The relationship between the latter two bears the hallmarks of a certain

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[28] Although its broader significance for our story remains a matter of speculation, both Borel and Steinhaus did have one thing in common: a strong interest in naval matters. We have already noted Borel's involvement as Minister of the Navy. Commenting on his own 1925 paper, Steinhaus (1960) writes: "I was especially interested in naval pursuit. After having found the concepts of minimax and maximin I was well aware that the minimax time of the pursuer is longer or equal to the maximin time of the pursued, but I did not know whether they are equal in all similar games. Consequently, I called "closed" the games for which there is equality and "open" the other ones. My pupils in Poland have adopted later this terminology (thus the pursuit of one ship by two is closed, and it is open when there are three pursuers)." (p. 108)

"standoffishness", if not outright academic antagonism. At no stage, despite their mutuality of interest, did either show a willingness to cooperate or exchange ideas. Nor is this particularly surprising. There is a natural spirit of competition which mathematicians feel with regard to their work. Priority debates, for example, reflect this sense of pride and the desire to be given credit for initiative and originality. In von Neumann's case, this natural tendency may have been magnified by two other features. First, as noted above, he was considerably younger than Borel and, while his genius was now being recognized, he no doubt relished the prospect of outsmarting somebody of Borel's stature. Second, it reflected the longstanding rivalry in mathematical matters between Gottingen and Paris which, in turn, was in keeping with the volatile nature of Franco-German diplomatic relations.[29] In the work of both Borel and von Neumann, from 1928 onwards, references to the contribution of the other were scant or nonexistent. Despite the fact that each clearly drew on the other to some extent, whether in the choice of examples or the basic framework, relations between them remained distant and disconnected. At no stage could anybody have spoken of a game theory "community".

Even in their collaboration on the book, during the early 1940's, von Neumann and Morgenstern worked in isolation. For example, neither seems to have been aware of the work in France: in December, 1941, Morgenstern *accidentally* discovered Borel (1938), the volume containing the elementary minimax proof by Ville. Neither was von Neumann aware of this work (see Morgenstern 1976, p. 811; Rellstab 1991a, p. 17). This of course is not too surprising, given the political situation. Even at Princeton, however, they were quite isolated. Through 1941-42, von Neumann gave a few related lectures which were not well attended, and the economics department at Princeton, to Morgenstern's dismay, remained aloof. He speculates that this kind of work may have somehow

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[29] Reid's 1970 anecdotes endorse this: for example, the 1909 visit by Poincaré to Gottingen "was an unwelcome reminder that the mathematical world was not a sphere, with its center at Gottingen, but an ellipsoid" (p. 120). Further, in 1917, when Hilbert wrote a memorial for the deceased Gaston Darboux, his house was besieged by a mob of angry students demanding that "the memorial to the "enemy mathematician" be immediately repudiated by its author and all copies destroyed" (p. 145).

been inappropriate to the conditions of war then obtaining.[30] That game theory was shaped at Princeton in virtual isolation from the mathematics and economics communities there is also borne out in other accounts. Ted Harris, later to be head of mathematics at RAND, graduated with a Ph.D. in mathematics in 1947 and recalled that his first encounter with game theory was his chance discovery of the Theory of Games in Princeton's bookshop, just before leaving the university. Harris had been unaware of this development during his career at Princeton, and had seen von Neumann speak only on ring operators and other topics.[31] Albert Tucker, student of Solomon Lefschetz and then professor of mathematics at Princeton, similarly did not become interested in game theory until 1948 when George Dantzig invited his participation in a trial project on linear programming being supported by the Office of Naval Research (Albers 1985, p. 343). Likewise, Sam Karlin, a fellow graduate of Harris, and soon to be a key game theorist at RAND, consulting from "Caltech", had had no exposure to game theory at Princeton. The Theory of Games, he argues, appeared to have had little effect on a Princeton mathematics department oriented primarily towards topology and analysis: indeed, Karlin suggests that game theory came into existence there only after he *left* in 1947.[32]

Thus, the Theory of Games and Economic Behavior, when it emerged, was without a "natural" audience. Though directed in its rhetoric towards economic theorists, its central ideas were equally novel in the mathematical sense. While the authors hoped that the impact would be greatest in economic theory, they also surmised correctly that some time would pass before the "game" idea became common currency. The second part of this paper illustrates the emergence of a game theory community. Here, for the first time, there appeared an extended group, among whose members the exploration of the mathematics of strategic

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30 Ironically, it was to be the consideration of war which would motivate the theory's early development (see below).

31 Ted Harris Interview, Feb. 29, 1990, Los Angeles.

32 Sam Karlin Interview, Mar. 2, 1990, Palo Alto.

interaction became a *modus vivendi.* What is of particular interest to economists today is the curious role played in the whole affair by the Theory of Games. The book certainly signalled to the broader audience in mathematics and economics that a theoretical innovation had taken place. However, a community of "game theorists" emerged *not* through their exploration and elaboration of the "great book", but rather as a result of the role of the older minimax idea in World War II. Postwar mathematicians came to grips with game theory by extending the earlier work of von Neumann and Ville on two-person games, *not* the characteristic function touted as the theoretical centerpiece of the Theory of Games.

Part II: Postwar Consolidation

The significance of wars in determining the direction of research and, therefore, the form of "discovered knowledge" is enormous. In this regard, the effect of World War II was unprecedented. The application of scientific methods to conflict not only destroyed lives and cities more "efficiently" that ever before, but had fundamental repercussions on the way science would be pursued when the conflict ended. And this applied to mathematics and economics as much as to the natural sciences. Shortly after the end of the war, interest in game theory had spread to other mathematicians besides von Neumann. It started to gain respectability as an area of applied mathematics, support for further research in the area was forthcoming, and a self-reproducing community appeared. The entire process represented the stabilization of the mathematics of games: this occurred through the demonstration of the links between games and what was already known, in other areas in mathematics for example, or what had recently been learnt, in the area of military strategy for example. It is somewhat ironic that while the grander ambition of the Theory of Games, particularly as expressed in Morgenstern's introductory chapter, was temporarily shelved in the process, von Neumann himself was a fundamental force in the entire movement.

We first look at the role of academics in wartime research. We then concentrate on two loci of particular interest: the Operations Evaluation Group, attached to the Navy, and the Statistical Research Group, attached primarily to the Air Force and part of the Applied Mathematical Panel. It was in the former that game theory found its first military application, while the latter would form the core of a group of postwar mathematicians centered at Project RAND, devoted to research on games. We show how the work at RAND reflected several influences: the continuation of research on military strategy as demanded at the end of the war; the exploration of games from the perspective of von Neumann's new interest, computation; the exploration of the theoretical links between games and linear programming, and statistical decision theory. Out of this milieu came the first textbook, the first layman's version of game theory, and the personnel who would carry the theory back to campus mathematics departments, as the latter were revived by the ONR.

Academics and War:

The German invasion of Poland in 1939 highlighted the fact that there were "grave shortcomings in the organisation of science for war" in the U.S. (Baxter 1946, p. 11). For 'science' one might as well read 'scholarship' in general, for the ensuing half decade saw the mobilisation of scholars of all shades, physicists and humanists alike. For example, the National Defense Research Committee (NDRC) coordinated the contracting of federal research, in the development of equipment and war techniques, by university physicists, chemists, engineers and mathematicians.[33] The Manhattan Project, developing the atomic bomb at Los Alamos, drew on physicists including Oppenheimer, Fermi, Teller, Wigner and von Neumann. Nor was it all "hard science": without a centralised agency to collect and evaluate foreign intelligence, "the inadequacy of the American intelligence apparatus had become conspicuous and critical" (Katz 1989, p. 2). The response was the Office of Strategic Services (OSS), which drew on academic economists, historians and sociologists, and ultimately became the CIA.

The wartime mathematical activity of interest to us fell under the control of the NDRC. The latter was established in June 1940, by an order of President Roosevelt. Its function was to "correlate governmental and civil research in the

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[33] Baxter (1946) is the official history of the NDRC. Its coverage of the work of the Applied Mathematics Panel is, however, negligible.

fields of military importance outside of aeronautics" and it was chaired by Vannevar Bush (Baxter 1946, p. 15).[34] The NDRC had five internal divisions catering, respectively, to perceived needs in armor & ordnance; bombs, fuels & chemicals; communication & transportation; detection, controls & instruments; patents & inventions. These proceeded by administering contracts with universities and other institutions for defense research. Because of the intense activity and the attendant sense of urgency, these university departments experienced considerable upheaval in the process. For example, the Radiation Laboratory at M.I.T., at its peak employed over 4,000 people from all over the country. Its key contribution was the development of shorter wave radar which yielded better resolution and clarity on the radar screen. Other large laboratories, developing underwater sound, were operated in California, at Columbia, Harvard, and the Woods Hole Oceanographic Institute.

Under the umbrella of the NDRC, mathematicians were engaged in the application of mathematics and probability towards improving the effectiveness of weapons, either through better use of existing ones or the design of new types. They had shown an early keenness to become involved: the American Mathematical Society and the Mathematics Association of America had, in 1940, jointly appointed a War Preparedness Committee with subcommittees on research, preparation for war, and education for service.[35] The two NDRC groups of interest from the point of view of game theory are the Statistical Research Group (SRG) at Columbia, whose work was used primarily by the Army Air Force, and the Anti-Submarine Warfare Operations Research Group (ASWORG) located in Boston and attached to the Navy. The reader should bear in mind, however, that the bureaucratic divisions which seem so neat on paper were, in practice, much less clear: everybody knew everybody else and, to the extent permitted by secrecy, there was much exchange and interaction.

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[34] Aeronautics was in the domain of the National Advisory Committee for Aeronautics (NACA) established in 1938.

[35] Among the consultants to the research group were von Neumann (Ballistics), Norbert Wiener (Computation) and Samuel S. Wilks (Probability & Statistics) (see Morse 1941, p. 296).

The Statistical Research Group:

In late 1942, the NDRC was completely reorganised and the activities of mathematicians were grouped under the control of the Applied Mathematics Panel, headed by Warren Weaver, with technical assistant Mina Rees.[36] Policy decisions were guided by a Committee including R. Courant, G.C. Evans, T. C. Fry, L. M. Graves, O. Veblen, Sam Wilks and Weaver. Courant, having left Gottingen in 1933, had established the Courant Institute at NYU. Veblen and Morse were both colleagues at the Institute for Advanced Study with von Neumann, who also served as advisor to the Committee (Rees 1980, p. 609). The AMP set up contracts with eleven universities including Brown, Berkeley, Columbia, Harvard, NYU, Northwestern and Princeton. Two broad categories of work can be identified among the projects undertaken: first, Fluid Mechanics, Classical Dynamics, Mechanics of Deformable Media, and Air Warfare; second, Probability and Statistics.

In the first category, the Applied Mathematics Groups operated at NYU, Brown and Columbia. At NYU, a group under Courant worked on gas dynamics and, in particular, on the theory of explosions both in the air and underwater. This resulted in the Shock Wave Manual (1944) and its successor, Supersonic Flow and Shock Waves (1948). At Brown, the focus was classical dynamics and the mechanics of deformable media, under W. Praeger. The Columbia group was concerned with aerial warfare, in particular air-to-air gunnery, a departure from the classical applied mathematics of the first two above (Rees 1980, p. 612). Here, the concerns included aeroballistics (the motion of a projectile from an airborne gun), the design of different types of weapon sights, and pursuit curve theory.

The principal work in the second category, Probability and Statistics, was done by Statistical Research Groups operating at Columbia, Princeton and Berkeley.

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[36] Rees (Ph. D Math., Chicago 1931) taught at Hunter College until 1943. After the dissolution of the AMP in 1945, she became head of the Mathematics Branch of the Office for Naval Research, which subsequently supported much mathematical activity, including game theory, in universities all over the U.S. (see "Mina Rees" in Albers 1985).

The former, the largest and most important of these, was run by Allen Wallis with Harold Hotelling as principal investigator.[37] Samuel Wilks, at Princeton, headed the second group, while Jerzy Neyman out in Berkeley ran the latter. Wallis's group occupied a portion of a building at W. 118 St. New York, which also had as tenants Columbia's Applied Mathematics Group, mentioned above, and the Strategic Bombing Section of Wilks's Princeton Group, run by John D. Williams. Both the latter and Wilks, it appears, interacted very closely with Wallis. During this time, Wallis and Hotelling gathered around them a exceptionally capable group of mathematicians and statisticians including Abraham Wald, J. Wolfowitz, Milton Friedman, James Savage, Abraham Girschick, Frederick Mosteller and George Stigler (see Wallis 1980). Wald had left Vienna in 1938, with an invitiation to the Cowles Commission, arranged by Oskar Morgenstern, and then moved to teach at Columbia (Freeman 1968). Wolfowitz, Friedman and Girschick had all been Hotelling's students during the 30's.[38] Savage, after a Ph.D from Michigan in 1941, had gone first to the IAS as von Neumann's assistant, and then to Cornell and Brown, before working for Weaver (Wallis 1980).

As with their Applied Mathematics neighbors, the study of aerial combat was central to the work of the Columbia SRG. It is impossible to adequately describe the 572 reports, memoranda and substantive letters which resulted, but a sample will give some idea. An early study on alternative ways of placing machine guns on a fighter aircraft involved studying the geomerty and tactics of aerial combat. This, in turn, led to work on anti-aircraft weapons, aircraft turret sights and dispersion of aircraft machine guns. A second broad area was the design of the optimal lead angles of aircraft torpedo salvos. This involved the interpretation of photographs of Japanese destroyers to glean information on

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[37] At the time that Weaver was still head of the Fire Control division of the NDRC, in 1941, Wallis had left Stanford for the Office of Price Administration (OPA) in Washington (Wallis 1980). At Hotelling's suggestion, Weaver approached Wallis in mid-1942, suggesting that he lead the Columbia SRG, which he did from then until its dissolution in 1945.

[38] Girschick, in fact, only spent 1944-45 at the SRG: the remainder of 1939-46 being spent as principal statistician for the Bureau of Agricultural Economics. Affected by his work with Wald, however, after a brief stay at the Bureau of the Census, he went to RAND, soon after its creation in 1946 (see International Encyclopaedia of Statistics, pp. 398-399).

speed and turning radius. The third field concerned the development of inspection and testing procedures. Out of discussions on the sampling of equipment came the idea that perhaps testing could be stopped before the prescribed sample had all been used, if the information gained thus far somehow suggested that adequate testing had been done. The ratification and formalisation of this idea became sequential analysis, developed by Wald.[39] Although Wald, himself, had already used the minimax idea in his 1939 paper on statistical decision theory, game theory, it appears, was not used as an analytical tool by the group.[40]

One area where the mathematics of games *did* find some use was at ASWORG. Headed by Philip Morse[41], physicist at M.I.T., they performed, as their name suggests, operations research directed towards anti-submarine warfare (see Morse 1948, 1951).[42] Morse's group was constituted primarily in response to the presence of German U-boats along the north Atlantic convoy shipping routes. Analysing data sent in from widespread naval bases, the group made recommendations about the use of equipment in the pursuit of submarines. For example, taking account of the detection range of equipment and the rate at which a plane or ship could patrol a given area, they calculated a "sighting likelihood curve", which related the probability of sighting to range, visibility and other factors. Simple probability theory was used to recommend the best way, given the limited resources, to carry out radar and sonar searches of areas of sea.

