Axiomatic Choice Theory Traveling between Mathematical Formalism, Normative Choice Rules and Psychological Measurement, 1944-1956

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Abstract

The following analysis is meant to contribute to a history of rational choice theory. More specifically, I provide a multi-layered account of rational choice theory in terms of its biography as a scientific object. I argue that its axiomatic version, choice theory traveled between different research sites, specified within the context of different mathematical formalisms and occupying different epistemic functions; it was being applied to prescribe rules of proper behavior, as representation of behavioral hypotheses, and as measurement device to capture individual values. New modifications of what I call 'axiomatic choice theory' did not fully replace old versions of it, which prevents the reconstruction of its 'travels' as a continuous process and acknowledges the different versions of axiomatic choice theory that are currently used in the social sciences, particularly in economics. Furthermore, by revealing the diversity of its manifestations within the context of social networks and within particular research sites, the account of axiomatic choice theory developed here will ultimately contributes to an explanation of the disunity and confusion surrounding current debates about rational choice theory and allows for providing a more nuanced picture of its nature and scope. Jacob Marschak's professional development is used as a guide through this history of axiomatic choice theory to illustrate its journey.

JEL classification: B2, B3, B 21

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1. Disunity about Rational Choice Theory

In this paper, I provide the first sketch of a narrative that traces axiomatic choice theory developed by von Neumann and Morgenstern and how it traveled through the Cowles Commission in the 1940s and 1950s. The paper is part of a larger project that aims to contribute to the formulation of a history of rational choice theory. Ever since the inception of the discipline of economics, the traditional rationality-principle has been of interest for methodologist.[1] At the latest since the second half of the 20th century, it has gained extraordinary prominence beyond economics. Under the label 'rational choice theory', the principle records a history of powerful applications from political decision making, military strategizing, over providing new approaches in social anthropology, to peace research and conflict resolution. Additionally, research on the conceptualization of rationality has bound extensive research resources in philosophy, mathematics and statistics, computer sciences, operations research, artificial intelligence, game and decision theory, and in the behavioral sciences more generally conceived. As such, rational choice theory not only constitutes the behavioral backbone of economic models, but has also found its home in other disciplines.

In spite of its apparent success philosophical debates concerning the potentials and limitations of rational choice theory continue to be characterized by disagreement. With its re-conceptualization of the decision-making processes of rational agents in form of some fundamental axioms formulated in set-theoretical language, some have praised rational choice theory as capturing the core of human decision-making and enabling the predictability of behavior. Yet, others have rejected it as ignoring how humans actually behave, as being a purely formal framework whose interpretation lacks resemblance with what has traditionally been characterized as human reason and its constituents. The extensive application of rational choice theory has furthermore fueled charges raised against the economics profession of imperializing the social sciences, i.e. of extending the use of economic tools to all social scientific problems, with a rather unsuitable framework.[2]

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[1] The literature on rational choice theory and its application in economics and the social sciences more generally is extensive. While the high years of rational choice theory were roughly between 1970 and 2000, Google Books provides an indication of the extensive amount of literature on rational choice theory; listing more than 3,300 books between 1995 and 2011 with "rational choice theory" in the title alone. In the same period, around 48.400 books contained the term "rational choice theory" somewhere in the main text.

[2] From ca. the 1960s on, the term 'economic(s) imperialism' has been used to denote the tendency to extend "economics to topics that go beyond the scope of traditional economic questions, which include consumer choice, theory of the firm, (explicit) markets, macroeconomic activity, and the fields spawned directly by these areas" (Lazear 2000, p. 103), while committing to the conceptual and methodological rules of mainstream economics (Hirshleifer 1985; Mäki 2009). Examples of prominent cases for 'economics imperialists' are Gary S. Becker in economics, James S. Coleman in sociology, and Jon Elster in political science.

Disagreement notwithstanding, the extensive interest in the topic raises hopes that the methodological status, the main constituents, the scope of application, and the interpretation of what the concept of 'rational choice' entails, must be agreed upon. Indeed, scholars frequently refer to the 'received view' of rational choice theory, the development and specification of which has become closely linked to economic scholarship (Gauthier 1974; Lagueux 2004; Peter 2003). Critics appraise rational choice theory as if it were a monolithic framework, as if that what has been labeled 'rational choice theory' was an established and recognized theoretical construct (e.g. Dupré 2001; Hausman 1992; Rosenberg 2012). Yet, despite its prominence, fundamental disunity exists among defenders and opponents alike with respect to the aforementioned aspects. Rational choice theory has come in nearly as many versions and denotes as many theoretical concepts as there are social scientists talking about it (Herne and Setälä 2004). While most critics consider it implicitly as a (long falsified) psychological theory of human behavior, others defend it as being merely a methodologically useful heuristic or a primitive from which economic reasoning is to depart. This fundamental disunity has given rise to extensive confusion in the literature and hampers the quality of existing appraisals.

One fruitful way of explaining this disunity is to approach rational choice theory historically. However, as yet no comprehensive history of rational choice theory has been written, presumably because such a history could be written in various ways and/or because it would require an extensive review of practices in various disciplines. Depending upon one's particular focus and understanding of what constitutes 'rational choice theory', such a history could be constructed as a narrative of the invention of *homo oeconomicus*, the emergence of game theory, as a history of mathematical economics, of Milton Friedman's second Chicago school and Gary Becker's specific version of consumer choice theory, or as the history of decision theory beginning with Daniel Bernoulli. One could also lay the focus on social sciences other than economics, the beginnings of social choice theory in political sciences, or the provision of rational choice explanations in sociology, beginning with James Coleman and Becker at Chicago's sociology department – all those theoretical endeavors have at one time or another been labeled as 'rational choice theory'.[3]

[3] Giocoli's *Modeling Rational Agents* possibly comes closest to a history of rational choice theory. Other examples for historical treatments are Amadae (2003) and Erickson (2010), who placed the focus on the appropriation and use of game theory on conflict resolution at the University of Michigan in the 1950s and 1960s. One finds introductory chapters or paper sections that provide some overview of a history of rational choice theory either as introductions to books on game and decision theory, papers within the larger context of the rationality-conception in economics (e.g. Cudd (1993) and Hollis and Sugden (1993)), and histories of consumer theory or microeconomics (Hands 2006; Mirowski and Hands 2006). For some work about specific aspects of the history of decision theory, see for example Guala (2000), Heukelom (2010), Jallais et. al. (2008), among others. For an example for an increasing interest in applying game theory in social anthropology and the behavioral sciences more generally, see the edited conference volume by Buchler and Nutini (1969). Yet, those histories do not appear

Bits and pieces of such histories have been produced, frequently set into distinct institutional, social and political contexts (e.g. Amadae 2003; Davis 2003; Engerman 2010; Erickson 2010; Fishburn 1991; Giocoli 2003; Guala 2000; Hands 2006; Lagueux 2004, 2010; Leonard 1992, 1995, 2010; Lewin 1996; Mirowski 2002b; Mirowski and Hands 2006; Morgan 2006; Weintraub 1985, 2002). Yet, those histories, while valuable with respect to their respective focus, do not provide a comprehensive explanation for the disunity surrounding current debates about rational choice theory. Furthermore, by placing a strong emphasis on particular contexts that are considered as providing a comprehensive explanation of the emergence of rational choice theory (e.g. the Cold War context in American social sciences), they frequently settle for a stylized narrative at the expense of a more multi-facetted picture that a theoretical framework such as rational choice theory would require.

My analysis complements rather than contradicts existing historiographies that are concerned with and have in one or another way contributed to the history of rational choice theory. Yet, while my story is meant to dovetail into existing accounts, I depart from existing narratives in that I take the diversity in historical accounts of rational choice theory as one indicator for a parallel development and existence of various manifestations of rational choice theory in the 20th century. I aim at developing a narrative that contributes to an explanation for the conceptual, methodological and epistemological disunity and partly alleviates economists from the accusations of 'economics imperialism'.

Two overarching arguments underlie my general project: First, by tracing the emergence of 'rational choice theory' and the use of the axiomatic method in American economics between 1944 and 1956, I argue that throughout this early period rational choice theory did not appear as a unifying psychological theory of individual behavior – the opposite was the case: Rational choice theory underwent various modifications arranged to fit distinct discipline-specific problems and was fundamentally shaped by different disciplinary orientations as well as by the prevalence of diverging epistemic interests. Second, I argue that contrary to alternative narratives that proclaim economics imperialism, rational choice theory's early history suggests, if at all, a tendency towards 'rational choice imperialism'. It originated in different *research sites* that were not only more isolated from the day-to-day business of academia in the average economics department, but were also characterized by distinctive scholarly culture. Rational choice theory subsequently spread throughout the social sciences having a large impact on the economics profession that to its effects, however, had been comparable to that on other social scientific disciplines.[4]

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coherent, which might be explained by the complex task at hand and the different concepts and traditions such a history would have to consider.

[4] Note that this draft so far only contributes to those two arguments that will have to be further developed in a more advanced version.

As such, I do neither consider rational choice theory to be solely the result of the Cold War context, or exclusively reflecting the increasing mathematization of the economics profession. Nor do I consider its emergence and further development a linear process of gradual scientific progress or degeneration. Recent historiographers, such as Giocoli (2003), Amadae (2003), Lagueux (2010) and Davis (2003), depart from the premise that the story of rational choice begins with the history of the neoclassical theory of consumer demand, which has its roots in the work on cardinal utility theory elaborated by the Marginalists and further modified by Alfred Marshall and Vilfredo Pareto. Their stories characterize the development of the theory as a process of transitions as it experienced a fundamental change with Pareto's preference-based approach, the ordinal revolution, and with Paul Samuelson's revealed preference theory that finally laid the foundations for the choice-based approach in modern economics. In other words, those existing works presuppose a stringent sequential narrative in terms of how the story of the rationality-principle can be (rationally) reconstructed, that it underwent a transition process of successive modifications, re-formulations, and replacements and finally, and justifiably, *became* rational choice theory.

While drawing upon those narratives, I make an attempt in this project to contribute to a more differentiated understanding of the emergence and usage of the different kinds of 'rational choice *theories*' within distinct context of actual economic practices, the behavioral sciences movement and the history of post war social sciences in general. While I believe that such a history is valuable in its own right, this approach also enables a better understanding of rational choice theory's nature, explains the existing disunity in the current literature and thereby creates a basis for bringing some clarity into the clouded debates. Furthermore, such a history allows us to address a more fundamental question of *how theories travel within and across disciplines*, for which rational choice theory appears to serve as a fruitful case study.

My narrative is situated largely in the context of Jacob Marschak's professional biography between 1944 and 1956. Marschak's biography serves as a common thread guiding us through those research sites that appear relevant for tracing the emergence of rational choice theory in that period. Marschak's concerns largely reflect the development and application of rational choice theory within specific institutional settings such as the 'Cowles Commission' and the 'Center for Advanced Study in the Behavioral Sciences' and elicit the changing justifications voiced by economists for using the axiomatic method in economics.

Although Marschak has in various ways played a fundamental role in promoting rational choice theory among mathematical economists and even though he has himself contributed towards its theoretical development, there has, up to the present day, been relatively little

scholarship on Marschak's work and life.[5] Marschak held key positions in institutions such as the Cowles Commission, the RAND Corporation and the Center for Advanced Study in the Behavioral Sciences. How rational choice theory traveled among and between different research sites becomes visible in Marschak's persona, traveling and corresponding back and forth between those institutions. Marschak fostered interdisciplinary research efforts on human action and the formal representation of decision-making processes, a feature that equally applied to the development of rational choice theory.[6] He was one of the protagonists in developing rational choice theory further, explicitly arguing for its interpretation and usage as an empirical and later as a normative theory that allows for prescribing rules of conduct in situations where future prospects are uncertain. Finally, his personal stance towards rational choice theory changed throughout his career within a more profound transformation (Cherrier 2010), when he began working closely with (mathematical) psychologists, making axiomatic choice theories fruitful for them.[7] As such, Marschak's own work and academic positions reveal transformation and also reflect difficulties, tensions and contradictions that axiomatic choice theory underwent in its development.

But not only do Marschak's institutional background and scholarly interests make it interesting to let his biography frame a history of rational choice theory. He approached science in an open and tolerant way, letting himself be guided by problems and their solution rather than by method and dogma. As Kenneth Arrow remarks in his biographical memoir of Marschak:

> He looked at the problem in hand from every useful angle, drawing on every good idea and theoretical presupposition, then subjected it to severe criticism regarding utility, clarity of expression, and contribution to the understanding of economic issues. This process allowed him to be remarkably open to the new ideas and methods, which he would then transform with his own improvements and clarifications. (Arrow 1991, p. 130)

This way of approaching economics as a problem-orientated scientific enterprise, requiring changes if problems demand distinct solutions, makes Marschak a particularly fruitful scholar for tracing the emergence, modifications, and the variety of uses of rational choice theory. Departing from and oscillating around Marschak's professional life allows for tracing the beginnings of axiomatic choice theory within an institutional context thus provides a more nuanced characterization of rational choice theory than the view given extensively in the literature to date.

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[5] Exceptions for accounts of Marschak's professional life and work are for example Arrow (1991) and (1978), Cherrier (2010), Hagemann (2006) and Radner (2008).

[6] For example, his correspondence with economists, mathematicians, statisticians and mathematical psychologists such as Luce, Savage, Debreu and Donald Davidson reflects a lively exchange between the major scholars in this field.

[7] For example Marschak (1950, 1951).

Finally, it has to be remarked that investigating into the historical roots of rational choice theory requires at least some idea of what we take it's most basic constituents to be. Two political scientists, Kasia Herne and Maija Setäla (2004), have made an effort to investigate into the commonalities of the various versions of contemporary rational choice theory. Limiting themselves to thirty cases in political science, they conducted an analysis of what they called 'rational choice models' and have identified three common features shared by all those models.[8] First, while rational choice models differ with respect to how the rationality-concept is interpreted, all approach human behavior as being at least minimally rational behavior, i.e. agents have either a "consistent preference ordering" according to which they make choices or that their actual choices are consistent.[9] Second, they observe that all rational choice models rely upon the "axiomatic method", i.e. all rational choice-models are constructed on the basis of a set of axioms, whereas most models make use of mathematical techniques, also from logic. Third, they observe that all models rely upon some "micro-reductionism", i.e. social choices and interactions between agents are modeled in terms of the preferences of the individual agents (Herne and Setälä 2004, p. 68 f.).[10]

While those results leave much space for interpretation and further specification of rational choice theory, the two most general characteristics of rational choice as it became interpreted from middle of the 20th century on (what has become labeled 'Cold War rationality' in for example Erickson et al. (2013)) appear to be that its formulation relies on the axiomatic method and that rationality is interpreted in terms of some criterion for consistency. As such its history is in large parts a history of what I call 'axiomatic choice theory' that has become widely noticed mainly since the work of John von Neumann and Oscar Morgenstern and has spread in the second half of the 20th century throughout economics and the social sciences more widely conceived. This is where the paper departs.[11]

The paper is structured as follows: After giving a rough distinction between two different kinds of axiomatization that might help clarify and distinguish the different versions of

[8] The sample includes, as they say, models, theories, and theorems within different disciplines and different fields of application. For the list of analyses included in their sample, see Herne and Setälä (2004, p. 82 fn. 2).

[9] Herne and Setälä contrast this 'thin' rationality with 'thick' rationality. In the latter case, the content of the preferences of an agent is specified. Differences between the models arise as to the primitives of analysis: choices or preferences. In the case of models defining rationality in terms of consistent preferences, the concept of rationality is identified in terms of certain characteristics of the agent's preferences, i.e. the structure of the preferences and does not relate in any way to the content of the preferences in their rational choice-models.

[10] Note that the rational choice model's commitment to the micro-reductionism that Herne and Setälä identify does not imply a commitment to methodological individualism. In a footnote, they allow for actors in rational choice models that are individual human beings as well as collective actors that operate as an individual actor, such as a political party (Herne and Setälä 2004, p. 82 fn. 5).

[11] The terms 'rational choice theory' and 'axiomatic choice theory' are used interchangeably in the following. I am aware that their being one and the same has been an issue not agreed upon. I treat axiomatic choice theory as a particular version of rational choice theory.

axiomatic choice theory in section 2, I depart from John von Neumann and Oskar Morgenstern's contribution to the axiomatic version of rational choice theory, a collaboration that elicits already the secondary or at least partial role that economics played in characterizing their axiomatic decision theory. Section 3 highlights the mediating role that Marschak played by introducing von Neumann's and Morgenstern's axiomatic version of a theory of rational behavior to mathematical economists and the different functions it occupied for scholars at the Cowles Commission to address the problems they were concerned about. In section 4, I show how Marschak initiated a shift away from formal and towards more substantial interpretations of axiomatic choice theory. It is argued that this shift culminated in Marschak's interest in the behavioral sciences, which envisaged interpreting and applying axiomatic choice theory for the purpose of a general theory of human behavior, interpreted empirically and normatively. I conclude with a preliminary attempt of interpreting this historical sketch.