ASWORG used game theoretic analysis in two applications. The first of these was the barrier patrol: where a narrow seaway is patrolled regularly by aircraft in

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39 The Quartermaster Corps of the Navy apparently made significant economies in inspection using these procedures (see Wallis 1980)

40 Savage, too, was familiar with games, to some extent: "[At lunch one day], Wald discussed some of his ideas on decision theory and Savage . . . remarked that he knew a rather obscure paper that would interest Wald, namely, VonNeumann's [sic] 1928 paper on games. Wald laughed and said that some of his ideas were based on that paper." (Wallis op. cit. p. 334)

41 Morse had already done some research work for the Radiation Laboratory and for the Army Air Force (see Tidman op. cit., p. 34).

42 Morse's discussion of these ideas is in two sources. Morse (1948) is his Josiah Willard Gibbs lecture to the American Mathematical Society. Morse & Kimball (1951) is the declassified version of a report by the same authors written in 1946 (see Tidman 1984, pp. 102-3).

order to prevent the passage of submarines. For example, the Straits of Gibraltar were monitored to keep U-boats out of the Mediterranean, and the area between Brazil and Ascension Island was patrolled in order to catch German ships returning with tin and rubber from Japan and Malaya. The passing submarine, incapable of remaining submerged during its trip through the straits, must choose a point at which to surface. The patrolling airplane must choose a point at which to cross the sound in surveillance. The latter wishes to make a contact, while the former wishes to avoid it. In order not to have its move predicted, a mixed strategy is preferred: otherwise, the plane would simply choose the point at which it knew the U-boat was going to surface, or the submarine would simply avoid surfacing at the point at which it knew the plane was going to cross. The game is a continuous one: each player must choose a point on the strait, of fixed, finite length, and does so by applying a probability distribution to the spectrum of possible points. Morse shows how to find the minimax solution to the problem (see 1948, pp. 613-619; 1951, pp. 105-109). When applied, this solution ensures that the lowest probability of contact that the plane can ensure itself is the highest risk of contact that the submarine will have to face. The second application is in the allocation of forces into strategical and tactical components by two opposing armies: Blue and Red.[43] Without going into details, we again note that the minimax solution provides a "safe" option for both sides, given many simplifying assumptions about the relative effectiveness of opponents' forces against each other's production and each other's armies. Any side's choice of tactical force, relative to strategic force, will depend positively on both the opponent's total forces and the opponent's relative weakness in production (see 1951, pp. 73 - 77).

Morse finishes with a rallying call:

> "The difficulties of solving such problems are not ones of tedious detail, but often due to lack of fundamental techniques. Much more basic research must be carried out before many problems of practical importance can be solved"
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> (1948, p. 619)

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43 Manichean mathematics! The reader should have little difficulty identifying who's who in this excapade!

"The studies of von Neumann and Morgenstern show that there are solutions to each problem and show the general nature of these solutions. They do not show, however, the technique for obtaining a solution. . . . A great deal more work needs to be done in finding solutions to various examples before we can say that we know the subject thoroughly . . . . . It is to be hoped that further mathematical research can be undertaken on this interesting and fruitful subject

(1951, p. 109)

The alert reader will observe that the above is largely an unwitting "resurrection" of the pre-war work of both Borel and Ville. The Blue and Red force allocation problem is essentially the sort of application suggested, but not analysed, by Borel in 1938 (see p. 18 above). The barrier patrol is simply a game on the unit square, the solution of which is first offered by Ville, as discussed above. Von Neumann of course, was familiar with all of this, having been shown it by Morgenstern in 1941 while they wrote their book (see p. 30 above). He was also close to the applied mathematics being done during the war.[44] He was also completely capable, however, of independently seeing applications and finding solutions to problems presented by Morse and others.

As Morse probably knew, his 1948 talk simply endorsed an agenda for postwar research on games that was already in the making.[45] It was in this context of national defense that the theory first gained broader recognition.

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[44] Von Neumann began consulting to governmental organisations after he became a naturalised U.S. citizen in 1937. His first contact was with the Ballistics Research Laboratory (Army Ordnance Dept.) at Aberdeen, Maryland, to which he was probably introduced by Oswald Veblen. From Sept. 1941 to Sept. 1942 he was attached to Division 8 of the NDRC, working on detonation waves, or how to arrange explosive charges so as to direct and maximize the blast. From Sept. 1942 to July 1943, he advised the Mine Warfare Section of the Navy Bureau of Ordnance on the operations research of mine warfare. This is the work that took him to Washington D.C. during the last few months of 1942, and to England for the first half of 1943, while working on the Theory of Games. In late 1943 and early 1944, he was back at Aberdeen, working with Theodore von Karman on aerodynamics, and continued to advise the Navy on mines. Beginning in Sept. 1943, he was consultant at Los Alamos on the "Bomb". (See Asprey 1990, pp. 25-27) This suggests that his work with Morgenstern was a "side interest". From 1943 onwards, games would be of interest to von Neumann primarily to the extent that they related to computation.

[45] Ed Paxson recalls: "I bought my copy of TGEB [i.e. Theory of Games] . . . in 1946. I was working at the Naval Ordnance Test Station, China Lake, California. Immediately fascinated, I formulated what was later to be called a differential game. This was a real problem, a duel between a destroyer firing at a maneuvering submarine, with allowance for denial of sonar coverage in the destroyer's wake.

Project RAND:
As the war ended in 1945, discussions in the War Department, Office for Scientific Research & Development, and branches of the military, centered on the prospective return to the campus of the many academics that had been involved in military-oriented research. The links forged by war were about to be dissolved by the peace, and there was a concern that much accumulated experience, along with the possibility for future cooperation, would simply be lost. In March 1946, followed a discussion of this type between "Hap" Arnold (Army Air Force Chief of Staff), Dr. Ed. Bowles (scientific consultant to the War Dept.) and some engineers from the Douglas Aircraft Co., the former committed $10m of research funds remaining from the war and thus Project RAND was born.[46] Initially located under the Douglas roof and then moving to its own quarters in Santa Monica, the function of this group of physicists, mathematicians and engineers was to conduct a program of research on "intercontinental warfare, other than surface, with the object of advising the Army Air Forces on devices and techniques" (p. 46). In fact, the group had more flexibility than this suggests, and Arnold, initially to the disgruntlement of his own military minions, ensured that they were not tied to time or forced to work on immediately applicable ideas. A postwar venue was thus available in which research was easily pursued, was very well paid, and was moreover free of the usual academic duties such as teaching and administration. It was the institutional stabilization of the military-academia symbiosis that had begun during the war.

Reflecting their ostensible purpose, the mathematics group at RAND was labelled the section for the Evaluation of Military Worth.[47] At the head was John Williams, who had headed the New York branch of Wilks's Princeton Statistical Research Group. Among the others to arrive early were Edwin Paxson (see note 45), Morris "Abe" Girshick, who had also been at the Columbia SRG, Olaf

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In November of 1946, I spent a day in Princeton with Johnny von Neumann working on this problem. He sketched an approach, and even did some machine coding for his yet unborn machine." (quoted in Shubik 1980, p. 414)

46 The engineers had advised Bowles in a successful study of the B-29 bomber. Arnold had been supportive of such work and wished to see it continued. (see Smith 1966, passim)

47 Harris interview

Helmer, who had worked for the AMP in New York, Melvin Dresher, who came from the Office of Price Administration, where Wallis had been before the war, and J.C.C. McKinsey, another mathematician trained at Columbia in the 30's. Girschick, Helmer and Dresher were also immigrés: from Russia (1932), Germany (1936) and Poland (1932) respectively (see Kaplan 1983). Exerting a strong influence from a distance were von Neumann, Weaver, and Wilks, all of whom became RAND consultants.[48] Wilks, back at Princeton, directed his best Ph.D students towards Williams, among them Ted Harris. Samuel Karlin arrived at the "Caltech" mathematics department, from Princeton, in 1947 and became consultant to RAND the following summer, at the suggestion of his chairman, Henry Bohnenblust, himself a RAND consultant and veteran of wartime operations research in England. Present too were Lloyd Shapley, then with only a B.A. from Harvard and, part-time, David Blackwell, of Howard University.[49]

RAND was in this period a center of intellectual ferment. In addition to a constant flow of consultants, lengthy summer sessions were held, to which was invited anybody of note or ability who might have something to contribute.[50] For the decade till the mid-50's, Santa Monica was the point of reference for those working on matters related to game theory at Princeton, Michigan, and in other military-sponsored research institutions. This decade saw the "stabilization" of

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[48] At $200 per month for several hours work, they were well compensated. Von Neumann in particular was much sought after by many groups: it wasn't flattery when Williams told him: "Paxson, Helmer and all the rest are going to be especially nice to me if I succeed in getting you on the team". Letter Dec. 16, 1947, von Neumann Papers, Library of Congress, Container 15, File RAND Corp. Contract Corresp.

[49] Karlin would later persuade the exceptionally talented Shapley to return to Princeton for a Ph.D in mathematics, which he did towards the end of the 1940's (Karlin interview). Blackwell had met Girschick in Washington D.C. in 1945, where the latter had lectured on Wald's sequential analysis. There began a collaboration which resulted in, *inter alia,* Theory of Games and Statistical Decisions (Wiley 1954).

[50] Beginning in 1948, months-long gatherings were held for the discussion of games and related matters. Those participating were either RAND staff, regular consultants or visitors from outside. 1951's guest list, for example, included M. Abramovitz, K. Arrow, E. Domar, J. Duesenberry, J. Marschak, O. Morgenstern, P. Morse, P. Samuelson, and R. Solow. These convocations were generally of the "Secret", if not "Top Secret", variety, and Air Force clearance was required for attendance (for example, see letter K.E. Wells to O. Morgenstern, June 15, 1950, OMPD, Box 14, File RAND).

the mathematics of games through the elaboration of its links with other areas in mathematics, with linear programming, and with statistics: the theory became one strut in the framework of ideas developed under immediate postwar military patronage. The concentration on two-person games reflected its relation to military conflict as revealed during the war.[51] As we discuss below, it also resonated with ideas in activity analysis (linear programming) and statistics, and with von Neumann's new interest in computation, all of which were eagerly sponsored by the military. We conclude Part II by briefly illustrating this stabilization process. This is done to convey to the reader a sense of the broad agenda in which game theory became central: the finer detail must await further treatment.

The stabilization of games:

Beginning in 1946 with Loomis's completely algebraic proof of the minimax theorem, there began a stream of further proofs, largely falling into two categories. The first of these rest on fixed point theorems, or iterative procedures, and the second on the theory of convex sets (see Kuhn 1952, pp. 71 ff.; Luce & Raiffa 1957, Appendix 2). Included in the former are Weyl (1950) and Gale, Kuhn & Tucker (1950a), while the latter includes Nash's (1950) proof of the existence of an equilibrium point for all n-person games, of which the minimax is a particular case.[52] Other work utilized the geometric properties of the minimax solution to derive inductive proofs (see Luce & Raiffa 1957, Appendices 3 and 4). In Dresher et al (1948), the first collective publication by the RAND mathematics group, the issue of the actual calculation of solutions by matrix methods is raised, and the properties of infinite games under various assumptions about the convexity and continuity of the payoff function are presented. The issue of calculating solutions was particularly important for von Neumann, given his new interest in computation, discussed below. The

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[51] Mirowski (1991) argues that all of game theory in the immediate postwar period reflected the military influence. Making this case peruasively necessitates showing how all related pursuits such as linear programming, statistical decision theory, experimental games, and computation were part of the military design. One should also show how the work on cooperative games, in Nash (1950) and Thrall et al (1952), was more the exception than the rule.

[52] For a good discussion of various proofs, the reader is directed to Kuhn (1952), his lecture notes on games.

exploration of continuous games with various types of payoff function was related to hypothetical duels, of the bomber-fighter type, and so held the promise of future military value. They also constituted, however, mathematically intriguing problems and offered endless theorem-solving possibilities to the period's most capable mathematicians.[53]

Directly related to the problem of proving the existence of an equilibrium is that of finding, or calculating, the solution. In Kuhn & Tucker (1950) this had become "the principal outstanding problem of zero-sum two-person games" (p. viii). Although it was not easy to get the busy von Neumann to travel to Santa Monica[54], he was particularly interested in the work linking games and computation. Writing to Warren Weaver in 1948 he said:

> "I was very glad to see your comments on RAND's work on the theory of two-person games. I have seen several of their reports, and I need not tell you that I am also very much interested in the fact that some of their attention is now going to this subject. . . . I have spent a good deal of time lately on trying to find numerical methods for determining "optimum strategies" for two-person games. I would like to get such methods which are usable on an electronic machine of the variety which we are are planning, and I think that the procedures that I contemplate will work for games up to a few hundred strategies."
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> (Mar. 1, 1948, VNPLC Box 32, File Correspondence W. Weaver)[55]

Indeed, it could well be argued that he remained actively interested in games only to the extent that they related to his work on computation: his only further publication on the topic was Brown & von Neumann (1950), a constructive proof

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[53] The question of what "really motivated" those working on game theory during this period recurs periodically. Personally, I believe motives were mixed. Dresher (1961), the declassified version of an earlier RAND volume, is virtually exclusively devoted to games capable, in principle, of military application. Williams (1954) is also particularly concerned with the value of games in strategic considerations. Others, however, such as McKinsey (1952) and Shapley, in general, appear much less "gung-ho" and concerned above all with the mathematics.

[54] In October 1951, von Neumann's consulting fee was doubled so that he might pay more attention to RAND than he had hitherto done. (See letter Alex Mood (RAND) to von Neumann, Oct. 1, 1951, VNPLC Box 15, Folder RAND)

[55] For various requests for advice from McKinsey and Paxson to von Neumann, see various letters, VNPLC, Box 25, File RAND Corp.

intended for "utilization when actually computing the solutions of specific games"(p. 73). [56]

The theoretical links between games and other areas such as statistics and linear programming were soon established. Following Wald (1945), which showed the application of the minimax theorem in statistical decision theory, the decision process being characterized as a game against Nature, these ideas were further developed in Arrow, Blackwell & Girschick (1949) and Blackwell & Girschick (1954). In Koopmans (ed. 1951), both Dantzig and Gale, Kuhn & Tucker demonstrate the equivalence of the tasks of solving a game and a linear programming problem, thereby drawing on activity analysis to provide *constructive* existence proofs in game theory. 1952 at RAND also saw the first work in experimentation on games (see Flood 1958). With regard to the question of military application, both Haywood (1954) and Caywood & Thomas (1955) offer simple, stylised examples. These illustrate bettter the *confidence* the military held in the mathematics as a tool rather than the direct *usefulness* of the ideas in this context. Finally, out of this milieu in the early 1950's, came the first textbook on game theory, McKinsey (1952) and the first popularisations, McDonald (1950) and Williams (1954). While the latter books became immediately popular, McKinsey's book was perhaps a little too austere for the economics readership of the 1950's and was supplanted several years later by the more "user-friendly" Luce & Raiffa (1957).[57]

<u>Conclusion:</u>

This paper supports a relatively simple claim about the evolution of what we now know as game theory. For the 35 years preceeding the end of World War II, the thinking on games was limited, disparate and disconnected. In the absence of

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[56] For an exploration of von Neumann's work in computation, see Aspray 1990. In 1943, while on a research trip to Britain for the Navy, von Neumann wrote to Veblen of his newly developed "obscene interest in computational techniques" (quoted in ibid, p. 27). That game theory could be sustained despite his diminished interest was one sign of its maturation as a field of applied mathematics.