2. When a Mathematician meets an Economist: The *Theory of Games*

The gradual introduction of the axiomatic method and the development of 'axiomatic choice theory' respectively took place roughly from between the mid-1920s until the early 1960s (Grant and Van Zandt 2009, p. 22).[12] It was partly the development of game and decision theory and its application in economics that had a strong influence on the profession in this respect (Fishburn 1991, p. 27). More specifically, the seminal publication that is commonly identified with famously introducing the axiomatic method into economics is the crucial publication of the *Theory of Games and Economic Behavior* in 1944, a work of more than six hundred pages written by John von Neumann, a Hungarian mathematician and physicist, and the economist Oskar Morgenstern. The collaboration between these two scientists already reflected the hybrid character of axiomatic choice theory. At a closer look, the accomplishment of those two distinct individuals already questions the disciplinary homeland of axiomatic choice theory in economics. It suggests the history of axiomatic choice theory to be one of a scientific framework traveling from mathematics to, and across, the landscape of economics and other social sciences. More specifically, I argue that it was through Morgenstern, an economist that the mathematical theory developed by (and with) von Neumann could be made fruitful for what Morgenstern considered as some fundamental problems in economics.

[12] In line with e.g. Blume and Easley (2008), who speak in a similar way about "General Choice Theory", I subsume under axiomatic choice theories the attempts in 20th century mathematics, statistics, psychology, economics, etc. to formulate "axiomatic choice theories", such as expected utility, subjective expected utility theory, game theory, etc. I do not have to specify them further, as it suffices at this point to consider that they are *axiomatic* choice theories as opposed to empirical and/or behavioral choice theories.

The Axiomatic Method in Economics

At the time of Marschak taking up the research directorship at the Cowles Commission in 1943, the axiomatic method was not yet widely accepted among economists (see e.g. also Cherrier 2010, p. 451). Only very slowly did economists fully accept its introduction to their field during the second half of the 20th century.[13] Von Neumann and Morgenstern's *Theory of Games* had doubtlessly been a turning point for the social sciences in this respect (e.g. Boumans and Davis 2010; Debreu 1986). As Marschak (and others) immediately recognized, its first chapter contained a 'theory of rational behavior' formulated in axiomatic terms, which was only one but a very important contribution that could be made fruitful for economic theory.

Roughly speaking, the use of the axiomatic method can be understood as a systematic way to formulate concepts by the use of logical rules of inference (Stigum 1990). It goes back to the last half of the 19th century and is concerned with the 'ordering of principles' and the internal logical coherence of formal structures. As such, it implies the "organization of a deductive system in a strictly axiomatic form" (Blanché 1973, p. 162).[14] While there had been undertakings in economics at the beginning of the 20th century to represent individual behavior through a set of formal axioms, what became axiomatic choice theory was conceptually distinct from those early attempts. The difference was primarily reflected in the kind of mathematics, the function axiomatic choice theorists attributed to mathematics, how they justified its use, and the problems to which the application of mathematics helped to find a solution.

In particular, we can distinguish between two approaches to axiomatization that can both be found in economics in the first half of the 20th century (Giocoli 2003). They were distinct mainly with respect to the reasoning procedures involved, their relationship to empirical evidence, and the purposes to which they were applied. The first approach to axiomatization sought to validate results that had already been established empirically. The idea was to start from an empirically confirmed theory and then see whether the results of the theory could also be obtained through deduction from a limited set of axioms that were necessary and sufficient to theoretically establish the result. Here, the interest lied primarily in empirical analysis that motivates the introduction of the axiomatic method in order to ensure that the theory would fulfill the prerequisites necessary to conduct the empirical analysis. In contrast,

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[13] Giocoli (2003) makes this case with respect to the late acceptance of game theory in the economics profession.

[14] While one of the first approaches to an axiomatic treatment can be found in Aristotle's syllogistic theory and though Euclid was considered the initiator of axiomatization, the first appearance of the term 'axiomatics' can be found in an entry of the *Enciclopedia Italiana* only in 1949 defined as "name adopted recently to signify that branch of mathematical science that deals with the ordering of principles" (Enriques quoted in Blanché 1973, p. 163).

the second approach to axiomatization was an empirically rather neutral procedure that was highly formal and as such open for application to a much broader set of problems.[15] According to the second approach, one started from primitive concepts, formulated axioms that had to satisfy certain consistency conditions, and deduced theorems from those axioms according to specific logical rules of inference. Those theorems, consisting only of a set of symbols, required some interpretation in order to be tested empirically, which was not automatically given by the axiomatic formulation of a theory. Rather, as Debreu for example put it, "an axiomatized theory has a mathematical form that is completely separated from its economic content" (Debreu 1986, p. 1265). This aspect allowed for applying the framework to structurally similar, but discipline-independent, problems and thereby made a theoretical framework highly flexible with respect to its use. Another characteristic was that independent from empirical reality, it could serve as building an axiomatic system that could be investigated only with respect to its internal consistency and mathematical properties.[16]

Given this distinction, von Neumann and Morgenstern's work was different from a first serious attempt by Ragnar Frisch to axiomatize consumer choice theory. In a paper entitled 'Sur un problème d'économie pure' published in 1927, Frisch had introduced two kinds of axioms to characterize consumer choice with the final aim of grounding econometric analysis of consumer behavior upon theoretical foundation and to arrive at a measure for marginal utility (Frisch 1971 [1926]).[17] Frisch's first attempt falls in the first category, as his primary aim was to theoretically support observable and/or already highly confirmed relationships and further estimate theoretically accepted relationships, such as between price and demand for

[15] It is actually a non-trivial question what empirically neutral could mean in this context. A choice of specific axioms as constraints on a particular formal system can be made, not because in the first instance those axioms are directly inspired by observable reality, but rather because they can be applied to particular problems of interest. As such, their choice would still be influenced by empirical reality. Yet, what is meant here is basically that the choice upon which the axioms are introduced is primarily based upon formal convenience and not because their structure reflect a particular structure of an empirical phenomenon that has already been observed and only requires theoretical justification.

[16] In general, three different meanings of the term 'axiom' can be distinguished: the first is an axiom as an immediately plausible principle (e.g. principle of contradiction). Second, the initial principles of a deductive system could be understood as hypotheses (i.e. premises and conjectures) whose truth had to be proved by testing the consequences to support the premises and not the other way round. An axiom then became a law of nature that had been validated many times of the experimental method (as opposed to the demonstrative method of mathematicians), of which one example was the axioms of Newton. The third meaning of axiom is an assumed and not deduced proposition (the modern formal understanding of axiom) (Audi 1990; Blanché 1973).

[17] While the question why the economics profession largely ignored Frisch's attempts to measure utility by the use of axiomatization is historically interesting in its own right, no answer can be provided here. One tentative suggestion is that as Frisch wrote his paper in French and published it in the unknown Nowegian journal *Norsk Matematisk Forenings Skrifter*, it became translated only by Chipman, which might have at least partly prevented it from having any influence. Another suggestion is that because Frisch abandoned his work on utility axioms in this subsequent work. In his subsequent book *New Methods of Measuring Marginal Utility* that he published in 1932, Frisch mainly introduced additional methods for deriving specific estimates of the properties of the marginal utility function from data by making specific assumptions about its functional form, such as about separability of goods.

specific goods. Abandoning the use of differential calculus and optimization techniques, the mathematical elements in von Neumann and Morgenstern's work originated in mathematical logic, the (axiomatic) theory of sets, proof theory, epistemic logic, and the theory of relations.[18] By providing a new set of tools, flexible and refinable with respect to the various kinds of problems economists, the social sciences and psychologists were subsequently concerned with, von Neumann and Morgenstern's theory of rational behavior set the ground for introducing an approach to axiomatization into economics that falls into the second category.

Distinguishing between the two approaches to axiomatization not only reveals the ambiguous empirical status of 'axiomatic choice theory' already visible during its birth years. It also allows us to see the different functions and interpretations axiomatic choice theory had between 1944 and 1956. Given the increasing acceptance of Lionel Robbins's definition of economics in terms of choice under scarcity and a widely accepted commitment to methodological individualism across the social sciences during the first half of the 20th century, introducing the axiomatic method of either kind into economics meant that human action could be formally represented by a set of axioms that were either validated by empirical observation, of self-evident character, or chosen in such a way as to secure the internal consistency of a formal-axiomatic system.[19] The axiomatic theory of choice developed in the *Theory of Games* was first and foremost a mathematical theory, not independent from interpretation (as for example Debreu's *Theory of Value* was), but largely open for application to a variety of structurally similar yet not necessarily economic problems of behavior and interaction. Subsequently, axiomatic choice theory largely provided a mathematical basis for internally consistent axiomatic systems, allowing for the formal deduction of theorems. As providing explanations or predictions of empirical phenomena was not their primary purpose, the axioms were, if at all, chosen only in a second step with respect to their usefulness to represent properties of human decision-making. Furthermore, it did not appear possible in the first instance to derive any meaningful hypotheses from such a system because without any interpretation no substantial propositions can be formulated and subjected to empirical testing.[20]

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[18] See for example Amadae (2003), who argued for the conceptual difference between Marginalist rational behavior and axiomatic rational choice.

[19] While the direct influence of Robbins's definition on how economics as a discipline developed is debatable, it has frequently been considered to be a turning point that enabled and accompanied several currents in economics, which have typified the development of axiomatic choice theory in 20th century economics. Backhouse and Medema (2009c; 2009b) for example claim that Robbins's definition, turning the focus of analysis away from wealth and resource allocation more towards the general concept of individual choice was closely linked to the acceptance of the axiomatic method in economics.

[20] Note that even if the choice of axioms may not be influenced by direct empirical reality, it might be by methodological considerations about which axiom system can be applied best to a specific

An Axiomatic Choice Theory for Economics: The First Chapter of the Theory of Games

In their *Theory of Games*, von Neumann and Morgenstern announced to present "a discussion of some fundamental questions of economic theory which require *a treatment different* from that which they have found thus far in the literature" (von Neumann and Morgenstern 1953 [1944], p. 1; italics mine). They had been searching for a theoretical solution to problems posed by uncertainty in games of chance. Building upon the analysis of two-person zero-sum games, they formulated the *minimax* theorem, a prerequisite for modeling strategic choice as minimizing the potential costs of a worst-case outcome in situations of risky prospects, and an axiomatic theory of expected utility.

The relationship between the *Theory of Games* and economics had already been made clear in the first chapter of the book entitled "Formulation of the Economic Problem", which also contained their axiomatic theory of expected utility. Before presenting their axioms, von Neumann and Morgenstern stated that the basic problems that arise from studying economic behavior have "their origin in the attempts to find an *exact description* of the endeavor of the individual to obtain a maximum of utility, or, in the case of the entrepreneur, a maximum of profit" (von Neumann and Morgenstern 1944, p. 1; italics mine). From their point of view, economics had failed to come to terms with the problem of this inexactness caused by excessive yet unsuccessful use of mathematics in economics on at least four levels: first, "the economic problems were not formulated clearly and are often stated in such vague terms as to make mathematical treatment *a priori* appear hopeless because it is quite uncertain what the problems really are. There is no point in using exact methods where there is no clarity in the concepts and issues to which they are to be applied. Consequently the initial task is to clarify the knowledge of the matter by further careful descriptive work" (von Neumann and Morgenstern 1944, p. 4). Second, in a similar vein, von Neumann and Morgenstern identified an inappropriate use of mathematical methods in economics that was either a pure translation from verbal into mathematical expression, which provided no further mathematical analysis or was unsatisfactorily handled. Third, they considered the empirical background of economics inadequate, i.e. they identified a fundamental ignorance about economic facts. And finally, they saw a difficulty in the kind of mathematical tools economists used to formulate their problems. The offering of mere mathematical assertions ("which are really no

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discipline or problem and thereby becomes empirically inspired too. As such, the difference between how for example the difference between Ragnar Frisch's and Gérard Debreu's choice of axioms can be understood as a difference in degree rather than a difference in kind. With respect to Debreu, it is the question in how far he was actually concerned with economic problems in his book *Theory of Value*. As will be argued later, while this work can be rather understood as an example of the second kind of axiomatization, it might be argued that he actually turned economic problems into mathematics and not the other way round. He might have chosen the constrains (like homogeneity) as axiom because economics required a particular set of constraints, while if he would have liked to apply the same kind of axiomatic framework to physics, he would have chosen a different set of constraints. While this is an important aspect, it will not be developed further at this point.

better than the same assertions given in literary form"), rather than formal proofs, had created difficulties according to their judgment without helping to improve the understanding of the complex problems economics had to cope with (von Neumann and Morgenstern 1944, p. 5).

As a means for circumventing the problems they identified, von Neumann and Morgenstern proposed a new 'theory of rational behavior', which they specified in the first chapter to address "the problem of *rational behavior*", their proclaimed general goal (von Neumann and Morgenstern 1953 [1944], p. 8 f.; emphasis mine). More specifically, in focusing on economics as a particular discipline, their aim was to find "the mathematically complete principles which define 'rational behavior' for the participants in a social economy, and to derive from them the general characteristics of that behavior" (von Neumann and Morgenstern 1944, p. 31). While they considered mathematics as still invaluable for such an undertaking, they had a set of tools in mind that was very different from infinitesimal calculus. The problem's "exact positing and subsequent solution can only be achieved with the aid of mathematical methods which diverge considerably from the techniques applied by older or by contemporary mathematical economists" (von Neumann and Morgenstern 1944, p. 1 and p. 6). They suggested a "mathematical theory of 'games of strategy'" (von Neumann and Morgenstern 1944, p. 1), within which "[t]he character of the procedures [i.e. of mathematical reasoning] will be mostly that of mathematical logics, set theory and functional analysis" (von Neumann and Morgenstern 1944, p. vii). What their approach implied for the formulation of economic theories was thus to actually abandon the mathematics that had been used since the Marginalists and to make use of mathematical logic, especially the theory of sets and the axiomatic method.

This shift in mathematical method had primarily an impact on the conceptualization of economic agents. While general equilibrium theory in the Walrasian and Paretian tradition had taken preferences to be ordinal and had described them using indifference curves in the theory of the household, von Neumann and Morgenstern assumed that utilities are "numerically measurable quantities" (von Neumann and Morgenstern 1944, p. 16) for whose "exact and exhaustive elaboration […] the use of the axiomatic method" would be required (von Neumann and Morgenstern 1944, p. 18 f.). They departed from the idea that agents have preferences over uncertain outcomes of the world that would be realized with a certain probability. Given a set x of outcomes and a set A of all the probability distributions over x, they introduced a binary relation and an operation that secured an ordering of the outcomes by satisfying the logically consistent axioms of completeness, transitivity, independence, and continuity.[21] As properties of this binary relation, those (rationality) axioms classified the

[21] The axiom system of von Neumann and Morgenstern was more refined than I present it here. This level of technical detail is, however, not important at this point. As a side note, it appears curious that

preference ordering as that of a 'rational agent' (von Neumann and Morgenstern 1944, p. 26 ff.).[22] They showed that the preferences of a rational agent could be formally represented by a real-valued utility function. Expected utility theory then said that the agent would compare every two uncertain outcomes with respect to their expected utilities and would choose the option with the highest expected utility.[23] As such, von Neumann and Morgenstern revived the problem that Bernoulli in his solution to the St. Petersburg Paradox had addressed but provided an axiomatic foundation.[24]

The development of a "theory of rational behavior in a social exchange economy" was an important product of developing "the theory of 'games of strategy'" (von Neumann and Morgenstern 1944, p. 31). Von Neumann and Morgenstern were well aware of the idealized picture of the individual embodied by their axioms. Yet those axioms were chosen in order to derive a specific result: "[t]heir thinking about the possibility of a numerical representation of utility led to an axiomatization of choices in risky situations; this allowed for the inference of the existence of a real continuous function as an order-preserving representation of utility", which could subsequently be proven analytically (Weintraub 1993, p. 88 f.).[25] Furthermore, confining their analysis to the 'average person' allowed for "concentrat[ing] on one (the subject proper of the investigation in hand), and to reduce all others as far as reasonably possible, by simplifying and schematizing assumptions" (von Neumann and Morgenstern 1944, p. 16). Thus, it was for formal convenience that the axioms of rationality were chosen; yet their goal was to axiomatically support the principle of expected utility.

Apart from mathematics, another innovative aspect of the theory of games of potential interest for economics was to acknowledge the interdependence among actors, an aspect that

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von Neumann and Morgenstern do not reference Frisch's first attempts to axiomatize the theory of consumer behavior.

[22] These axioms are a specific set of axioms necessary to derive the respective theorem analytically. Expected utility theory is, as such, an example of an 'axiomatic choice theory' in that the structure of the preference relation and the set of alternatives are explicitly stated. The same holds for L.J. Savage's 'subjective expected utility theory' for example.