[57] I owe this observation about the early textbooks to William Riker.

anything that could be regarded as discourse, the "theory", if it could be called such, was of limited meaning to a limited number of people. What ultimately gave it life was not Morgenstern's hope for its transformation of economics, but the changed postwar environment for mathematical research, the strong military interest in the theory, and the attention given to it by the mathematicians under their patronage. The confluence of these various streams created the context in which the theory was first stabilized. There remains to be written an historical account of the passage of game theory out of military research into various universities with the encouragement of the ONR, its assimilation into political and biological science, and, above all, the halting process by which it gradually transformed the canon in microeconomics. Tackling the latter in a full manner would involve examining the development of courses in game theory at such institutions as Princeton and Stanford, the work of early figures who straddled both the mathematics and economics groups, such as Martin Shubik, the transformation of theoretical journal articles as they began to incorporate strategic considerations, and the process by which this "trickled down" to the textbooks. An initial feasible approach would be to confine one's attention within economics to a particular field such as industrial organisation, where the influence of game theory has, arguably, been most profound.

Chapter 2: War as a "Simple Economic Problem" The Rise of an Economics of Defense

Introduction

This paper has a dual purpose. The first part is concerned with offering a partial answer to the large question: why has the economics of defense not become as developed an academic subdiscipline as, say, health or education economics? After all, a curious disparity exists between the amount of public resources devoted to defense expenditure and the degree to which academic economists have devoted intellectual energy to understanding the phenomenon. The second concern is to offer a reconstruction of the institutional development of an applied economics of defense, beginning in World War II and culminating in the Pentagon reforms, of the 1960's, under Secretary Robert McNamara. In Part I we examine the early application of economics to conflict in the wartime research groups operating in the U.S. and England. Then, the RAND Corporation, which was formed in 1946, is presented as a continuation of this line of work. Part II shows how economic thinking played an important role in RAND's subsequent evolution and eventually stimulated the budgetary reform known as the McNamara Revolution. Part III reflects on this reconstruction in an attempt to understand the paucity of academic concern.

Part I A Foot in the Door: World War II and the Establishment of RAND

Scientists Against Time, J.P. Baxter's (1946) account of the involvement of American scientists in World War II through the Office for Scientific Research and Development (OSRD), is long on the impact of physicists and chemists, and the importance of such places as the Radiation Laboratory at M.I.T., but quite neglectful of the wartime role of social science in general, and economics in particular. Indeed, reading his reconstruction, one might believe that the war was essentially won on the basis of physical ideas, the innovations of physicists and chemists, with the concepts of economic efficiency, and marginal cost and benefit having no place in the heat of conflict.

Such a conclusion would be wrong. Economists, like virtually every other academic group, found themselves plunged into the national war mobilization and though their output was less tangible than the physical hardware produced by scientists, they achieved a measure of success, reputation and authority which was ultimately to propel them further in the strategic realm than anybody in 1940 would have dreamt possible. Their application of the economic paradigm to the relatively "small", localized problems of strategic bombing or aircraft armory created a voice for economists in the consideration of conflict, a voice which grew increasingly loud in the postwar period, by the 1960's booming resoundingly across the stage of national strategic design. The winning of access to this stage is the concern of this section. An understanding of this can be gained only by examining the place of economic ideas in World War II.[1]

The Birth of Operational Research:

That the framework which allowed economics to influence strategy in the U.S. for at least two decades after World War II had its genesis in Britain, and not America, will no doubt strike some as curious. However, the main impetus for the evolution of what was eventually to become systems analysis grew from the role in military advice given to scientists in England, beginning in roughly the 1930's (see Stockfisch 1987, Harrod 1959, Macdougall 1951 and War Ministry 1963). In 1934, an ad hoc committee, chaired by H.T. Tizard, was set up by the British Air Ministry to investigate "how far recent advances in scientific and technical knowledge can be used to strengthen the present methods against hostile aircraft" (Stockfisch 1987, p. 5). From this grew the development of radar by Robert Watson-Watt and there followed a series of experiments designed to discover how best the device should be employed. The effort to make the radar operational involved the close cooperation of the scientists and the military: the former became privy to RAF knowledge and

[1] Certainly, economists were crucial to financing the war in the Treasury Department, Office of Price Administration, and War Production Board, and it was there that the majority of economists were employed, but others, particularly in the Office of Strategic Services (OSS) and Statistical Research Groups (SRG) of Columbia and Princeton, were more concerned with fighting it (see Katz 1989). Here, economics and economists proved vital to strategic planning and gained an influence which was to ensure a place for them in related research when the war was over.

operations. This kind of interaction with civilian scientists quickly became common in many units in the RAF, the Admiralty and the Army. By 1940, scientists were addressing problems of aircraft acquisition, antiaircraft gun location and radar sighting. By 1942, the Admiralty had its own Department of Operational Research, headed by Dr. P.M. S. Blackett, and throughout 1942 and 1943, such expertise was extended to aiding Army forces in the Middle East, Italy and India on land-warfare.

While the nexus described above seems to be a technological one, it in fact permitted the influence of the neoclassical economic framework in framing and answering certain, related questions. The best British example was that of the Statistics Branch (S-Branch) which, from late 1939, supported Oxford physicist F. A. Lindemann (later Lord Cherwell) in his role as scientific advisor to Churchill. Although ostensibly appointed for his scientific acumen, Lindemann in effect spent almost two thirds of his time addressing what can best be characterised as economic questions (see Macdougall 1951, Harrod 1959). For this purpose, he surrounded himself with a group of university economists including Roy Harrod, G.L.S. Shackle and Donald Macdougall. They collected and analysed extensive statistics on the wartime economy, predicting shortages and designing rationing schemes, and offered economic arguments to guide the transfer of national resources from civilian to military use, essentially acting as watchdog against the excessive "requirements" of the latter. Both Churchill and Lindemann, it seems, were particularly concerned with sustaining civilian morale and with having solid statistical arguments to back them up in mediating between the claims of various departments:

> "There was a classic occasion on which the Prof. [Lindemann] got the requirement for anti-aircraft shells defeated . . . . by calculating how many thousands of shells would, according to this requirement, be needed to bring down each separate enemy bomber and showing that the resources required for their production would be many times as great as the damage that the enemy bombers could, on the most pessimistic assumption, inflict"
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> Harrod 1959, p. 200

The general success of operational research in the branches of the British forces stimulated a strong interest on the part of the U.S. forces in Britain. By late

1942, General "Hap" Arnold, commander of the Army Air Forces, recommended that each air command have an O.R. capability. Stockfisch (1987) suggests that "oneupmanship" played a role here: it was intolerable to the Americans that they should have to face their British counterparts without having their own scientists in tow. Regardless of the reason, the strengthened position of science in military operations paved the war for the involvement of American economists in Britain, and a core of them was soon installed in London. To understand how they got there, however, we must retrace our steps somewhat and examine what was happening in economic circles in the U.S.

Economics and the OSS:

In mid-1941, arguing that the gaps in American intelligence-gathering were leaving the U.S. dangerously ignorant on the eve of war, the swashbuckling "Wild Bill" Donovan had himself appointed Coordinator of Information by President Roosevelt (see Katz 1989). Donovan lost no time in constructing his own little empire and within a year the COI had become the Office of Strategic Services, forerunner of today's CIA, with a staff of over two thousand. In addition to Secret Intelligence and Special Operations sections, the OSS had a Research & Analysis Branch (R&A) to "transform raw intelligence data into concise, factual and rigorously objective analyses for the use of government agencies" (Katz 1989, p.14).

A Board of Analysts administered R&A, recruited through the old-boy network by director J.P. Baxter (who was soon replaced by Wm. Langer in 1942). This "college of cardinals" included, amongst others, economists Ed Mason (Harvard) and Calvin Hoover (Duke).[2] These, in turn, used their university contacts to recruit economists, historians, sociologists and anthropologists, who were distributed among Economic, Psychological (Political), and Geographical groups under the R&A umbrella. The organisers of this intelligence-gathering machine overlooked ideological differences they may have had with able young scholars, and room was readily found for Marxists Paul Sweezy and Paul Baran, and

[2] The complete Board comprised Mason, Hoover, D. McKay (French history, Harvard), Ed. Earle (military history, Institute for Advanced Study), J. Hayden (political science, Michigan).

virtually all the critical theory scholars of the exiled Frankfurt School, then at Columbia, including Max Horkheimer, Herbert Marcuse and Franz Neumann. Among the fifty or so economists, headed by Mason, were Emile Despres, Charles Kindleberger, and Chandler Morse (all from the Federal Reserve), Moses Abramowitz, Sidney Alexander, Carl Kaysen and Abram Bergson. In addition to the central office in Washington D.C., the R&A had outposts in London, Stockholm, Moscow, Honolulu, Algiers, Cairo, Istanbul, New Delhi and Chungking. These all gathered, analysed and interpreted data "in the field", passing it back to D.C. or onto the Allies in Europe. As we shall see, the London outpost was later to become singularly important for the activity of economists.

With the invasion of Russia by Hitler in late June 1941, the Economics Division in Washington launched an analysis of the German economic and military position. Why had the Germans been forced to a halt 500 miles inside the Soviet Union and, given the freezing weather, what were the requirements for a continued offensive and how did the rail system constrain the passage of physical supplies? The questions were posed and answered in a manner amenable to the tools of simple economic analysis. The emphasis was placed on potentially quantifiable entities, those which could be described by expected values if not certain quantities. Furthermore, the staff showed a painstaking application to the gathering of detailed data, a feature which characterised most of the wartime work in this vein and, indeed, a decade later, the work of their counterparts at RAND. Chandler Morse sent out economists to collect information on issues related to the German position: technical information from railway officials on the efficiency of locomotives at subzero temperatures; daily forage requirements of horses used by the German infantry; volume and weight of dehydrated troop rations; ammunition expended at different levels of combat intensity; meteorological records to forecast weather conditions. Nor were correlations on a priori grounds always used in statistical forecasts: estimating German aircraft losses, for example, was done using a spurious correlation between this and the mean of Russian admissions of Soviet losses and German claims of Russian losses! (Katz 1989, p.109). This belabored attention to detail was to prove essential to the economics fraternity in their gaining authority in military circles and was also a source of empiricist pride in relation to their historicist and humanist colleagues. Interdisciplinary tension was a feature of life at R&A. As Kindleberger recalls,

> "There was a methodological struggle between historians and economists. When it came to estimating Russian wheat production, for example, Russian historians claimed that the economists could hardly make a contribution if they did not know how to read Russian, which would give them access to crop reports and the like. The economists, on the other hand, claimed that with data on acreage, historical yields and weather they were in a better position statistically to estimate output and the wisps of evidence from the daily press were diversionary rather than helpful"
>
> 1980, p. 238

This intolerance and at times belligerence towards the "blindness" of other disciplines was to become a recurring theme for the next twenty years in the involvement of economists in military affairs. As we show below, economists remained loath to cooperate significantly with others such as historians or political scientists and only those who adopted the tools of economic analysis commanded their professional respect.

Of the work done abroad, especially valuable was the serial number analysis undertaken by a group headed by Sidney Alexander. Traipsing around the Tunisian desert, Alexander gathered serial numbers from captured and damaged enemy equipment and used them to reconstruct the sources and patterns of German tank production. Such information was particularly useful to the R&A London outpost which in early 1943 began to assume particular importance. As we mentioned above, the U.S. Army Air Corps had begun to make operational research a feature of its planning by late 1942. Now, several months later, there was rivalry on air strategy against Germany between the RAF Bomber Command and the U.S. Army Air Corps. The former, after the Battle of Britain, favored area-bombardment of Germany, in the belief that a large number of civilian deaths would sap German morale. The latter, on the other hand, favored precision bombing of selected economic targets in order to reduce German ability to sustain the war. Accordingly, in Fall 1942, Col. R. Hughes, senior target planning officer for the U.S. Air Force persuaded John Winant, U.S. ambassador to England, to bring in economists to guide on target selection. A group of the

Washington R&A economists were transferred to the Enemy Objectives Unit (EOU) in the Economic Warfare Division at the U.S. embassy in London. The earliest arrivals, in late 1942, were Morse, Walt Rostow and Wm. Salant. Soon, they were joined by Kindleberger, Kaysen, R. Rosa and H. Barnett. Also present was Charles Hitch who had been there in 1941-42 as staff economist at the Mission for Economic Affairs, had spent the previous year at the War Production Board in Washington, and was now returning with the OSS. Hitch had been at Oxford since 1934 and was particularly well placed to ease the path of a new group of Americans operating in England.

The EOU had initial difficulties in gaining the ear of American Air Force people, who had till then relied on intelligence provided by the British Air Ministry. A difference of opinion, however, between the EOU and its British counterpart, in which the former were proved correct, soon resolved this problem. Using aerial photographic interpretation, prisoner-of-war interrogations and Polish intelligence, the EOU concluded that the Folke Wolf plant had been moved from Bremen to Marienberg, Poland. The Air Ministry disagreed but were soon proved wrong. "From that time on, the Air Force was willing to listen to its most unmilitary economists" (Kindleberger 1980, p. 238). The growing importance of target selection, and hence the EOU, became clear when Alexander's serial number analysis revealed that German tank engine manufacturing was confined to two companies and that gear boxes were being made in only two plants (Katz 1989, p. 111).

In response to Hughes's request for aid in target selection, the EOU in late 1942 and early 1943 completed a series of Aiming Point Reports containing microscopic detail on location, function and layout of various industrial facilities. These impressed the Air Force and there followed a further series of Target Potentiality Reports which showed how to maximize damage to the German war machine for a given effort in air attack. Kindleberger recalls how this was little other than the intuitive application of input-output analysis and capital theory (see Kindleberger 1980). The former showed how removing one row of inputs, such as oil, would bring the economy to a halt; the latter suggested that account also be taken of the possibility that labor could be substituted for damaged capital. They reasoned in terms of "depth", the lag between production and its use on the German fighting front, and "cushion", those inventories and alternative supplies which could be substituted to sustain supplies to the front. This sort of analysis had a concreteness and an immediacy which gained the attention of the Joint Chiefs of Staff and enhanced the authority of economists relative to their humanist colleagues.

The extent to which the bombing strategy advocated by the EOU was effective in the defeat of Germany is a controversial issue on which the books have still not been closed. Their main opponent in this debate on appropriate strategy was Briton Lord Solly Zuckerman, scientific advisor to the joint Allied Expeditionary Air Forces. With regard to securing the beachheads for the Normandy landings in mid 1944, operation OVERLORD, Zuckerman favored bombing the French railway "marshalling yards" on the grounds that this would render the railroad system disfunctional and useless for transporting German soldiers and supplies to the Atlantic front. Eight months previously, Zuckerman had examined the pre-invasion bomb damage in Italy between Sicily and Naples, where railroad marshalling-yard damage had been extensive. As a consequence, Zuckerman subsequently had great faith in the importance of railyards as bomb targets. The EOU, on the other hand, favored a strategy of railway and road bridge interdiction to restrict German supply lines, on the a priori grounds that marshalling yards could be repaired within hours and that, furthermore, rail traffic had a civilian "cushion" of up to 85%. As might well be expected from a conflict of enormous scale, the historical evidence yielded by the leadup to the Normandy landings remains sufficiently unclear to vindicate either group: both line interdiction and yard bombing occurred and differences as to their relative importance remain unresolved.[3] Similarly, in the strategic bombing of Germany itself, Zuckerman stuck to his marshalling yards targets while the EOU favored fighter aircraft and ballbearing plants, and synthetic oil facilities. To the latter's chagrin, Zuckerman's policy was officially adopted at first, but again the

[3] For the details of this debate on strategy, which continued long after the war ended, see Zuckerman (1978), Rostow (1981), Kindleberger (1978, 1980).

evidence yielded by war was less then clear: some aircraft manufacturing plant was in fact bombed. The postwar Strategic Bombing Survey shows this to have had a limited effect on German fighter production but, Kindleberger claims, it directed German pilots away from resisting the Allied landings on D-Day (1978, p. 40). Also, oil targets were officially adopted as the bombing proceeded, thus legitimating at least some of the advice of the EOU.