[23] Note that von Neumann and Morgenstern take the concept of objective probability and adopt a frequentist interpretation of probability, i.e. the probability of a given event is measured in terms of the relative frequency with which that event occurs in a number of repetitions in an experiment. This was later supplanted by Savage's work on subjective expected utility theory, which covers decisions under partial beliefs.

[24] For the technical exposition of their EUT, see chapter 1.3. of the *Theory of Games*.

[25] Von Neumann and Morgenstern delivered the actual proof of the theorem only in the second edition of the *Theory of Games*, published in 1947. The minimax theorem used by von Neumann and Morgenstern in 1944 was based on the first elementary (that is, non-topological) proof of the existence of a minimax solution, proven by Emile Borel's student Jean Ville in 1938. In a series of notes from 1921 to 1927, Borel, a French mathematician and probability theorist, had developed the concept of a mixed strategy (assigning a probability to each feasible strategy rather than a pure strategy selecting with certainty a single action that the opponent could then predict) and had shown that for some particular games with small numbers of possible pure strategies, rational choices by the two players would lead to a minimax solution. Each player would choose the mixed strategy that would minimize the maximum payoff that the other player could be sure of achieving (Dimand and Dimand 1992; Leonard 1995; Weintraub 1985).

would have fundamental implications for a theory of society and of the economy in particular. Von Neumann and Morgenstern considered the fact that agents interacted with each other as crucial for economics. Contrary to the Robinson Crusoe case, the decision of an individual was affected by other individuals' decisions. Therefore, the latter should affect the former's considerations of the optimal solution. For economics this implied that utility and profit calculations of an agent would actually depend upon the behavior of other actors (e.g. consumers or firms), a fact that should be acknowledged in formulating the problem of individual decision-making. This had not been satisfactorily done in the language of calculus. By modeling individual decision-making as maximization under external constraints, other agent's decisions had played no role in the utility or profit function of consumer and producers.

Albeit those difficulties of, and potentials for, economic theory laid down in the first chapter, the axiomatic 'theory of rational behavior' formulated in the *Theory of Games* was not primarily developed as an economic theory. It was a mathematical theory, mainly developed by a mathematician for mathematicians. Especially for von Neumann, solving the fundamental problems of economics was not of primary concern (Leonard 1995, p. 753). Princeton's economics department at the time was not interested in those new developments of game theory and the implications it might have for the discipline. It was the mathematics department, which had been particularly strong in topology and algebra, the Institute for Advanced Study, with which von Neumann was affiliated, and the Fine Hall that provided the stimulating environment for discussing methodological questions, for exposing as well as further developing this "new mathematics of society" (Leonard 2010, p. 6).[26] To make game theory fruitful for economics, it required Morgenstern (and other economists) to occupy the position of mediators between a highly abstract mathematical theory and the discipline of economics with its diverse problems. More specifically, Morgenstern considered game theory as fruitful for economics because it allowed him to address the concerns in economics that had accompanied him already long before he had met von Neumann.

Von Neumann's Formal-Axiomatic Theory

That the *Theory of Games* and the axiomatic choice theory contained in it was primarily a mathematical theory, was especially reflected in the particular interests and academic background of von Neumann. Given his wide-ranging abilities, von Neumann's concern for economics should not be overestimated. In his first contribution to a theory of games, "Zur Theorie der Gesellschaftsspiele", published in 1928, von Neumann had already presented a

[26] For an account of those early years of game theory as well as the relationship between the economics and the mathematics department at Princeton, see Shubik (1992).

first proof of the minimax theorem.[27] The focus of analysis had been on parlor games and was not particularly concerned with economics (Leonard 1995, p. 735). This is not to say that he was not at all interested in economic problems. His paper "Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes" published in 1936 had become one of the most important papers in mathematical economics (Weintraub 1985).[28] And he was more generally concerned with social scientific problems. That von Neumann and Morgenstern believed economics to be the appropriate science to apply their theory to becomes evident in for example the several phases of finding the title of the book, in which economics had been present from the beginning.[29] Yet, apart from the fact that the book contained very few references to recent economics (Hurwicz 1945, p. 924), that the treatment of utility was not the priority of the theory was indicated with their apology for just bypassing the problem of utility, a "subject of so great a conceptual importance" (von Neumann and Morgenstern 1944, p. 16). However, more important was that von Neumann's main goal in replacing the dated mathematical foundation of traditional economics was that economics should build upon axiomatic foundations. This was, however, nothing particular of economics and would presumably also have applied to any other social science. Rather, this position resulted from a particular mathematical school that had on its general agenda the axiomatization of all scientific disciplines.

Von Neumann's views on the role of mathematics and his approach to axiomatization were fundamentally influenced by the mathematician David Hilbert (Leonard 2010; Weintraub 2002). The kind of mathematical formalism, defended by Hilbert, implied a specific approach to the axiomatic method in mathematics (Weintraub 2002). For Hilbert, a mathematical theory was composed of a consistent set of axioms and a number of theorems that could be deduced from those axioms. Hilbert's view was that many mathematized fields of research were characterized by a number of key propositions or principles, which could be considered as that field's axioms: "the progressive development of the individual field of knowledge then lies solely in the further construction […] of the framework of concepts" (Hilbert quoted in Weintraub 2002, p. 86). Hilbert's hope was that universal formalization and axiomatization would bring clarity and rigor not only to mathematics but equally to applied fields of research (Weintraub 2002, p. 89). As such, while the content of the axioms

[27] For a comprehensive historical discussion of von Neumann and Morgenstern's role in the emergence of game theory, see e.g. Leonard (2010).

[28] Von Neumann had made himself early contributions to economics such as his linear growth model; see also Leonard (1995) for a discussion on von Neumann's theoretical pedigree and position in the economics profession. In his 1936-paper von Neumann had solved the problem of "establishing an equilibrium in a uniformly expanding economy" and as such had solved the fundamental problem that economists were concerned with (Weintraub 1993, p. 77).

[29] The potential titles were 'Theory of Rational Behavior and of Exchange' (Sept. 1941), 'Principles of Rational Economics' (Oct. 1941), 'Mathematical Theory of Economic Behavior' (Dec. 1941), 'Theory of Games – and Its Applications to Economics and Sociology (Dec. 1941); see Rellstab (1992, p. 88).

and theorems through their potential interpretation were of interest to Hilbert, the abstract-logical structure inherent to an axiomatic system and its internal consistency was of prior importance (Backhouse and Medema 2009a; Weintraub 2002).[30]

As much of the current literature on the history of mathematical economics stresses, what is reflected in the *Theory of Games*, and as such also in von Neumann and Morgenstern's account of rational behavior, is that von Neumann was strongly influenced by Hilbert's research program.[31] He considered his theory of games to be another application of Hilbert's method, specifically the first application of Hilbert's axiomatic method to the social sciences (Giocoli 2003, p. 216 and 220). The *Theory of Games* contained "an axiomatic description of the game as a formal mathematical entity, complete with discussion of the completeness, freedom from contradiction, and independence of the axioms (Leonard 1995; 2010, p. 234). According to von Neumann and Morgenstern, axioms were also chosen for analytical rather than for purely cognitive reasons, with the ulterior aim of proving a defined mathematical theorem:

> A choice of axioms is not a purely objective task. It is usually expected to achieve some definite aim – some specific theorem or theorems are to be derivable from the axioms – and to this extent the problem is exact and objective. But beyond this there are always other important desiderata of a less exact nature: The axioms should not be too numerous, their system is to be as simple and transparent as possible, and each axiom should have an immediate intuitive meaning by which its appropriateness may be judged directly. In a situation like ours this last requirement is particularly vital, in spite of its vagueness: we want to make an intuitive concept amenable to mathematical treatment and to see as clearly as possible what hypotheses this requires. (von Neumann and Morgenstern 1944, p. 25)

In the person of von Neumann, Hilbert's quest to develop axiomatic foundations for scientific theories arguably played an important role in the development of mathematical economics, the introduction of the axiomatic method to economics, and the axiomatic representation of the behavior of individual agents. Hilbert not only changed the idea of rigorous scientific

[30] Weintraub (2002, p. 90 ff.), following Leo Corry, distinguishes between two dimensions of Hilbert's research program, his 'Axiomatic Approach' that entailed a quest for developing axiomatic foundations for mathematical and, more generally, scientific theories; and the 'Finitistic Program for the Foundations of Arithmetic' that sought proof of the consistency of arithmetic, logic or set theory. Weintraub considers only the former as having played a role in the development of mathematical economics in the 20th century, while the latter had been fundamentally questioned by Gödel's 'Impossibility Theorem'. It is in this sense that von Neumann's work on for example the axiomatization of quantum mechanics in 1928 and subsequently his early work on game theory should be understood, namely that scientific theories "should be formalizable in the sense that their fundamental theorems should be developed from a formal axiomatic base", i.e. in the tradition of the Axiomatic Approach' (Weintraub 2002, p. 95). Note that von Neumann's early equilibrium paper did not belong to the same set of von Neumann's papers in the late 1920s, but rather falls in a later period when Gödel's results about the 'unprovability of consistency' had already been made public in 1930, which, again, had implied the death of Hilbert's 'Finitistic Program' (but not of the 'Axiomatic Approach') (Weintraub 2002, p. 95 f.).