While the contribution of the R&A in Washington and London may never be assessed in a manner satisfactory to both sides of the debate, economists emerged with a confidence in the applicability of the economic paradigm to conflict. For some of them, war, like inflation, unemployment or economic growth, was now just another difficulty to which the tools of economic analysis could be fruitfully applied:

> "War is a relatively simple economic problem. The objective function has only one argument --- winning. . . . . and one constraint, to keep the domestic civilian economy moving."
>
> Kindleberger 1980, p. 239

The Statistical Research Group:

If the R&A branch of the OSS facilitated the direct influence of economic reasoning on war strategy, the Statistical Research Groups of Columbia and Princeton also contributed to the creation of an authoritative voice for economists, albeit in a less direct manner. Administered by the National Defense Research Committee (NDRC), the Applied Mathematics Panel (AMP) coordinated the bulk of mathematical and statistical research directed towards the development and deployment of weapons (see Wallis 1980; also Chapter 1). The AMP was run by Warren Weaver, formerly Director of Natural Sciences at the Rockefeller Foundation, assisted by Mina Rees of the Hunter College Mathematics department.

Much of the research in probability and statistics with weapons application was done by the Statistical Research Group at Columbia (SRG-C), run by Allen Wallis with Harold Hotelling as principal investigator, both of whom were economists as well as statisticians. As neighbors in their building on W 118 St., New York, they had the Strategic Bombing Section of the Princeton SRG, run by John D. Williams, applying statistical methods to similar issues as the EOU in London. The two groups interacted closely, focusing in particular on aerial conflict. Using the geometry and tactics of aerial combat, and the probability of hitting, they suggested the optimal placement of machine guns on fighter aircraft. Similar studies were made of antiaircraft weapons and aircraft turret sights. As with the EOU, photographic interpretation played a role; for example, photographs of Japanese ships yielded information on their manoeuverability which helped in the design of optimal lead angles of aircraft torpedo salvos.

The gathering assembled by Wallis and Hotelling was as mathematically and statistically capable a group as one could have desired. It included A. Wald, M. Friedman, G. Stigler, R. Bennett, M. Hastay, J. Wolfowitz, J. Savage, A. Girshick and F. Mosteller. While their work was primarily in mathematical statistics, it is by no means insignificant for what followed the war that "7 of the 18 principals were primarily or secondarily economists". First they influenced the subsequent work in game theory and decision theory "both of which are essentially economic theories" (Wallis 1980, p. 329). Second, they sufficiently impressed leaders Weaver, Wallis and Williams such that the latter actively sought the inclusion of economists in related postwar research. The institution ultimately destined to draw together the strands which linked economics and conflict would appear neither in London nor in Washington, but on a beach overlooking, somewhat ironically, the Pacific.

The Emergence of RAND:

As the war drew to a halt, it became clear that much of the civilian scientific advice which had proved decisive in securing military victory would disappear as scientists and engineers returned to academe. Nobody was more keen to retain a coterie of scientific advisors than General "Hap" Arnold, head of the Air Force. Science had served the military well during the war. The SCR-584 radar, M-9 director and proximity fuze had all been developed under the NDRC and had been crucial in turning the odds against the Germans after June 1944 (Baxter 1946, p. 36). Above all, the work of the Manhattan Project, with the bombing of Hiroshima and Nagasaki, had ended the war. Ambitious in his plans for the Air

Force, Arnold saw the harnessing of scientific progress as a path by which he might retain control of the atomic bomb and thereby secure superiority among the military branches.

Discussions in the latter half of 1945 between Arnold, some engineers from Douglas aircraft company, and various advisors who had been attached to the War Department, led to Arnold's committing $10m of wartime funds to research.[4] Project RAND (Research & Development) was attached to to the Douglas firm in Santa Monica and comprised a group of physicists, engineers and mathematicians engaged in a "program of study and research on the broad subject of Aerospace Power with the object of recommending to the United States Air Force preferred methods, techniques, and instrumentalities for the development and employment of Aerospace Power" (Goldstein 1961, p. 3).[5] The project was given a remarkable degree of freedom with the power to accept or reject Air Force suggestions, strong financial support without pressure for tangible results, and scope to pose questions and analyse problems as the staff saw fit. This was a reflection of the confidence on Arnold's part in the value of scientific research directed towards conflict. To smooth RAND's path in their dealings with a less flexible Air Force materiel and procurement bureaucracy, Arnold installed Curtis LeMay as a liaison officer mediating between the two.[6]

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[4] The engineers in question were A. Raymond, Chief engineer at Douglas and his aide F. Collbohm. Both had advised Arnold in a particularly successful project on the B-29 Bomber during the war Smith (1966) claims that the RAND idea essentially came from the Douglas people, who were keen because wartime projects were being liquidated and staff reductions were likely. The key meeting took place on Oct.1, 1945, at Hamilton Field outside San Francisco, attended by Arnold, Raymond, Collbohm, D. Douglas (sr.), E. Bowles and F.W. Conant.

[5] Early suggestions, during RAND's formative stage, of integrating the project with the work of the Army and Navy were quickly rejected (see Smith 1966).

[6] LeMay was quite forceful in removing institutional obstacles that might have shackled RAND. Air Force materiel and procurement officers at Wright Field wanted RAND to fulfill only very specific orders for equipment development but were quickly overruled by him. It is ironic that LeMay, twenty years later, would become one of the harshest critics of strategic thought associated with RAND and their alumni at the Pentagon.

The early characteristics of RAND --- in particular its 'self-conception' as revealed through its policy statement and the prevalence of physical science and mathematics types on its staff ---- show little evidence of the impact that social science, and economics in particular, was to have on the institution. By the time it officially started, in May 1946, Project RAND had four employees in a walled-off section at Douglas working on two projects with a distinctively "hardware" connotation. The first, presciently requested by LeMay, was "a study of the feasibility, design and military utility of an earth-circling satellite" (Goldstein 1961, p. 6) and the second a comparison of ramjets and rockets as strategic offensive weapons systems. As one veteran of those days put it, "In the beginning, the engineers were topdogs at RAND".[7] Within a year, however, as the institution started to expand, the authority that the economic paradigm had earned during the war began to resurface.

The process by which this occurred reflected exactly the network of alliances and influences that had grown around economics and economists in the wartime context discussed above: the OSS in Washington and London, and the SRG in New York. One of RAND's first employees, John D. Williams, had come from the New York SRG to Santa Monica to lead the work by mathematicians and budding game theorists in RAND's department for the "Evaluation of Military Worth". In late 1946, Williams successfully persuaded LeMay to allow him to begin recruiting social scientists and, in particular economists (see Smith 1966, ch. 2). This he undertook at the instigation of Allen Wallis, former head of the Columbia group discussed above, who was influential in postwar research and optimistic about what economists in this area might achieve. Williams's first recruit was Armen Alchian, formerly Wallis's student at Stanford, and now assistant professor of economics at nearby UCLA. Alchian began as a part-time consultant talking "with some difficulty" to the mathematicians and physicists then dominant at RAND. [8] The drive to net social scientists continued and, in September 1947, Williams

[7] Interview with J. Digby, Mar. 5, 1990, Santa Monica.

[8] Project RAND, by this time, had moved away from the Douglas Corporation and had relocated in a different part of Santa monica. Alchian recalls not really knowing what he should discuss with the RAND staff, as he had little in common with them and no experience of this type.

arranged a meeting in New York to which economists, sociologists and others of that ilk were invited. The group was addressed by Warren Weaver, former head of the Applied Mathematics Panel, who explained the tentative, and still fuzzy, character of the RAND enterprise. Among those present was Charles Hitch who, recall, had been an OSS economist in London working with Kindleberger, Morse and the rest of the Enemy Objectives Unit. Hitch was appointed head of RAND's new Economics Division.

The lines of influence were short but clear: from OSS through Hitch and from SRG through Wallis and Williams. They all knew each other and they had been significant during the war. They could have had no conception, however, of the impact the group now in the making would subsequently have, nor of how far the application of simple economic principles would carry them. For the moment, however, Hitch simply had to gather a staff and give it, or let it find, its own direction.

PART II Storming the Citadel: from Santa Monica to the Pentagon

RAND, at the time of Hitch's arrival in 1948, was still a fledgeling thinktank with a large budget and much flexibility, but little direction; essentially it was a hodgepodge of physicists, engineers, mathematicians and newly-arrived social scientists, working on a range of issues from nuclear propulsion to two-person zero-sum game theory, all reflecting as much the academic interests of the staff as the military concerns of the Air Force. It had neither a clear self conception nor an established public image, but still leaned towards research of a technical, engineering kind. Had anybody at the time suggested that RAND's identity was soon to be molded by articulators of the economic method, analysts of costs and benefits, rather than nuclear physicists or even aeronautics engineers, they might have been ridiculed. This, however, is exactly what happened. The affiliates Hitch asembled quickly made their presence felt among their colleagues --- often in neither a diplomatic nor timorous manner --- and, by the 1960's, RAND had become identified with systems analysis, a cost-benefit approach to conflict, refined and implemented by economists.

The Criterion Problem:

With the help of Alchian, Hitch quickly recruited a collection of economists, mainly graduates or young professors from the more prestigious universities.[9] These included Stephen Enke, J. Kershaw, A. Marshall, R. Nicholls, J. Hirshleifer and D. Novick.[10] Hitch's managerial style, which is universally deemed important for RAND's subsequent history, was to maintain a low profile giving his colleagues much leeway. With them they brought an emphasis on cost, central to the language of economic analysts but not necessarily to that of engineers, whose concerns were more technical. Coupled with the emphasis on cost was the stress on the need for adequate criteria in making choices about weaponry. This marked the beginning of the intellectual hegemony of economic thinking at RAND. With a belligerence similar to that in evidence in the R&A, Enke hammered home to audiences of engineers and physicists the need for adopting economic criteria when making engineering choices. The issues are best illustrated by an example (see Enke 1965, p. 417). By 1950, the U.S. Air Force faced a choice among several conceivable "next generation" strategic bombers. Some were turboprops: slow, lowflying and quite accurate in bombing but also relatively vulnerable. Others were turbojets: less accurate but fast, highflying and not as open to attack. What criterion should be applied in choosing which type of bomber should constitute the strategic force? Assuming the task is to destroy a particular target, is it the bomber of which the least number are required? The kind that will be subject to less loss of planes and hence crew? Or the one that will achieve a hit with the least number of costly nuclear bombs? The bomber chosen will differ depending on the criterion adopted, as will a host of ancillary choices such as penetration tactics, bombs required and planes sacrificed. Engineers suggested all sorts of criteria: for example, minimising the total weight of aircraft construction, assuming that weight and construction

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9 Alchian recalls that they had difficulty attracting Harvard graduates at the time, many of whom were interested in working in Washington on budgetary and macroeconomic problems. (Alchian interview, Mar. 5, 1990, Los Angeles)

10 Enke had recently left UCLA. Hitch had met Kershaw in Brazil where he had spent the previous year visiting the Univ. of Sao Paulo. Marshall and Nicholls were both Chicago graduates and Novick, somewhat older, a Roosevelt "Brains Truster". (Alchian, Digby interviews, Mar. 5, 1990, Los Angeles)

costs were positively correlated. Economists responded that all resource costs should be taken into account when making choices about weapons: maintenance costs for a bomber force were no less important than those incurred in acquiring it. Also, choices of particular strategic bombers implied choices about necessary support functions, and sacrifices in other areas such as tactical force. The systemic and intertwined nature of military choices was presented as a Gordian Knot which could be chopped at haphazardly, or systematically unravelled using economic rationale.[11]

While Enke was pounding the desk at RAND, the case for economic thinking in defense analysis was also being made by his colleagues outside Santa Monica. Nobody did this more eloquently than the political scientist Bernard Brodie, student of Jacob Viner at Chicago and then influential strategic thinker at RAND.[12] In a 1949 article in World Politics, Brodie illustrated and condemned the absence of any significant body of strategic thought: the "profession of arms.....has yet to round out a five foot bookshelf of significant works on strategy" (1949, p. 476). Classical principles of strategy, such as "Don't divide the fleet!", had become slogans, without any hints as to how or when they should be implemented in practise. Such principles, inherited from the 19th century and earlier, were without any theoretical foundation and, in a nuclear age when war techniques were changing more rapidly than ever, had become dangerously anachronistic. The best hope for elaborating any theory of strategy, Brodie argued, lay in exploring its parallels with "the science of economics", which had "enjoyed the most systematic and intensive development among the social sciences" (p. 475). Strategy was the development and utilization of the resources of the nation "for the end of maximizing the total effectiveness of the

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11 Addressing an audience of economists, Enke happily points out that principles of efficient allocation were demonstrated to senior generals using Edgeworth-type box diagrams. Isoquants showed the number of strategic and tactical targets destroyed respectively, with aircraft and nuclear weapons on the axes. (Enke 1965, p. 420)

12 From Yale's Institute of International Studies, Brodie went to the Air Force's Air Targets Division. There, his philosophy of restraint with respect to the use nuclear of weapons quickly estranged him from his military peers and he moved, in 1950, to RAND. See Herken (1985) pp. 30-35.

nation in war" (p. 476) and its broader variant, security policy, would incorporate political, social and economic considerations, as well as military ones. Such a complex of functions could "hardly be the province primarily of the soldier" (p. 477). The methods of economics, he argued, were necessary to give meaning to such widely used concepts as "balanced force": balance in one set of circumstances was imbalance in another and once this was realized, the concept of marginal utility became useful. Only in such terms could one discuss the optimal allocation of military forces in contingencies of various risk.[13]

Writing for an audience of operations researchers, Hitch (1953), too, made similar arguments. Operations research typically tackled local problems: for example, the adequate placing of escort gunboats in the Atlantic during the war, or the best way to arrange a unit of manufacturing assembly line. As such, of necessity, it focused on "low-level criteria", taking as given the broader context within which the problem was situated: in these examples, respectively, the achievement of victory in World War II, and maximising profit for the firm as a whole. Ideally, however, local problems should be solved with an eye to the bigger problems of which they were a part. Thus the choice of criterion to be satisfied was crucial and these should recognize the existence of opportunity costs, possibly borne outside the confines of the immediate problem but nonetheless significant. Hitch harked back to a use of economic reasoning in operations research during World War II: the problem of deciding the optimal size of North Atlantic shipping convoys. There, it was decided that the ratio of German U-boat losses to domestic merchant ships lost was the correct objective function. Given the relationships between convoy size and these two losses, it was decided that raising the average convoy size would be desirable. Hitch points out that this choice of ratio as criterion had no basis in economic theory and, even more importantly, the whole exercise ignored the reduced shipping efficiency of larger convoy size. For the sake of sustaining the flow of supplies to Normandy,

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13 Interestingly, Brodie also refers to von Neumann & Morgenstern's (1944) Theory of Games and Economic Behavior casting doubt, without stating reasons, on the applicability of their theory to military strategy (see p.479, n.13).

it may well have been worth sustaining the loss of a few extra ships. Hitch's punchline was qualitatively no different from that of Enke or Brodie:

> "The only discipline I know which has made any attempt to explore the characteristics of operations criteria, and the intimately related question of the relation between lower and higher level sub-optimization, is economic theory. Some of its conclusions and insights have wide applicability to operations research . . . . and indeed constitute the modest beginnings of a scientific analysis of the problem of selecting operations criteria"
>
> 1953, p. 93

The a priori case for economic analysis in military decisions was being made unambiguously and unequivocally. All advocates were essentially making the same point and were implicitly echoing Kindleberger's view that "war is a relatively simple economic problem". The correct allocation of military resources could be achieved only by recognising the "general equilibrium" nature of the problem. The protagonists were also implicitly challenging the authority of the military in what had traditionally been regarded as the latter's own arena. The full impact of this challenge, however, would not be felt for over a decade, during which time economists had to "prove" themselves in military and policy circles. Such a task ultimately fell to one of Hitch's newer acolytes, Albert Wohlstetter, in a study which transformed RAND and proved pivotal in the intellectual history of economics and defense.