[31] More specifically, von Neumann was still influenced by Hilbert's 'Axiomatic Approach' when he wrote the *Theory of Games*, not anymore by Hilbert's 'Finitistic Program'; see previous footnote.

argument but also the idea of scientific knowledge and mathematical rigor in particular (Weintraub 2002). While mathematical rigor for economists like Pareto and Frisch had referred to the goodness-of-fit of models about the economy with economic data, in mathematical economics after the 1940s, rigorous models increasingly meant models based upon a cogent system of axioms, intuitively appealing yet internally consistent and conducive to mathematical proof. This was reflected in von Neumann's goal. Empirical testing, explanation, and prediction in economics appeared less essential, as it was the internal consistency of those axiomatic systems and the deductive proof of the existence of theorems that was of primary interest for mathematicians of this tradition.

Yet, while there was a tacit belief in 20th century economics that axiomatization would lead to the development of a better science (Boumans and Davis 2010, p. 25), von Neumann still conceived of mathematics as occupying an auxiliary function. It should be applied only insofar as it illuminated the real world. In a lecture published in 1947 called "The Mathematician", he remarked: "As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from 'reality', it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art.* […]. [A]t a great distance from its empirical source, or after much 'abstract' inbreeding, a mathematical subject is in danger of degeneration" (von Neumann cited by Murawski 2010, p. 209; italics in original). As such, von Neumann held that a system of axioms was meant to capture intuitive ideas about empirical reality and that mathematics should always lend itself to scientific inquiry.

Von Neumann's background is illuminating for a refined understanding of the nature and status of von Neumann and Morgenstern's 'theory of rational behavior'. His approach to axiomatization was von Neumann's response to questions about the construction of a "mathematics of choice behavior" on a very general level (Leonard 2010, p. 235). Yet, it was this view of mathematics inspired by Hilbert's formalism but not meaningful as an empirically independent enterprise that was exemplified in the *Theory of Games* (Leonard 2010, p. 234). As von Neumann was convinced that any use of mathematics was fruitful only when there existed a link between the real world and the abstraction, he would most likely not have agreed to some abstract spheres towards which his theory would travel over time. On the contrary, he believed that it "is possible to describe and discuss mathematically human actions in which the main emphasis lies on the psychological side" (von Neumann quoted in Leonard 2010, p. 234).

Morgenstern's Economic Problem

Leonard's (1995) and Rellstab's (1992) analysis suggest that most or even all of the technical part of the *Theory of Games* had to be credited to von Neumann and that

Morgenstern acted mostly occupied the function of a secretary (Rellstab 1992, p. 89). Yet, it was in large parts Morgenstern's contribution to make axiomatic choice theory and game theory more generally to fit economics, as it allowed him to address problems in economics he had long been concerned about. Morgenstern had set the theory of games into an "economic context" (Rellstab 1992, p. 89), a contribution that should not be underestimated. He played an important role not only in making the axiomatic 'theory of rational behavior' travel towards economics. Morgenstern was also able to draw von Neumann's attention and interest to problems of economics, as a "kind of catalytic factor" that attracted economists to this new mathematical theory (Morgenstern in his diary 1943 quoted by Leonard 1995, p. 753). Morgenstern was a perfect 'mediator' between the theory of games and the economics profession. On the one hand, he was critical towards contemporary economic theory, while at the same time he admired the exactness of mathematics and logic, seeing the potential personified in von Neumann for developing and using a new kind of mathematics needed to solve the problems that he believed his discipline was encountering.[32]

Morgenstern bridged the gap between the theory of games and economic theory by pointing to problems that economics would have to solve. As his correspondence, his notes and his published and unpublished works reveal, before coming to the United States and meeting "Johnny" at Princeton in the fall of 1938, he had been thinking about questions of optimal social organization and cooperation under conditions of limited foresight, the role and limitations of policy intervention, regulation and control to secure individual freedom and stability in a system committed to liberalism, as well as the place of economics within a theory of society.[33] More specifically, he had concerned himself with the problem of formulating policy regulation within a liberal welfare-maximizing society constituted by heterogeneous individuals with limited foresight.[34] His notes written in the time leading up to his collaboration with von Neumann show that Morgenstern was asking himself questions about the definition of, and relation between, individual and collective rationality, the interdependence of individual decision-making, as well as about the rational course of action

[32] Note that Morgenstern was highly influenced by the Austrian school and their idea of the 'logic of choice', yet his turn towards mathematics was what later separated Morgenstern from the Austrians such as von Mises; see Leonard (2010).
[33] See Morgenstern, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP.
[34] Those questions had been a natural consequence of Morgenstern's critical state of economics when he had still been in Vienna, attacking the economist's limited concerns for precision in the formulation of assumption structures (for example the existence of limited foresight and its implication for the possibility of a general equilibrium for example (Morgenstern 1935)) and in particular of the decision problem every individual agent in an economy is facing. For a detailed account of the early years of Morgenstern's career in Vienna, see Leonard (2010). For a personal account, see Morgenstern (1976).

that should be followed given other people's considerations within different social and political circumstances (see also Rellstab 1992).[35]

Against this backdrop Morgenstern's draft paper 'Quantitative Implications of Maxims of Behavior', dated May 18th 1941, should be understood. While this paper was never published, it contained the core issues about which Morgenstern and von Neumann saw overlap of their works.[36] It allows us to better understand the problem Morgenstern subsequently felt that an axiomatic treatment of individual decision-making would enable him to address. The paper was meant to offer a systematic elaboration of the problem of interdependency in decision-making that a theory of society would have to confront.[37] To appropriately formulate the different aspects of the problem (qualitatively), Morgenstern identified the relevant formal properties of human decision making, its underlying structure and leaving out "more inclusive principles", i.e. substantial hypotheses, that he thought would be sufficient for a 'theoretical study' of an "abstract system of acts".[38] He fully acknowledged the crude approximation of human action embodied in his assumption of minimal rationality but reminded that, to elicit different classes of maxims (or 'plans') of behavior that determine single acts, the material content would not be important.[39]

What he investigated - on a purely analytical level - becomes reflected in his crucial distinction between unrestricted and restricted maxims; Morgenstern defined the former as decision rules, which were meant to secure the desired course of action given a person's end ('minimum of rationality"[40]) and could be followed independently of other people also following it; and the restricted maxims, which Morgenstern defined as decision rules whose capacity to secure the group's or the individual's end depended upon whether or not other

[35] These notes interestingly reveal a concern about how individual and social choices relate to each other, as the individual might not be able or would not find it necessary to 'break up the aggregates' and as such accepting maxims that contribute to the common good require a "very strong belief (or an insufficient knowledge) to accept this maxim which for the system as a whole is doubtlessly true but may not hold true for the individual case" (Notes dated May 1940, folder 'Maxims of behavior, 1938-1970, n.d.', box 49, OMP).

[36] It has frequently been claimed that it finally entered as the first chapter of the *Theory of Games.* Comparing the first chapter of the *Theory of Games* with the maxims-paper shows, however, that the former is much more sophisticated. The chapter contains the axiom system and definition of the utility concepts as well as the precise exposition of the critique of neoclassical economics.

[37] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 1. Leonard (1995, p. 749) points to the influence that von Neumann had already on Morgenstern while the latter was working on the 'maxims'-paper. Rellstab (1992) finds that in May 1941 von Neumann even motivated Morgenstern to write the paper on his ideas about maxims of behavior. Yet, Morgenstern's concerns about social order, and the role of maxims in behavior for the stability of cooperation were most likely of genuine interest for Morgenstern, reflecting the early influence of Menger and his Viennese years.

[38] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 2 ff.

[39] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 1 ff.

[40] Ibid.; p. 2.

people in a society would equally follow the maxim.[41] Thereby, the effect of other people following the same maxim on one's own outcome could be a negative or a positive one. Whether following a maxim would be threatening (negative) or constructive (positive) for the outcome one is aiming for would depend on how many people conform to the maxim. Thus, the difference in those maxims lied in the existence of a "logical connection between the properties of this maxim and the number of individuals accepting and refusing it if the possibility of successful application is considered", that is with respect to their "quantitative implications".[42] As such, the restricted maxims could become a threat or a support for the social order in either maintaining it or leading to its breakdown, while the unrestricted maxims would have no implications on society. There would be threshold level of the group size, at which a restricted maxim would become self-destructive, inefficient or non-advantageous.[43]

That the individual's "intelligence" (i.e. foresight) was crucial to understand the quantitative implications of, and to establish and adhere to, a maxim with respect to the individual's decision problem appeared obvious.[44] Given that people differed with respect to their ability to foresee and to form correct expectations about other people's behavior, "[i]t is quite easy to see that the close one approaches the actual conditions the more complex the problem becomes and very little is covered in a first approximation like the present one".[45] The natural question that resulted for Morgenstern's original problem about the role of policy was then to "study the conditions under which it might be advisable to establish some rules and to exercise some coercion in such a manner that they would substitute for the intelligence and for the information of the individuals, or rather for the lack thereof. There is no question here of changing the maxims but merely of improving the chances of their successful application".[46]

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[41] Morgenstern defines (subjective) rationality as a person having "the belief that their acts will contribute in a given manner to the aims which they pursue", which is coupled with minimum "definiteness of maxims" so that their classification with respect to their effect on the outcome is unambiguous (ibid, p. 3 ff.).

[42] Maxims could have the form of an order or command for Morgenstern as well as just be an expression of the individual that, when followed, would guarantee the individual with a specific preferred result; ibid, p. 4.

[43] An example would be a baking run. The problem here is that "scientific studies should enable us to indicate where this point lies. We are, as yet, in no position to do anything of this kind, hardly even when the numbers of actors in this very small. This is especially true because the same deliberation has to be made by every one individual making up the group; we would thus obtain an extremely complex phenomenon"; ibid, p. 5.

[44] For the formation of the maxims, Morgenstern considers real information as well as imagination and beliefs as equally important. Behavior thus varies due to differences in understanding the effects, i.e. to "variations in foresight"; ibid, p. 7.

[45] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 9.

[46] Ibid., p. 15.

With respect to questions in modern economic dynamics, Morgenstern thought that the implications of such properties of maxims within distinct social and institutional settings were not discussed. He warned: "whether and how these plans can be assumed to be 'compatible' so as to insure either stability or equilibrium or both", it would be "advisable to revert to these problems only after further properties of the two classes of maxims have been examined".[47] Finally, the problem that Morgenstern elaborated from those thoughts was two-fold:

> The problem of choice which is at present treated so extensively in economics appears in a somewhat different light now that it can be seen that is not only a matter of making preferences between aims in a purely qualitative manner in order to establish a line of action of maximum subjective rationality. Not only do these quantitative factors have to be taken into account but also the further problem of their compatibility has to be solved by each single individual where such maxims occur. If the individuals are not aware of the existence of this problem then we clearly get a different kind of behavior than if they are aware of it and accordingly the structure of the theory would have to be modified. It does not need many words to make clear that this opens up new fields of investigations. The other part of the twofold problem is that it would be necessary to find out how the distribution of maxims and changes in this distribution affect the determinateness of any given social or economic process as investigated by current theories. It is possible that some of the difficulties encountered at present with regard to stability conditions in economics have to do with the presence of such quantitative factors.[48]

This problem of a precise formulation of the decision-problem of individuals and the consistency of individual plans was not only necessary for formulating the conditions for equilibrium in economics, but basically embodied the general 'logic of social behavior' according to Morgenstern, and was as such equally important for sociology.[49] For its desired

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[47] Ibid; p. 6; Morgenstern appears to go back and forth between a normative and a descriptive interpretation of the maxims of behavior. While I will not go into detail at this point, is an interesting question whether his project was primarily a normative one, as his notes are not only filled with references to Kant's categorical imperative and examples of ethical maxims in general, but in the 1970s he continued to think about Kantian maxims of moral behavior and limited foresight (see notes, folder 'Maxims of behavior, 1938-1970, n.d.', box 49, OMP).

[48] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 21, italics in original.

[49] Morgenstern clearly stated in a first outline of the paper: "Generally sociological. Significance for the understanding of the structure of society and of some principles of policy"; see 'Maxims of behavior: first outline' 18 May 1941, folder 'Maxims of behavior, 1938-1970, n.d.', box 49, OMP; italics in original; Even for Morgenstern, who particularly considered economics to be one, or maybe even *the* discipline, for making use of game theory, economics was just one possible discipline of application. Morgenstern was very critical of economics and challenged the ideas of the Austrian school, particularly their resistance to the use of mathematics in the social sciences, as he himself believed that developing social scientific theory required the application of the axiomatic method (Backhouse and Medema 2009a, p. 486; Leonard 1992, 1995, 2010). Morgenstern justified his focus on economics as follows: "The illustrations used in this paper will be taken, as a rule, from economics because there alone the beginnings of a theory of social behavior are discernible; furthermore, it is in the economic field where the most direct applications of results could be expected" (Morgenstern 1941, p. 1). His notes and draft relies heavily on the works of K. Menger and F. Knight.

precise formulation of the problem, however, "not to mention the solution"[50], "advanced logical techniques" would be required,[51] "which are not available at the present time".[52]

In thinking about choice rules, normative and descriptive, how they shaped and regulated social and economic life and how to derive efficient policy regulations against this background was thus the context within which Morgenstern believed that von Neumann's ideas could help addressing this problem in economics. Morgenstern's considerations implied a more complicated treatment of individual behavior within a society than traditional economics suggested, a fact that he considered "a most serious limitation".[53] He argued, "the necessary mathematics do not, as yet, exist. Consequently, it will be necessary to leave it to the professional mathematicians to work out solutions and it is not exaggerated to think that they will have a very had nut to crack".[54] Yet, while he had already for some time lamented the poor state of mathematical economics and hoped for a more exact formulation of economic problems, Morgenstern only took his ideas one step further when meeting von Neumann.[55] It was von Neumann, who could provide the 'systematic treatment' that would allow for adequately addressing the problems Morgenstern felt so pressing.

Axiomatic Method and the Economic Decision Problem

While their theoretical framework was not immediately taken up in economics (Giocoli 2003), von Neumann and Morgenstern's book did not pass without notice. Martin Bronfenbrenner (1950) later acknowledged the "new approach" as proposing "nothing less than the replacement of the present framework of theoretical and mathematical economics, based as it is on maximum and minimum conditions for the most part, by an *entirely new framework* based on strategy, allowing explicitly for uncertainty, and with greater scope for inter-actions between individuals" (1950, p. 484; italics mine). The numerical representation of utility, the provision of a proof of the minimax theorem, and the tools to arrive at this proof were impressive results and, as Leonid Hurwicz (1948) noted, had wide implications for a number of disciplines. Von Neumann and Morgenstern's discussion on mixed strategies "presaged an entirely new approach to the structure of economic optimization theory, an approach that led to a global characterization of objective functions and constraint sets through convexity arguments. (Local characterization, using partial derivatives or marginal conditions, had been the standard approach since the 'marginal revolution' of the latter part of

[50] See 'Quantitative Implications of Maxims of Behavior, 18 May, 1941, folder 'Maxims of Behavior, 1938-1970, n.d.', box 49, OMP; p. 22.
[51] Ibid., p. 9.
[52] Ibid., p. 1.
[53] Ibid., p. 22.
[54] Ibid., p. 22.
[55] For a detailed reconstruction of the collaboration between von Neumann and Morgenstern, see Leonard (1995) and Rellstab (1992).

the nineteenth century)" (Weintraub 1993, p. 88). What they, or better say von Neumann, had done was actually to apply the axiomatic method to the analysis of individual behavior and as such lay the foundations for the use of axiomatization in conceptualizing individual behavior, a project of interest not only for economists but across the social sciences at large.[56]

The new axiomatic theory could be applied to the problems that Morgenstern had outlined in his maxims-paper, as game theory captured the essential features of economic (inter)action and allowed formulating what a 'proper solution' to a given situation would be, given that players aim to behave rationally on the basis of specific choice rules. The axiomatic theory of rational behavior lent itself to economic interpretation because the choice and interaction problems in economics were assumed to be of a strategic type. Yet, a formal description of the structure of strategic choice did not entail that game theory would yield descriptions of actual reality. What von Neumann and Morgenstern sought to show was "that the typical problems of economic behavior become strictly identical with the mathematical notions of suitable games of strategy" (von Neumann and Morgenstern 1944, p. 2).[57] These game-forms provided "a *model description* of [economic life], that is, a description of its essential features" (Giocoli 2003, p. 246; italics in original). As such, game theory itself had no explanatory and predictive power for von Neumann and Morgenstern; it instead allowed for mathematical representations of situations of strategic interaction. Actors remained *black boxes*, their actions predetermined by the payoff structures, the strategies defined for choosing one option over another under the rules of the game (Giocoli 2003, p. 246 and p. 279).