The Strategic Bases Study:

In May 1951, the Air Force requested that RAND examine the future acquisition, construction and use of overseas air bases. They envisaged significant building of new bases and sought guidance principally on questions of efficiency: minimising the cost of a given number of facilities. For a while, nobody at RAND showed interest until Hitch persuaded Wohlstetter to take a look at the issue.[14] The latter had recently come to RAND, with a background in mathematical logic and a recent career ranging from the NBER through the War Production Board to his

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[14] This section draws on Smith (1964).

own construction business.[15] At RAND, the Bases Study was his first significant project.

Through most of 1951, Wohlstetter worked with Harry Rowen, an economist but also with an engineering background. An early document by them at the end of 1951 (D-1114, Economic and Strategic Considerations in Air Base Location: a Preliminary Review) drew attention to the fact that foreign airbases were particularly vulnerable to surprise attack. Note that in raising this objection, Wohlstetter and Rowen were implicitly questioning the *accepted* strategic context in which the Air Force had requested the study: in terms of Hitch (1953) above, they were moving away from low- to higher-level criteria. Given this observation about vulnerability, they began considering strategic alternatives to advanced overseas bases. The three other options considered in addition were

(i) bombers based on intermediate overseas bases operating in wartime
(ii) U.S.-based bombers operating intercontinentally, with air-refuelling
(iii) U.S.-based bombers operating intercontinentally, with ground refuelling at overseas bases

Taking into account vulnerability of equipment on the ground; distances from bases to targets; points of entry to enemy defenses and making various assumptions about Soviet atomic capabilities and deployment, Wohlstetter and Rowen, by now working with two others, concluded that the last alternative above was strategically the best option under various scenarios. The choice of U.S. bases with foreign ground-refuelling was considerably superior to the scheme of advanced overseas bases then deployed. In a full report produced in mid-1952 (RAND R-266), they suggested that not only was their proposal strategically securer, but it would also save the defense budget practically $1bn !

[15] Wohlstetter claims that he went to RAND because he was tired of writing about abstractions and wanted to learn about "the empirical world". His wife, Roberta, was already with RAND's Social Science Division, and several other RAND staff, including J.C.C. McKinsey, A. Girshick and O. Helmer were also acquaintances of his. (Interview with Wohlstetter, Feb. 28 1990, Los Angeles)

Wohlstetter and his supporters were convinced by their analysis, but they still faced the difficult task of persuading the Air Force: the latter were naturally reluctant to adopt drastic policy changes and insisted on going over everything with a fine tooth comb.[16] Finally, however, --- with Wohlstetter by this stage having confronted the Air Chief of Staff Thos. White --- the Air Force Council concurred with the need for a strategic shift and concluded that the RAND findings, for the most part, should be adopted. In particular, vulnerability was to be recognised in all Air Staff planning: critical overseas bases were to be hardened against attack; and new overseas bases were to be designed for refuelling. In short, RAND's systems analysis ---the cost-benefit analysis of strategic posture--- had caused a significant reorientation in Air Force thinking.[17] Civilian advisors had made an impression upon their military patrons in a manner qualitatively different from that done during the war. They had gone from responding to calls for advice on target selection to now claiming that the Air Force was, in fact, asking the wrong questions. As Wohlstetter wryly remarked, "'Requirements' are not deliverances from heaven. [They] come down ....from higher up, but not from On High." (1964a, p116). And further, the systems analysts had laid persuasive claim to having not just the "right" question, but also the best achievable answer.

RAND's Reconstitution:

The success of the Bases Study had implications for RAND. In Santa Monica, the emergence of systems analysis, as applied in the study, resolved the uncertainty and lack of direction which characterised RAND's earlier years. The Economics Division Became one of the most prominent units and economists came to form almost one fifth of the research staff (see Smith 1966, p. 63). While systems

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16 An ad hoc committee of the Air Staff was created in early June 1953 to double-check the RAND study.

17 RAND's Jack Stockfisch (correspondence August, 1990) suggests that some debate still exists over the actual extent to which the Bases Study actually influenced Air Force policy. The account here is based largely on Smith (1964). Herken (1985) pp. 93ff focuses on SAC commander Curtis Le May's opposition to the idea of sheltering SAC bombers and his general incredulity towards the threat of Soviet first strike. The actual changes as recounted by Smith, however, are not incompatible with Herken's account.

analysis called for interdisciplinary input, its intellectual foundation was cost-benefit analysis within the economic paradigm and its successful application earned particular prestige for economists in the RAND hierarchy.

And just as economics rose in the intellectual pecking order, so other disciplines fell. In the late 1940's and early 1950's, RAND had been the key institution in the development of game theory. Hopes for its application had been very high and much effort went towards the elaboration of game-theoretic conflict models, such as bomber-fighter duels and the construction of computerised wargames for battle simulation (see Brewer & Shubik 1979). With confidence among RAND among management and the Air Force that game theory would yield concrete strategic guidance (see Haywood 1954), mathematicians enjoyed the freedom to do abstract research.[18] The systems analysis typified by the Bases Study, however, was much closer to an elaborate "back-of-the-envelope" calculation than an exercise in complex modelling and computer simulation.[19] Wohlstetter, whilst never keen to portray systems analysis as something accessible to the unsophisticated, was quick to disparage the "new toolism" which favored complex techniques:

> "Mathematical models figure then as necessary but quite subordinate tools. . . . The tools that turn out to be useful for analysis here are likely to be more homely, but more productive" 1964a, p.105 [20]

Consequently, as systems analysis ushered in a new era during which it became RAND's emblem, the environment there for academic research deteriorated.[21]

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[18] Interviews with Alchian, Digby and Harris (Mar. 5, 1990, Los Angeles).

[19] Having said this, Wohlstetter spent great effort in accumulating data and a detailed knowledge of the workings of the Strategic Air Command, visiting bases to discover the degree of readiness of squadrons etc. in a manner similar to Morse and Alexander in the OSS during the war.

[20] Wohlstetter's repudiation of game theory as being central to systems analysis is inconsistent with the popular conception of its 'insidious' role in strategic thinking. See especially Wohlstetter 1964b.

[21] Interview with T. Harris, Mar. 5, 1990, Los Angeles

The success of applied cost-benefit analysis implicitly cast shadows of doubt on areas such as game theory which the military had supported but found relatively unsuccessful. Hence the increased pressure from the Air Force for "mission-oriented research" and work with clearer military application.[22] The 1950's saw the exodus of many game theorists to universities, where ONR support was now available, and the mathematics department itself was dissolved in the mid-1960's. In 1968, the physics department resigned *en masse* to form their own private consultancy, a move which reflected the extent they had felt themselves become marginalised as RAND evolved.[23]

The late 1950's and early 1960's also saw a considerable diversification in RAND's clientele: between 1959 and 1962, Project RAND, the original contract with the Air Force, declined from 95% to 68% of total support (Smith 1966, p. 131). Most significantly, RAND began to make contracts with agencies at the Office of the Secretary of Defense (OSD) level, an initiative which strained relationships with the Air Force: in effect they were now dealing with the Air Force's "boss". The most significant of these was with the Office of the Comptroller in the OSD in 1961, a position then held by none other than Charles Hitch himself.

McNamara's "Whiz Kids":

In the 1960's, the influence of economics on defense policy reached a peak. The ideas of control and efficiency, central to systems analysis, soon outgrew the RAND-Air Force nexus. In one sense, it was natural that the systems analysts would find their way onto the larger stage: designing optimal strategic air posture was but one task in the overall project of providing adequate total defense and, if nothing else, Hitch and Wohlstetter had continuously encouraged a general rather than partial equilibrium view of defense matters.

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22 Interview with N. Dalkey, Feb. 27, 1990, Los Angeles

23 Interview with C. Wolf, Feb.27, 1990, Los Angeles

Naturally, however, systems analysis required a catalyst for its fusion with the federal defense policy process. Such an agent was Robert McNamara, Defense Secretary *extraordinaire* of the Kennedy-Johnson administration. An Air Force lieutenant-colonel, Harvard MBA, Harvard professor teaching statistical control methods and, most recently, president of Ford Motor Co., McNamara was well-disposed, intellectually, to the adoption of rational costing and efficient planning. The overhaul of the complete defense budget desired by McNamara, however, could not be achieved with the main architects remaining in an Air Force 'thinktank'. Consequently, by 1961, he had recruited three of RAND's key people, Hitch, Rowen and Alain Enthoven to the Pentagon, giving them the broad task of redesigning defense budgeting practises with the watchwords of 'efficiency' and 'control'.[24]

Up to that point, criticisms of budgeting procedures were becoming particularly harsh, with the Rockefeller Report on U.S. defense problems calling for a budget that "corresponds more closely to a coherent strategic doctrine" (Enthoven & Smith 1971, p. 11). The problem lay essentially in the fact that budgeting and planning were two distinct activities in the Pentagon. The former, done by the Comptroller, projected only one year ahead and reflected needs in such functional categories as Military Personnel, Operations and Maintenance, Procurement etc. (see Hitch 1963, p. 5). Military planning, on the other hand, done by the the Joint Chiefs of Staff (JCS), projected several years ahead, but in categories such as Strategic Retaliatory, Continental Air Defense, Antisubmarine Warfare etc. Thus, budgeting was done without direct reference to what it was designed to achieve: adequate defense of the U.S. Furthermore, the individual Services --- Army, Navy and Air Force --- tended to develop weapons-acquisition priorities on a unilateral basis, with no consideration as to how they complemented other Service activities in providing defense:

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[24] Wohlstetter too was invited but declined the offer. In the Department of Defense, Hitch became Comptroller, Enthoven became Assistant Secretary of Defense (PPBS) and Rowen became Budget Director.

"As a result, the Secretary of Defense each year found himself in a position where he had to make major decisions on forces and programs without adequate information........Choices with important long-range resource implications were often forced to decision prematurely or without adequate consideration of all the major alternatives"

Hitch 1963, p. 5

A golden opportunity was thus available to put into practise all the prescriptions of systems analysis, at the highest possible level, i.e., the consideration of the entire national defense function and the allocation of the defense budget among its various components. The procedure developed to cope with this task was the Planning, Programming, Budgeting System (PPBS), refined at RAND by Hitch and Novick (see Novick 1965). Complex in detail, but simple in purpose, this linked budgeting and planning phases, allowing the Secretary of Defense to evaluate individual Service budget requests on the basis of their overall contribution to the nation's defense capability. It likewise allowed him to strive for a balance between the defense mission's key components such as Tactical Air Forces, Land Forces etc. The exercise was destined to ruffle many military feathers, as social historian Charles Morris describes:

"The process seems straightforward enough, but it hit the Pentagon with the force of thunderclap. All the pet projects that had been for years in the private preserves of the service chiefs were suddenly dragged out into the white glare of McNamara's relentless scrutiny, subjected to the unremittingly logical analysis of the systems intellectuals, stripped of their lazy rhetoric to expose their underlying irrationalities, the confusions of purpose, the overlappings and the duplications, the lack of any integrating strategic and tactical overview. Hardest to take was the lack of respect for the old-line military wisdom"

1984, p. 27

Among the projects axed on the recommendation of the Systems Analysis Office were the B-70 Bomber and the Skybolt air-launched ballistic missile program, both pet projects of, of all Services, the Air Force. The first, it was claimed, was inferior to a ballistic missile system and the second inferior to non-air launched systems such as Minuteman and Polaris.[25] Traditional interservice rivalry over

[25] For a detailed discussion of these cases, see Enthoven & Smith 1971, pp. 243-266.

weapon systems held no place in the search for efficiency and Hitch, Rowen and Enthoven were quite prepared to tread on military toes. As far as the Air Force were concerned, their RAND protegées had become "too big for their boots": the ideas spawned at RAND were, in a sense, backfiring on LeMay, RAND's original protector and now Air Force Chief of Staff. On retirement he warned,

> "Today's armchair strategists . . . can do incredible harm. "Experts" in a field where they have no experience, they propose strategies based upon hopes and fears rather than upon facts and seasoned judgements"
>
> 1968, p. *x*

His colleague Thos. White, who only a decade ago had guided Wohlstetter's Basing study recommendations through the Air Force, wrote:

> "I am profoundly apprehensive of the pipe-smoking tree-full-of-owls type of so-called professional "defense intellectuals" who have been brought into this nation's capital. I don't believe a lot of these often overconfident, sometimes arrogant young professors, mathematicians and other theorists have sufficient worldliness or motivation to stand up to the enemy we face."
>
> 1963, p.10

The adverse reaction they elicited was a measure of the controversial influence gained by Hitch's group. They had attained the highest reaches of decision-making in the defense policy area: their ideas were based upon simple economic notions of opportunity cost and the equimarginal principle, yet they informed completely what was to become known as the McNamara Revolution. The application of rational costing and budgeting was widely hailed and received a further boost when, in 1965, President Johnson ordered that PPBS be applied to the entire Federal budgeting process. Also, defense budget design naturally rested on strategic considerations, a discourse sustained by Enthoven, Rowen and, externally, Wohlstetter. From them came the ideas that variously dominated the strategic scene: preservation of "second-strike" (i.e. retaliatory) capability through missile silo hardening; "flexible response" to achieve destruction of military but not civilian targets; and the emphasis on building up conventional forces.

From fairly humble beginnings in the war, the application of simple economic principles carried a small group of economists from the consideration of low level problems to shaping of defense policy at the highest echelon. Steadily gaining influence in the decade after the war, the so-called defense economists had their heyday in the early 1960's. In this sense, RAND had far exceeded its original purpose. In 1967, however, systems analysis' golden years came to an end. McNamara left as Defense Secretary in November, becoming president of the World Bank. With his departure, his team of economists dispersed: Hitch to the University of California, Enthoven to the private sector in the medical products area and Rowen back to RAND as president. Only the latter remained in an area related to defense economics, but his tenure at RAND too was to be cut short by Daniel Ellsberg's leakage of the Pentagon Papers in 1970. By the end of the decade, the end of an era had been reached, the bubble had burst.

Conclusion:

The impact of basic economic thought in defense circles had grown without interruption for almost thirty years. Surely this must have reverberated throughout the economics discipline itself, producing a defense economics in the manner that other policy influences have helped develop the existing subdisciplines of education- and health-economics and public finance? As is now well known, this did not occur: the consideration of defense issues never attained the status of a significant field among the economics community and the numbers of specialized journals and active academics remain very small. However, during the heyday described above, attempts were certainly made by the main protagonists to attract some talent into the area. Sessions on defense at the American Economic Association for a few years featured Enthoven, Enke and others, all trying to attract academic interest.

> "Now that economists have come to occupy positions where they can make these contributions [to] weapon selection problems, it is to be hoped that more members of the profession will interest themselves in this important field of research"
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> Enke 1965, p. 426

Why, despite its burgeoning success in the policy arena did this strand of economics not mushroom into something much bigger? One reason may be the infamy that grew up around such figures as Wohlstetter, Herman Kahn and McNamara's advisors. Once they entered the controversial terrain of strategic analysis, they were dealing with issues which few could approach calmly. All were roundly criticised by advocates of disarmament and attracted a form of public attention that many academics, however publicly-minded, consider unpleasant (see Green 1966, Herzog 1963, Kaplan 1983). Further, some of the central figures, such as Wohlstetter and Kahn, seemed almost to revel in offending the sensibilities of those less receptive to the message of economic reason. Related to this is the fact that strategic thought, however anchored it may have been in the economic paradigm, was no longer recognisable as economics per se. It was an extremely sensitive political, moral and ethical issue, as much the concern of philosophers and ethicists as "value neutral" neoclassical economists. However, perhaps the most important reason lay in the nature of the economics used, and here Enthoven is worth quoting at length:

> "[The] tools of analysis that we use are the simplest, most fundamental concepts of economic theory . . . .The advanced mathematical techniques of econometrics and operation research have not proved to be particularly useful in dealing with the problems I have described. Although a good grasp of this kind of mathematics is very valuable as intellectual formation, we are not applying linear programming, formal game theory, queuing theory, multiple regression theory, nonlinear programming under uncertainty, or anything like it. The economic theory we are using is the theory most of us learned as sophomores. The reason Ph.D's are required is that many economists do not believe what they have learned until they have gone through graduate school and acquire a vested interest in marginal analysis."
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> 1963, p. 422

The above came at a time when, the economics discipline in general was, in fact, leaning towards the refinement of technique and not away. All the analytical methods above, which Enthoven is quick to disclaim, were, by then, grist for the academic economist's mill: econometrics was riding high on inter alia the apparent success of Keynesian fine-tuning, and game theory, which ironically RAND had been so instrumental in nurturing, was finally finding a place in economic theory. For an economics discipline moving in such a direction, perhaps war as an "economic problem" --- to use Kindleberger's term ---- had become just *too* simple.

Chapter 3: To Advance Human Welfare! Economics and the Ford Foundation 1950-1968

Introduction

Between 1953 and 1968, the Ford Foundation gave support to the economics discipline, to the extent of approximately $ 95m, in the form of research funds at universities and other institutions, support for projects and dissertations by academics and graduate students, and other programs in economics education.[1] The aid was substantial and broad-ranging, and had a considerable impact on the profession.

This raises several questions. First, why did the Foundation place so much emphasis on economics and look to the economics profession for answers to what it deemed to be important problems? What was its perception of economics and how did this evolve over time? Second, did the Foundation influence the direction of research in a significant way? Or was such influence dissipated by the extent to which the Foundation relied on the economics profession in formulating its economics program? Finally, can any conclusions be drawn from this historical episode about the relationship between the economics profession and foundations? Are there particular features which are likely to characterise any such relationship and what, if anything, do they suggest for the future of such support?

The approach taken here is as follows. Section I addresses the first two sets of questions above. A brief history of the Foundation is given, emphasising in particular the document outlining its "raison d'etre", the Gaither Report. Then, we examine the development of the Foundation's program in Economic Development and Administration (hereafter EDA) under which it provided most support to economics, paying particular attention to the interaction between the Foundation and the economics community during the early stages of the program's

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[1] Ford Foundation 1979, "The Facts Have Changed: A New Look at Economic Policy Analysis" in Problems and Opportunities in Governance and Economics (Internal Discussion Paper).

evolution. There follows a review of the main features of EDA activity till the program's demise in the late 60's. Section II, reflecting on the broader issues, addresses the third set of questions.

<u>Part I: Digging for Gold</u>

The Gaither Report:

Following the death of Henry Ford in 1948, the Ford foundation inherited 90% of the shares of the Ford Motor Co., under the terms of the wills of the patriarch and his son Edsel, who had died in 1943.[2] The previously small foundation, which had hitherto devoted its philanthropic attention to the Henry Ford Hospital in Detroit, found itself with a greatly expanded Endowment. At approximately $500m, this was the largest aggregation in human history of private wealth ever devoted to philanthropy, and the decision about how to spend the interest clearly merited close attention. On the advice of Ford family confidant, Karl Compton, then president of M.I.T. and member of the Foundation's Board of Trustees, a study committee was set up for the purpose of devising a strategy for the philanthropy. This was chaired by H. Rowan Gaither Jr., San Francisco lawyer and formerly assistant to Compton at the Radiation Laboratory at M.I.T.[3]

The report of the Study Committee, or Gaither Report as it came to be known, is a fascinating document, reflecting both the tumult of Cold War politics of the late 40's and the strong conviction among American "men of affairs" that the U.S. had the ability, if not indeed the obligation, to offer leadership in the economic and social realm to the rest of the "Free World".[4] As the Marshall Plan had demonstrated so well, properly directed financial aid could have quick results

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[2] See Neilsen 1972.

[3] The Complete Study Committee comprised H.R. Gaither Jr. (Ch.), T.H. Carroll, T.Duckett Jones, Charles C. Lauritsen, Donald G. Marquis, Peter H. Odegard, Francis T. Spaulding. The Staff of the Committee H. R. Gaither Jr. (Dir.), Wm. McPeak (Asst. Dir.), Dyke Brown (Asst. Dir.), Paul Bixler, Don K. Price.

[4] H. Rowan Gaither et alia, <u>Report of the Study Committee for the Ford Foundation on Policy and Program</u> (Detroit, Michigan: Ford Foundation, 1949).

and, with American opinion riding high, there was a feeling that the American model had much to offer the world.

As laid out in the Report itself, the aims of the Study Committee were fourfold:[5]

1. To define what is meant by "human welfare".
2. To consider "ways in which human welfare is most thwarted".
3. To propose programs to tackle the above.
4. To define the operating procedure through which the above programs could be pursued.

The purpose of the Foundation, as it was originally conceived in 1936, was to increase human welfare, but, as we have mentioned, its efforts in this, or any other, direction, had been limited. Now, with greater resources at its disposal, it had to be made clear what improving human welfare actually meant. The Study committee's interpretation unambiguously reflected the foreign policy concerns of the postwar elite. Human welfare, the Report's authors claimed, was synonymous with attainment of "democratic ideals": belief in human dignity: in personal freedom; in equality of rights, justice and opportunity; in freedom of speech, religion and association, and in self government as the best form of government. Democracy, they believed, was "on challenge in the world today" and "the relationship among human beings and social organisations [was] heavily marked by tension and disorder". But preparedness to respond by military action when "democracy is threatened by war" was not enough. "If such a defensive attitude is allowed. . . we may grow like the thing we fight". It was necessary to "press democracy forward by reaffirming its principles in action. Without the resulting internal vitality and stability, national security in the long run [was] unattainable". A particular worldview clearly informed the deliberations of the early Ford Foundation.

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[5] Gaither, ibid.

The committee identified the threat of war, problems of government, the economy and education in a democratic society as the most important problems of human welfare. The threat of war lay with East-West conflict and this had direct implications for the U.S. in both international and domestic contexts. First, America had an obligation to present the U.S. model of democracy as a desirable alternative to "totalitarianism". "The ability of free peoples of the world to resist totalitarianism. . . . . lies in their continuous achievement towards democratic objectives", and their faith in any order could survive "only when that order holds more hope for the future, . . . than does the totalitarian alternative". Domestically, the threat of war would confer "enlarged authority" on military leaders and legislative authorities and it would be necessary to ensure that the principles of democracy were not compromised under such circumstances.

As for governmental problems, the root causes were the shortage of talented individuals pursuing careers in civil service, a lack of popular participation in the governmental process and organisational defects in government itself. The economic problems received much fuller attention. In the economics realm, the accepted "goals of a private enterprise system" were to achieve "increased economic stability, both at home and abroad, with satisfactory growth in output and the highest possible level of constructive employment". Developments, then occurring, which impeded progress here included the joint phenomena of increased industrial concentration and growing governmental regulation and control. Given the basic premise in economics that activity of many small competitive units made for greatest efficiency, this merited examination. The need to "discover the determinants of industrial peace" was emphasised "in order to reduce the individual and social losses involved in labor-management strife". Furthermore, stability in the U.S. economy was necessary for world stability and peace: economic cycles here were seen to "jeopardize democratic institutions throughout the world". Other issues included the conservation of natural resources, achieving practical equality of economic opportunity among individuals and increasing the level of economic understanding among citizens. Out of the discussion above, five Program Areas were designated for the organisation of the newly-enlarged Foundation:

1. The establishment of peace.
2. The strengthening of democracy.
3. The strengthening of the economy.
4. Education in a democratic society.
5. Individual behavior and human relations.

Area 3, strengthening the economy, is the concern of this paper, for this was to develop into a large program supporting economic research, with the intention of using the results for policy purposes and disseminating research findings. Several questions are pertinent: what was the Foundation's conception of the economics discipline, why did it single it out for attention of this sort and what did it intend to achieve in so doing?

The Gaither committee made quite clear their view of economic theory and identified several encouraging trends in the economics discipline of the time. They praised the testing and validation of theory and welcomed the growth of statistical techniques and data banks which facilitated such testing. They urged, furthermore, that the results of such exercises be utilised in economic policy and made available to the broader population. The latter recommendation highlighted their intention of increasing economic information available to all parties. Implicit here is the view that once all groups possessed sanctified economic knowledge, significant hitches in the functioning of the economy would be removed. Verification of theories was advocated so that inapplicable or untenable schools of thought would not prevail "through the use of convenient but unrealistic abstractions". "Of first importance is the expression of willingness by leading economists to investigate basic theories, to subject these theories to the acid test of verification, and, where evidence is lacking, to get it at original sources".[6]

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[6] Gaither, ibid.

At the same time as encouraging an empirical approach, the Report identified and encouraged what it saw as the trend towards interdisciplinary cooperation between economics and other disciplines. There was a valuable role for sociology and psychology, a need for awareness of, and coordination with, the practices in other fields. Central to validation of theory was the testing of the psychological assumptions used by economists: "the study of economics [could] no longer be carried on by professional economists alone". It is worth bearing this sentiment in mind as we trace the evolution of the economics program later. Finally, the growing "practice" of economics was praised, i.e., the increased reliance on the advice of economists by business, labor and government. This, it was thought, would help "bring together the development of theory and the solution of specific problems". Of all the above, the greatest needs, it was concluded, were for the validation of theories, old and new, and the more effective dissemination and use of verified economic knowledge.

Why was economics singled out for particular attention? In a December 1950 memo to Paul Hoffman, then president of the Foundation, Gaither spoke of the immediately ensuing economic and military mobilisation to respond to the threat of communism, in either continued continued cold war or "full-scale hot war". Furthermore,

> "[the] fundamental elements of the ideological appeal and international propaganda attack of communism are economic. Communism proposes to grant a better standard of living for the mass of the people and asserts the exploitation of the working man by capitalists. . . . . [Hence] the need for pursuance of United States policies of aid to other countries for the development of economic and military strength in such a manner as to destroy the propaganda effectiveness of the communists."[7]

Economic doctrine and policy were thus central to the designs of the developing Ford Foundation. As shown above, the architects of its policy had a strong faith in

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[7] Gaither to Hoffman, Dec. 27, 1950, Ford Foundation file, E.D.A. Program Development 1953. The Gaither Report had been endorsed by the Trustees of the Ford Foundation in their annual report in Sept. 27, 1950.

the economics discipline's methods and saw as necessary not a radical change in approach, but a strong nudge in the direction in which it was already moving. The economics profession's public role in the U.S. had been growing for the past thirty years, and just four years previously, in 1946, the Council of Economic Advisers (CEA) had been established, according economists "institutional standing . . . at the pinnacle of national decision-making" (Barber 1981, p. 181). The Employment Act of 1946 institutionalized the role of the government in economic stabilization, following the Keynesian success of the war. By the late 40's, it was clear that demobilization had not resulted in a reversion to prewar stagnation, contrary to the expectations of pessimists. Furthermore, in 1949, the second chairman of the CEA, Leon Keyserling, in contrast with his predecessor Edwin G. Nourse, brought a more active approach towards policy matters for the Council's economists, claiming for his group a role not as mere "neutral, scientific" advisors, but as partisan public advocates of the Administration's programs, a partisanship which "enhanced the Council's influence with the government of the day" (ibid, p. 185). That the Ford Foundation should thus focus on the economics discipline is not at all strange, given the tenor of the times. As it was put by Marshall Robinson, later a key figure in the economics program,

> "[The] bulge of bright and concerned young people who entered the field in the depression years of the 1930's had been the analysts and advisors for the economic successes of World War II and its aftermath. They seemed able to understand what was going on, and apparently when policy makers listened to them, the economy worked better"
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> (Ford Foundation 1979, p. 6)

What was the economics discipline expected to achieve? In its discussion of Area 3, the Committee proposed that the Foundation help to "advance the economic well-being of people everywhere and to improve economic institutions" for (a) a growing economy (b) procedures and administration of economic organisations (d) improving labor-management relations (e) achieving a balance between freedom and control in economic life (f) improved worldwide standards of living and (g) increasing the economic understanding of U.S. citizens.

To give further focus to the future economics program, the Committee posed four questions:

A. "What are the fundamental principles and goals of our economic system?": there was neither understanding nor agreement on the goals of the economic system and, thus, policy-making was inconsistent with practice. There was a need to "define" such goals and promote their understanding. "Because of their impartiality", foundations were better-suited to such efforts than economic or political interest-groups.

B. "To what extent are our economic practices consistent with our economic principles and goals?": there was a need for more "validated economic theory" to show to what extent policy was conducive to a strong economy, now that the economic structure was "receiving its greatest challenge".

C. "Do we know the capacity of our nation to meet the domestic and international needs placed upon it?": the economy had to remain strong to cater to such needs.

D. "Is there adequate recognition of the relation of our economic system to the world economy?": exports to the U.S. were necessary for Western European economic development and demands for protection were damaging in this regard. Economic goals needed to be anlysed and defined in this context.

A Slow Start:

For two years, during 1951 and 1952, after the Gaither Report was endorsed by the Foundation's trustees, little concrete progress was made in EDA. Paul Hoffman was at the helm as Foundation President with the acerbic Robert M. Hutchins at his elbow as his chief Associate Director. Other Associate Directors included Gaither (in charge of the program in behavioral sciences) and Milton Katz (involved in EDA).

For some time during this period, it looked as though Richard Bissell would come aboard with a plan for EDA. Bissell, a former M.I.T. economics professor, now consultant to the Mutual Security Agency in Washington, D.C., had a particularly strong interest in East-West relations. In a paper submitted to Hoffman on "Possible Activities of the Ford Foundation Directed Towards Strengthening the U.S. and World Economy", he expressed his concern that the intentions of the Foundation, i.e., to repel communism, be translated into action i.e., influencing the decisions of government, labor and business. Pulling no punches, Bissell suggested four ways available in principle to the Foundation for achieving this: the spread of ideas on the printed page; direct education or propaganda; financial inducements to action; and lobbying. Ruling out the feasibility of the last three, on practical rather that moral grounds, there remained the power of ideas. And here, Bissell displayed a skepticism about the ultimate effect of such work, believing that this would merely contribute to the shelves of unread reports already in existence. His final proposal for the program was for a Board of "distinguished citizens" with a director, based in Washington D.C., backed by public education- and research-staffs. Pervading the paper, though, was Bissell's skepticism about the ability of the Foundation to achieve anything concrete in the economic program and he subsequently chose not to become involved with Ford, going on to a more "active" career in the C.I.A.

By the end of the 1952, the EDA had distributed approximately $3/4m in support of economic research, the primary grantees being M.I.T. for research on the relationship between economic development and political stability, and another project to examine the growth of governmental activity in Western Europe and the U.S.; the American University of Beirut for an economic research center; and the group Resources for the Future for a conference on conservation.[8]

In early 1953, several important developments occurred.[9] The Foundation started to seek wide advice on formulating its program: a letter from Milton Katz

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[8] The M.I.T. grant went to its Center for International Studies. Resources for the Future would, two years later in 1954, receive a capital grant of $3.8m. This group grew out of the Resource Program Development Committee consulted by the Foundation on resource matters.

[9] By late 1952, the Board of Trustees had grown dissatsified with Hoffman and Hutchins. Dean Donald K. David of the Harvard Business School was brought in to recommend changes. In February 1953, Hoffman resigned. He was replaced by Gaither as President and the Foundation moved

to Theodore Schultz, at the University of Chicago, requested the names of individuals who might act in a screening capacity for prospective EDA projects.[10] Schultz recommended several people including Edward Shaw (Stanford), Kenneth Boulding (Michigan), George Stigler (Columbia), Arthur Burns (National Bureau of Economic Research), Arnold Harberger (Johns Hopkins) and Jacob Viner (Princeton). In addition, Schultz sent two memoranda: the first on the need for small research grants to economists, the second on recent adverse developments in financial support and organisation of research in economics.[11] On the funding of small projects, Schultz suggested the establisment of a joint American Economic Association-Ford Foundation committee, charged with administering a fund of small grants, the projects being selected through informal investigation by committee members. In the second memo, Schultz discussed some problems, as he saw them, in the funding of research. First, there was inadequate funding available for small projects. Second, inflation had impoverished academic economists and more remunerative undertakings were luring them away from academic research. Also, this relative poverty enabled "many interests . . . to re-model the work of economists to fit their particular image of what economists should be doing". He identified several of these interests:

(1) those who wished to relate economics to other behavioral sciences or make economics into a different kind of behavioral science. Schultz commended the former and condemned the latter.

(2) those who wanted economic research to be practical. This inevitably meant studying ad hoc situations and was just as damaging as ""over general" theoretical work".

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headquarters from Pasadena, California, to New York city. See Nielsen 1972 and Ford Foundation, Working Paper for Advisory Group Conference, 16- 17 Oct. 1953.

10 Katz to Schultz, Jan. 15 1953, Ford Foundation, Area 3, General Correspondence

11 Schultz to Katz, Jan. 19 1953, Ford Foundation, Area 3, General Correspondence

(3) public pressure and governmental procedures which made continued funding for particular long-term projects contingent on obtaining "results". This was simply too restrictive.

(4) those who wanted economics to support a particular doctrine. While taking care to acquit the "major established foundations", in this regard, Schultz condemned those businesses and individuals who, endeavoring to provide a certain kind of "economic education", wanted economists to "give prestige to and to propagate the doctrine".

(5) those who wanted economists to relate themselves to institutions, which rendered them less free in their choice of research than at university departments.

One could be forgiven for suspecting that the sentiment expresses in the last two sections above might not be well received at the Ford Foundation, when one recalls the Gaither Report and the related discussion. Whether or not this was in fact true, only the first memo seems to have been treated seriously by the Foundation. Three months later, in April, a meeting of several economists and others was convened to discuss the formation of an advisory group for the development of the EDA program: Schultz's first memo was not discussed nor was Schultz himself included.[12]

The Calkins Committee:

As an outcome of the April meeting, an Advisory Group was formed to study the tasks facing the EDA program and report to the Foundation presenting a proposed program of activities for Area 3. The group, which comprised Robert Calkins

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[12] In a letter of invitation form Katz to John Condliffe, Professor of Economics at Berkeley, and consultant to the Foundation, Mar. 3 1953, the meeting, planned for April 9, 1953, would include R. Bissell; Prof. Bertrand Fox (Harvard Business School); Dean Ed Mason (Harvard); Mr. W. Riefler (Federal Reserve Board, Washington D. C.); Prof. Sumner Slichter (Harvard); Prof. John Williams (Cambridge, Mass.) and Mr. Theodore Yntema (Vice-President, Ford Motor Co., Michigan). From the Foundation, there would be Dean Donald K. David, a Trustee, Mr. Tom Carroll, Dean, School of Business Administration, U.N.C.-Chapel Hill, and consultant to the Foundation, and Joseph McDaniel, Secretary of the Foundation.

(Brookings Institution) Chairman; G. Lee Bach (Carnegie Tech.); Kenneth Boulding (Michigan); J. M. Clark (Columbia); R. Aaron Gordon (Berkeley); John Lintner (Harvard); H. Myers (Committee for Economic Development); Lloyd Reynolds (Yale); E. S. Shaw (Stanford); G. Siefkin (Emory); M. Upton (Washington Univ.); W. Hoadley (Armstrong Cork Co.).

The recorded deliberations of the Advisory Group, or Calkins Group as it came to be known, began on October 16, 1953. The group met to discuss a working paper written by Tom Carroll, member of the original Gaither Report Committee and cousin of Gaither. Carroll's Working Paper, as might be expected, stayed broadly in line with the Gaither Report as far as proposed activity in economics was concerned, stressing research, the utilisation of knowledge advanced through research, and the dissemination of knowledge. It was proposed that the Division (EDA) would not maintain a large permanent staff but would use consultants; would work through existing institutions rather than create new ones; would actively suggest projects in areas of interest to the Foundation; could offset criticism of "directing research" by being ready to review projects suggested from outside, not seeking credit for project results, "not attempting to exercise control over a project for which a grant has been made", and "avoidance of partiality in making grants".

The committee's final report, "Program for Area III on Economic Development and Administration", or the Calkins Report as we shall call it, was submitted to the Board three months after the October meeting, in January 1954. The core of the fifty-two page document lies in its recommendations for 1. research 2. dissemination and utilisation of knowledge and 3. development of professional personnel. In form, at least, it adhered to the recommendations of the Gaither Report, with added emphasis on the development of personnel. Under economic research, four priority areas were identified (a) improvement of organisation, administration and performance of economic units, (b) achievement of growth, development and greater economic opportunity without undue stability, (c) attainment of appropriate balance between freedom and control, and improvement of public and private policies, and (c) improvement of economic relations among nations. The report also recommended operating procedures for EDA, agreeing with Carroll that existing institutions be used but suggesting also that an adequately large permanent staff be maintained and that outside consultants be engaged periodically to oversee the program's development.

The research areas proposed by the economists were comprehensive. Indeed, it could be argued that few areas of interest to economists were neglected.

1. Improvement of the organisation and performance of economic units. This focused on studying decision-making by agents such as households, individuals and other groups such as firms and trade unions. As we shall see, there was concern with labor unrest at the time and efforts in this area, it was hoped, would help reduce it. But it was broader than that and the range of project areas suggested in this category virtually encapsulated all of microeconomics: investment; consumer behavior; firm behavior; government and group (e.g. trade union) behavior; public policy; motives and incentives in organisations; and the supply of business leadership.

2. Economic growth and stability. The need for economic growth with minimal inflation was unquestioned and the proposed topics to be studied again virtually all of macroeconomics: consumption and investment; business cycle prediction and prevention; inflation; economic growth; economic and social problems related to growth; growth in underdeveloped countries; and demography.

3. Freedom and control and the improvement of public and private policy. There was need to determine the appropriate role of government in a free enterprise economy, to reconcile private and public interest and, related, to discover the effects of specific government policies. The projects suggested describe broadly the area of public finance: economic effects of military expenditure; role groups in public policy and income distribution; reconciliation of government control and personal freedom; the limits to the size of the government sector; sectoral policies e.g. agriculture taxation; evaluation of specific policies; all the above in relation to foreign governments; and how to utilize more effectively economists in public policy.

4. International economic relations. Here, the aim was "the establishment of sound and mutually beneficial economic relations among nations". It was necessary to construct foreign economic policy; expand world trade while maintaining national security and economic independence: reconstruct a stable international monetary system; expand international capital and technology flows; smooth international adjustment to structural change; and examine colonialism and dependency problems. Again the areas suggested cover the fields of international trade, finance and development; the effects of changing terms of trade between primary commodities and industrial goods; U.S. adjustment to facilitate balance of payments improvement in other countries; the role of capital flows in development; the relation of food and primary commodity output to population growth; re-establishing convertible currency systems; the economic implications of the coexistence of capitalism and communism; policies with regard to dependent areas; and the consequence of greater European and "Free World" political integration.

In explaining the above areas, the emphasis was to be directly on problem-orientation: "good fundamental theory" would grow out of practical projects: "support is not given to disciplines as such". Research would be encouraged in those areas outlined by the Gaither Report. The importance of developing and testing theory was restated, the goal being the construction of " a 'realistic' economic theory". An interdisciplinary approach was again stressed in which the cooperation of psychologists, political scientists, business and public administrators and others would be encouraged to extend the scope of economics, for example, formulating "a systematic alternative to the Marxist relationship between economic forces on the one hand, social and political forces on the other".

Apart from some relatively minor differences in emphasis among the committee members, the Calkins Report represented the consensus opinion of its contributors.[13] And just as it was delevered with unanimity by the Committee,

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[13] Lee Bach, in a letter to Tom Carroll, Jan. 18, 1954, suggested that a few large efforts be made rather than engaging many small enterprises as the Report suggested. John Lintner, in a

so too was it accepted by the Ford Foundation which, by this stage, was in great need of some structure to guide its efforts.[14] They now had a blueprint for an economics program. Furthermore, the economists had seized the opportunity and, on paper at least, had convinced the Foundation of their potential as problem-solvers and, at the same time, secured future support for not only a very broad range of research interests, but also, through fellowship and graduate support, the discipline itself.

Further EDA activity: an overview:

For the next year or so, till late 1955, affairs proceeded quietly. Attempts were made to interest some individuals, prominent in the publishing industry, in the dissemination aspect of the program's activities, without success. Various editors and bureau chiefs at Fortune Magazine, Business Week, New York Times, Wall St. Journal and others were solicited, with no apparent results.[15] Also interesting was a related attempt to engage J. K. Galbraith, at Harvard. Galbraith, having read the Calkins report, replied that he was "far from impressed" and that some of its recommendations "could involve a serious waste of money".[16] But again, there is no evidence of any further contact here.

For grant activity also, the years 1953 and 1954 were quiet, the main grantees being M.I.T. for continued work on the relationship between economic

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letter to Carroll on Feb. 10, 1954, stressed the need for a combination of empirical and theoretical work and advocated a multidisciplinary approach in selecting projects.

14 See Memo, Carroll to Gaither, Feb. 11, 1954, and to Neilsen, Apr. 23, 1954. These confirmed the program's adoption of the guidelines of the Calkins Report and signalled, furthermore, their intention to proceed with the support of graduate study and academics.

15 Letter, Carroll to Geo. Soule, Editor, Twentieth Century Fund, July 15, 1954. Microfilm, Area 3, 1954.

16 Letter, Galbraith to Carroll, Sep. 20, 1954, Area 3, General

development and political stability[17], University of Pennslyvania for interuniversity research on comsumer behavior, and University of Chicago for a large interuniversity project on labor in economic development. Work on input-output analysis was also supported. Yale also received a small grant of $25,000 to review its graduate program in economics, marking the beginning of both a prolonged, intimate relationship between that university and the Ford Foundation, and the Foundation's support of graduate study in general. Outside the universities, both the Brookings Institution and Resources for the Future were beneficiaries, the former for research on govermental activity and the U.S. economy, the latter as start of long-term general funding for its own activities in resource research and education. 1955 was marked by a substantial grant to the National Bureau of Economic Research in general support, and by further funding of Yale's review of its graduate program and a new program there to train foreign students for international technocratic positions.

In late 1955, Lloyd Reynolds took leave as economics professor at Yale to become Director of EDA. Reynolds had been a member of the Calkins Committee and had demonstrated considerable application and interest when he addressed hemself to that task. Now at the helm, he set about alleviating what he saw as the major obstacle to progress: the development of personnel in academic economics. He proposed an elaborate fellowship program for faculty and graduate students and the endowment of research professorships. Turning quickly to the academic community for advice, as suggested in the Calkins Report, Reynolds assembled another prestigious group, including such leading lights as Paul Samuelson (M.I.T.), William Fellner (Yale), Kenneth Boulding (Michigan), R. Aaron Gordon (Berkeley), Simon Kuznets (Johns Hopkins) and Joseph Spengler (Duke).[18]

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[17] This work by Bissell and Max Milliken at the Center for International Studies (CENIS), which later became embroiled in controversy and protest when students discovered links between it and the C.I.A. Milliken, Assistant Director at the C.I.A. prior to 1952, apparently continued to do C.I.A.-supported work at CENIS.

[18] The others solicited were Bach and Calkins, from the original Calkins Committee, and H. Bowen (Grinnell College); C. Burrill (Standard Oil New Jersey); R. Donham (Northwestern Business School); K. Funston (New York Stock Exchange); A. Grimshaw (Univ. of Washington); S. Slichter

Members of this group were to meet twice, in December 1955 and September 1956. In addition, a group was assembled to advise on the role of economic history and the history of economic thought, including Walter Rostow (M.I.T.), Alex Cairncross (World Bank), and Alexander Gerschenkron (Harvard), though this dialogue produced little. Reynolds, as we have mentioned, was quite keen to start funding research professorships and graduate fellowships. To this end, the consultants were asked to rank the top economics departments in the country. Consequently, Chicago, Berkeley, Columbia, Harvard and Yale received professorships in economics and Yale a further $1/2 m for its graduate program. Predoctoral, dissertation and faculty research fellowships increased substantially in 1956, going to thirty-five universitites and establishing a pattern which was to continue for the duration of the EDA program.

Part of the original stated intention of the EDA program, it will be recalled, was not to support the economics discipline in general, but rather to address important problems. As the program evolved, it was clear that very broad support was being given, not only in research areas but also in personnel training and economics education, the latter two elements taking up to 1/3 of the program budget. Under Reynolds, the program was directed by an economist and advised by mainly economists. The *de facto* effect of the Foundation's efforts was, indeed, support for the discipline. Not unexpectedly, the tension between this outcome and the intended problem-orientation was alluded to on occasion. At the September 1956 consultants meeting, Reynolds spoke of the need for general, longterm support for economics departments, but acknowledged that the Foundation Trustees would regard this as "aid and comfort of . . . economics departments [and not] solving world problems".[19] Nonetheless, several economics departments were to receive substantial support in subsequent years. An internal EDA memo in January 1957, further acknowledged this tension, albeit in a different way:

(Harvard); G. Stocking (Vanderbilt); A. Weimer (Indiana Univ.); Claire Wilcox (Swarthmore Coll.) and J. Williams (Harvard).

[19] Consultants Meeting, Sept. 29, 1956, Area 3, General Files

"The focus on problems is probably beneficial. It must be recognized that this leads us to controversial policy issues. It is often easier, both from the point of view of avoiding controversy and supporting top-notch scholarship, to retreat from problem-oriented research to more cloistered academic activity. There is room in the program for both".[20]

Mid-1957 saw the departure of Reynolds, who returned to Yale, and his replacement as Director by Neil Chamberlain. The latter brought a change of emphasis to the program and was "seriously concerned with encouraging research in business administration", i.e. the work of business shcools rather than economics departments.[21] His first step, as Director, was to assemble a group of consultants which, unlike Reynolds' advisors, contained relatively few academic economists.[22] Furthermore, in a report to business school deans and chairmen, Chamberlain emphasised the need for input into business management education from sociology, psychology and political science and announced a series of appropriate fellowships and awards: "Business is Too Important an Institution to be Studied by Only the Economists", proclaimed one report.[23] Consequently, the years 1957 to 1960 ---- Chamberlain left in 1960 ---- saw a dramatic increase in the funding of business education and research, especially in curriculum development, and although funding of economics did not suffer, discussion in and around EDA centered on boosting the business schools.

The years 1960 and 1961 were a period of relative turmoil at EDA with significant staff changes and, as a result, further changes in strategy. In early 1960, Chamberlain resigned as Director to go to Yale, and was replaced by

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20 Staff Meeting, Jan. 28, 1957, Area 3, General Files

21 Letter, Chamberlain to Deans of member schools of the American Association of Collegiate Schools of Business, Sept. 19, 1958

22 The group, which met Nov. 8, 1957, comprised Bach, J. Cassells (California Texas Oil Co. Ltd.); Prof. Richard Ruggles (Yale); Prof. Gardner Ackley (Univ. Michigan); Grover W. Ensley (Nat. Association of Mutual Savings Banks); Prof. Earl Heady (College of Commerce, State Univ. of Iowa); Prof. Douglas McGregor (Sch. Industrial Admin., M.I.T.) and Prof. John Jeuck (Harvard Business School).

23 "Ford Foundation Activities in the Field of Business Administration", EDA, Dec. 24, 1958

Kermit Gordon, from the EDA staff. Barely had Gordon assumed the leadership when he resigned, in January 1961, to go to Kennedy's Council of Economic Advisors, under Walter Heller. With that, yet another staff member, Oscar "Bud" Harkavy, took over. February 1961 saw the departure, too, of Thomas Carroll, Vice-President in charge of EDA, to become President of George Washington University. Carroll had overseen the program since its inception in 1953 and during his tenure, it appears, kept a low profile, saying little at meetings with consultants and having little visible impact on the course of affairs at EDA. Indeed, after his departure, his position at the Foundation was not refilled.

In early 1961, following discussions with Ruggles, Bach and Walter Isard, consultants to the program, Harkavy grouped the problems most worthy of attention under the rubric "Economics of Change". Key problems of adjustment to economic change were structural unemployment, innovation and technical change and arms control. Once again, the doors were thrown open to the economists who willingly congregated in June 1961 to discuss the economics of change and the type of support desirable in these areas.[24] The following two years saw a significant surge in support for related research in unemployment, technical change and arms control, with substantial grants going to Berkeley and Wisconsin for the first area, N.B.E.R. and Princeton for the second, and Columbia for the last.

By the mid-1960's, however, the socio-political climate was changing and new social priorities were gaining hold which were to spell the end for the special position economics had held at the Ford Foundation. Multi-faceted problems relating to the achievement of civil rights, race relations, the welfare of minorities, and urbanisation now loomed large on the political agenda. The EDA, in response, started funding research in employment of minorities and in related

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[24] Many of those present had advised the Foundation in some shape or form already: Ackley, Bach, Calkins, Chamberlain, Fabricant, Gordon, Ruggles, Schultz. New faces were Maurice Lee (U.N.C.), Mark Leiserson (Yale), Richard Nelson (Carnegie), Arthur Smithies (Harvard), and Lloyd Ullman (Berkeley).

educational improvement. The realisation was dawning, however, that a broader response was necessary and that economics, as it stood, afforded a somewhat narrow perspective. This conviction within the Foundation was strengthened in 1966 by the new President, McGeorge Bundy. A "Washington man" and a skeptic of economics while a political scientist and Dean at Harvard, Bundy placed more faith in action, directed to pressing problems, than in the fruits of academic research, however "problem-oriented" the latter might be. At his instigation, the Foundation's emphasis shifted markedly: both EDA and a program in Science & Engineering were phased out; education was given greater resources in the form of a special Division of Education & Research; and the problems of urban development and social change were given primacy. Funding previously granted through EDA now went through Higher Education & Research or International Affairs, as appropriate, and former EDA staff were reallocated to these areas. The situation then obtaining was later well characterised by Marshall Robinson, the final director of EDA before its demise: the Ford Foundation no longer "looked at economics as a uniquely important discipline, but . . . . as a useful tool for dealing with various problems on the social agenda".[25]

And So?

What is the main pattern of events emerging from the above? At the outset, we see a group of non-specialists, with a strong interest however in economic affairs, demonstrate a belief in the discipline and a willingness to support it towards the achievement of practical ends and the solution of problems. Despite some skepticism shown at an early stage, in the form of Bissell's disbelief in the potential of economics, the credulous prevailed and a program was started, ostensibly directed towards tackling policy questions. Quite soon, however, mainstream economists took hold of the program and succeeded in having broad support given to the discipline as a whole with sight soon being lost of the initial conception of directing economics to solve "real-world" problems. This continued for a decade before the skeptics were to reemerge and call a halt. A changing environment was making new demands of policy-makers and the

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[25] Ford Foundation 1979, Ibid

Foundation's new President, Bundy, no believer in the "trickle-down" effects of academic research, pulled in the reins. There arrived the end of era, during which the economics discipline had been greatly bolstered, and such support would now have to be found elsewhere.

Part II: Foundations and Economics

The case study above prompts consideration of the relationship between foundations and the economics profession. What characterizes this interchange and what do our observations suggest for the future development of the discipline? My thesis is that the relationship has been characterised by tension, reflecting fundamental divergences of interest and approach between the two groups. Furthermore, those features of the economics profession and discipline which gave rise to conflict in the past have become even more marked in recent years, suggesting that, for the foreseeable future at least, the vote of confidence afforded the economics discipline in the 1950's and 1960's by groups such as the Ford Foundation, is unlikely to be repeated.

First, somewhat trivially, foundations are important to academic research, no less now than in the early 1950's. Economists respond quickly to offers of research funding and, as Goodwin (1989) points out, foundations not only provide money but also publicity and acclaim. Under the EDA program, the Ford Foundation published several pamphlets publicizing, in layman's terms, work being done by economists under their patronage.[26] Today, we have MacArthur and Guggenheim fellowships and other highly prestigious awards.

Having said that, the relationship between sponsor and funded is a complex one. Each may have a different agenda and successful cooperation will depend on the extent to which diverging interests can be reconciled. The main characteristics of the early Ford Foundation were a desire to resist the spread of communist political influence, a belief that this could be done by preserving and fostering those aspects of the American political system which made it an attractive

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[26] *Ford Foundation, Prospecting in Economics, New York, 1966.*

political alternative, and a conviction that the economics discipline could help in this "mission". While it had broad goals and could delineate areas for the attention of the economics community, beyond this it was limited. Beyond a commitment towards "problem-solving", as distinct from supporting the discipline per se, the Foundation could do little and was dependent on the economists.[27] Engaging "insiders" to administer the EDA program no doubt improved communication with the academic community but, inevitably and perhaps unavoidably, it diminished the gap between the two resulting in the temporary "capture" of the former by the latter. Academic economists not only ran EDA for a period and provided review and advice, they also acted as referees for grant applications. Certainly, the Foundation ultimately controlled the purse strings, but in order to engage the economics community flexibility was necessary and this meant relinquishing some control.

Not only may the aims of a foundation be somewhat vague but gauging success in their achievement may be difficult. Increasing economic growth with stability, improving domestic and international economic relations and determining the appropriate role of government all seen eminently reasonable, but how a foundation should know when such aims are being achieved or to what extent this reflects their efforts, and not other historical factors, are not at all clear. Obviously, adequate passage of time is necessary to cast matters in perspective and look at the cumulative results of work in a particular area. This further reduces control and postpones accountability, and the main barrier facing the researcher is to justify a project by linking it to the foundation's broader aims. At the Ford Foundation, proposals emphasised the problem orientation and policy-relevance of the intended research, claims not easily disputed by EDA given the uncertainty about the outcome of such work.

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27 The stated intention not to support the economic discipline per se recurs throughout the history of the EDA program. Bissell's (1952) "Possible Activities of the Ford Foundation", stressed application and not the pursuit of knowledge as an end in itself. "The Economics of Change", an internal document discussed on June 16, 1961, again stressed not supporting the "professional field of economics per se". Oscar Harkavy (Associate Director EDA) addressing a Seminar on Simulation and Management Games at Carnegie Institute of Technology, in August 1961, claimed that "our intention from the beginning has not been to support the discipline of economics per se".

The issue of control and coherence was further complicated in EDA's case by the absence of internal program review, except through engaging external committees of largely academic economists. Serious consideration was given to the idea in 1965. It was recognised as a departure from the customary procedure of *ex ante* evaluation, i.e., not directly considering the results at all, and names for an evaluation committee were suggested. However, nothing came of the proposal and, ironically, in this instance it fell to the researcher himself to submit a report evaluating the completed project.[28]

The above are problems which any institution engaged in funding research is likely to face. The issues become more complex when the agenda of the economists is taken into account. First, in terms of *approach*, academic economists tend to differ from foundation staff: while the latter are often directed towards action aimed at helping others, economists tends to focus away from intervention and emphasise market solutions and individual self-directed action.[29] Goodwin (1989) claims that these fundamental differences of approach make for suspicion of foundations by economists and inevitably create tension between the two parties. There may also be *methodological* differences. Again and again, the Ford Foundation stressed its concern with application of theory and problem-solving and deemphasised abstraction or support for the economics discipline in general. The economics community on the other hand tends to deemphasise application and favour abstraction, today more so than ever before. Pressures in this regard were acknowledged repeatedly in the intercourse between the Ford Foundation and the research economists.[30] Among economists,

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[28] The project which prompted consideration of an evaluation committee was a $400,000 research effort in computer simulation of microeconomic systems by Prof. Guy Orcutt at the University of Wisconsin. See Grant File 61-104, Ford Foundation Archives, New York.

[29] The Calkins Report suggests that the Foundation should support research aimed at delineating goals and objectives, but should not "decide what ultimate goals people should pursue" (p. 9). This distinction between the positive and the normative is stressed in the Report and contrasts with the distinctly political agenda put forward in the Gaither Report.

[30] The Calkins Report claimed: "In every major problem area a more thorough understanding of fundamental processes is needed, and this research alone can provide" (p. 6). Furthermore,

concern for policy development and application continues to be less than that for theoretical research and academic discussion. Research is undertaken with, perhaps, a vague implicit notion of ultimate "policy" application, but also with the understanding that this can come about only after considerable inquiry and debate about the "validity" of "results". A third source of tension concerns the tencency for the economics profession to avoid serious working contact with other social scientists. Although the desirability of this was stressed repeatedly in Foundation documents, in fact it came to little and economists' actions in this regard never went beyond lip service.[31] If anything, this aversion among economists to a multidisciplinary approach has grown over time.

To speculate on what the future may hold, it is certain that foundations will continue to play a role, focusing upon particular causes and problem areas and engaging academic researchers, economists amongst them, to help fashion "solutions". That such exclusive attention will be given to the economics discipline seems unlikely, however. It no longer enjoys the prestigious role it had in the 1950's and 1960's and the perception is strong that the profession is given to ambivalence in matters of policy. Historically, economists have been prepared to enter the fray in the discussion regarding choice of goals in economic and social policy. Today, the self-perception of the dominant neoclassical school is that of a provider of value-neutral technical advice to policy-makers, who then use this in making political choices. Discussion of policy matters does not feature significantly in professional training in economics. Additionally, from the foundations' perspective, the predominant current dilemmas such as problems of urbanisation, racial discrimination and drug abuse ---- many of

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"although the Foundation should encourage the solution of immediate problems, it must be prepared to support . . . long-run efforts". "The development of fundamental solutions for critical problems . . . . will require heavy expenditures on research" (p. 7). In his farewell letter to the Orcutt project, Marshall Robinson pointed out the tendency for economists to "wander off into other areas dictated more by their own intellectual cuiosity than by the original design". Letter Robinson to Orcutt, Grant File 61-104, Ford Foundation Archives, New York.

[31]The Gaither Report called for integrating other fields such as psychology and sociology into economic research: "the study of economics can no longer be carried by progessional economists alone". The Calkins Report duly suggested interdisciplinary cooperation, where necessary, as a criterion for project selection, but the discussions leading up to the report revealed some unease among the committee members at the prospect of such collaboration.

which emerged in the late 60's ----- appeal not to any one discipline for a solution. Such problems are many-faceted and call for, if not overt cooperation between disciplines, then at least consideration from several different angles. Other global problems such as famine and environmental destruction are similarly complex, so that the economic analyses must be preceeded by extensive scientific inquiry. In the face of this, a repeat recruitment en masse of the economics profession is improbable and economists will likely be increasingly required to pool their resources with other disciplines.

Conclusion:

From 1950, for almost twenty years, economic research in the U.S. was heavily supported by the Ford Foundation. The initial impetus for this came from the prevalent postwar belief that the application of economic expertise to policy problems would preserve the attractiveness of the American political system relative to the communist alternative. Guided in its funding strategy by an intellectual composite of institutionalists and neoclassicals, they sought to *encourage economic research directed* towards policy questions. As their academic advisors became increasingly homogeneous, i.e., dominated by "mainstream" economists, this emphasis on applied research was steadily diluted: the Foundation ultimately found itself supporting, in addition, the whole gamut of academic activity from theoretical research to graduate program development. This uneasy alliance fell apart in the late 1960's when multi-faceted social problems threw the internal dynamic of mainstream academic economics into sharp relief: the latter's increasing abstraction and its intellectual isolation within the social sciences had now made it less attractive as a resource in the policy arena.

Concluding Overview

In the introductory chapter, we discussed how these essays are thematically linked and the various symmetries which bind them together. We pause here to consider various areas still in need of attention which might be addressed in future work. All three essays offer scope for development into larger, individual contributions.

Chapter 1 shows how game theory was first significantly shaped by the occurrence of war and the wartime involvment of mathematicians as expert advisors. This marked the beginning of the process by which strategic interaction became central to contemporary neoclassical theory. This is the question of greatest interest to economists and, as such, merits further work. Chapter 2, on the relationship between economics and defense is the most complete of the three. It raises a host of interesting issues, however, which might be more fully discussed in an expanded version. The central issue is the economists' acquisition of authority by providing a language, or thought structure, which gave direction where there was indecision, which made the confused seem more intelligible. During World War II, the advice of economists on strategy conflicted with that of other scholars and with that of some military planners. The same issue arose afterwards in the debates on strategic decisions with engineers at RAND, in the reactions of those military figures whose authority was challenged by the budgetary reforms of the 1960's, and in the shock evinced by Herman Kahn's "rational" approach to the prospect of nuclear conflagration. A larger version might develop further this theme of the use of economic rhetoric in the context of military strategy, paying greater attention, in particular, to its role as a systematic approach to the nuclear debate. The essay on the Ford Foundation, Chapter III, has the greatest potential for expansion. The Foundation's impact on the discipline at large is not explored in the detail the issue merits. While we certainly show how the Foundation became captured, to some extent, by the dominant academic interests in economics, we have left relatively unexplored the obverse influence, how the economics discipline itself was changed by such massive financial support. This would involve not only speaking to those involved in the administration of these schemes, in both the Foundation and the universities, but also examining the composition and trend of grants to economics by field and by institution. To what extent does the field of development economics owe its present configuration to the support given it by the Ford Foundation? How important was the special attention afforded the business schools, in the late 1960's, in giving them the prominence they have today?

Even as they stand, however, these essays say a good deal about writing the history of economic thought, particularly as it pertains to the period beginning in 1945. First and foremost, examining published economic work alone is, analogically speaking, akin to regarding the polished diamond while ignoring the work in the mines. There is much to be gained from delving beyond the journal pages to the earlier creative stages where numerous influences manifest themselves. Our understanding of what passes for economic literature is enhanced by the exploration of institutional archives and the personal papers of those involved. All of the essays in this collection rely very heavily on these sources. In addition, since many of the figures involved in developments since World War II are still with us, it remains possible to conduct personal interviews. In addition to providing extra factual information that the published record does not reveal, these interviews convey, especially to historians of later generations, a sense of how the participants saw their role at the time (or at least their recollected version of such!). While the methods of oral history may, to some, seem somehow trite or unscholarly when juxtaposed against the Talmudic analysis of great works or journal articles, their importance in shaping the interpretation offered in these papers cannot be overstated. They are crucial, at least in this author's view, and will figure prominently in the continued work to which these essays give rise.

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