Yet, the 'mathematical theory' of games neither had nor sought the status of an empirical theory, nor was it devised as a specifically economic theory. While von Neumann and Morgenstern addressed the problem of rational behavior by justifying its need in economics, it does not appear that they developed it as an economic theory in the first place.[58] As Hurwicz states, the problem that von Neumann and Morgenstern address is "to solve the problem of rational behavior in a *very general type of situation*" (Hurwicz 1948, p. 436; italics mine). As this problem was crucial to account for decision-making in general, its results "are of relevance in many fields of knowledge, among them economics and statistical inference" (ibid.). While economics was a convenient discipline to apply them to, the principles underlying strategic behavior were of a more general nature: "while the principles ought to be perfectly general – i.e., valid in all situations – we may be satisfied if we can find

[56] As opposed to Samuelson's choice-based account, for example, von Neumann and Morgenstern's account of the individual was again a preference-based approach as it had been before Samuelson's introduction of revealed preference theory.
[57] Hurwicz, who himself made fundamental contributions of RCA, interprets this not as 'identity' but rather draws the analogy between a theory of firm behavior to "elaborating the theory of rational behavior of a poker player on the assumption that he knows the strategy of the other players!" (Hurwicz 1948, p. 436).
[58] See e.g. chapter XI "General Non-zero-sum Games" of *Theory of Games* (1944, pp. 504-586).

solutions, for the moment, in some characteristic special cases", which was economics (von Neumann and Morgenstern 1944, p. 31). Thus, their suggested set of new mathematical tools not only allowed for applications in economics. Rather, it was the general theory of strategic interaction that had a high analytical value on its own and other important applications in a variety of fields of knowledge.[59] Furthermore, von Neumann and Morgenstern's primary goal was not limited to the theory of consumer behavior but was "designed to fulfill a much more general goal […]. Game theory represented the most mature fruit of the Hilbertian dream of colonizing all sciences, including social sciences, under the flags for formalism and axiomatics" (Giocoli 2003, p. 206); game theory was intended to become the first mathematical theory for the social sciences; and the axiomatic theory of rational behavior contained in it the first mathematical theory of human behavior.

Acknowledging this history and purpose of the axiomatic 'theory of rational behavior' are illuminating, for it fundamentally questions the claim that more recent manifestations of rational choice theory originated in economics, were developed for economic problems, and have subsequently spread throughout the empirical sciences in a process of 'economics imperialism'. Game theory and the 'theory of rational behavior' had been "primarily a product of mathematicians and not of scientists from the empirical fields" (Luce and Raiffa 1957, p. 3), and it should be appraised with respect to the problems they sought for it. More specifically, "the theory was originated by a mathematician and was, to all intents and purposes, first presented […] as a highly formal […] structure, thus tending to make it accessible as a research vehicle only to mathematicians" (ibid.).[60] However, as D. Luce and H. Raiffa in a later edition of their book stress, there is hardly a comparable case in which a theory of such mathematical character as game theory came to be applied at all in the social sciences. Bearing in mind these two dimensions - a "sophisticated theoretical structure" (Luce and Raiffa 1990 [1957], p. 3) and an applied theory of human decision-making - may improve our understanding of the kind of purpose the theory was originally meant to accomplish and provides the starting point from which we depart to trace how axiomatic choice theory traveled further.

[59] Note that the first chapter has been attributed to Morgenstern, as it draws the link between von Neumann's mathematical game theory and the problems of neoclassical economics, which is just another indicator that von Neumann and Morgenstern's axiomatic theory of expected utility had been primarily developed by a mathematician, not primarily for solving economic problems, and far less so for explaining individual behavior (Leonard 2010, p. 249).

[60] Game theory had at first a far greater impact in applied mathematics, especially mathematical statistics, than in the empirical sciences (Luce and Raiffa 1957, p. 3).

3. Turning Axiomatic Choice Theory into Mathematical Formalism

While axiomatic choice theory was prominently developed (mainly by von Neumann) and had gained a foothold into economics (mainly through Morgenstern), it soon developed a life on its own, independent or even contrary to von Neumann's views on the function of mathematics and the relation between mathematical theory and science. Searching for a first prominent station in the theory's journey leads us to the Cowles Commission in Chicago, the stronghold of mathematical economics during the 1940s and 1950s. As Weintraub and Mirowski (1994) have argued, the Commission was one, if not the most, important institutional context within which mathematical economics came to be understood as applying the highly formal axiomatic method to economic problems. The approach to axiomatization at Cowles increasingly turned towards being of the second type introduced in section 2.1., i.e. as a procedure that was in the first instance entirely formal, allowing for the deduction of theorems from empirically rather neutral axioms according to specific logical rules of inference, which ultimately distinguished axiomatic choice theory developed at Cowles (especially by Debreu) from von Neumann's theory of rational behavior.[61]

Marschak's years at the Cowles Commission, first as a director (1943-1948) and subsequently in various functions (research associate, research consultant, etc.), reveal that axiomatic choice theory in particular had a strong impact there. As it constituted a new kind of mathematics, it did not only influence the theory-focused research program of Koopmans but already had an impact during Marschak's directorship, whose initial focus still lied on work in econometrics and mathematical statistics. Becoming mainly developed further by scholars trained in mathematics, statistics and mathematical economics, axiomatic choice theory at Cowles departed from von Neumann's ideal to keep close connections between mathematics and the empirical world.

Introducing Game Theory to the Cowles Group

Jacob Marschak arrived in Chicago in early 1943. Besides becoming director of the Cowles Commission, he had also been appointed professor of economics at the University of Chicago.[62] Upon his arrival, Marschak undertook a fundamental reorientation of the Commission's research program that was reflected in three kinds of categories presented in the Commission's Annual Report for 1943, under which the studies undertaken at the

[61] Again, by 'empirically neutral' I refer to the selection of the set of axioms as a process not primarily guided by how well those axioms reflect actual psychological or physiological processes of deliberation for example.

[62] See website of the Cowles Commission: http://cowles.econ.yale.edu/P/reports/1932-52.htm#4. [accessed on February 25, 2013]. Note that the Cowles Commission, while affiliated with the University of Chicago, had been an independent research institution and was rather closely affiliated with the mathematics department. Düppe and Weintraub (2012) stress this mutually stabilizing relationship.

Commission could be subsumed: 1) wartime peace control and rationing; 2) studies in economic behavior; and 3) unemployment and business cycles. Especially studies in the second category were concerned with behavior of consumers, entrepreneurs, firms and investors, whose investigation and analysis required improved mathematical tools for econometric analysis. Marschak outlined the method for the studies that fell under the respective category as follows:

> The method of the studies [...] is conditioned by the following four characteristics of economic data and economic theory: (a) the theory is a system of simultaneous equations, not a single equation; (b) some or all of these equations include "random" terms, reflecting the influence of numerous erratic causes in addition to the few "systematic" ones; (c) many data are given in the form of time series, subsequent events being dependent on preceding ones; (d) many published data refer to aggregates rather than to single individuals. The statistical tools developed for application in the older empirical sciences are not always adequate to meet all these conditions, and much new mathematical work is needed. To develop and improve suitable methods seems, at the present state of our knowledge, at least as important as to obtain immediate results. Accordingly, the Commission has planned the publication of studies on the general theory of economic measurements. […] The available results of mathematical analysis are currently applied and tried out in econometric investigations; conversely, new situations arising in the course of practical work present new problems to the mathematician. It is intended to make this hand-in-hand work the basis of the Commission's activities.[63]

As Weintraub (2002, p. 118) notes, this was clearly an institutional orientation that placed the focus of the commission's research activities on econometrics, the application of mathematical statistics to empirical problems by using the structural estimation method (see also Cherrier 2010).[64] Yet, the *Theory of Games* immediately caught Marschak's attention. By the 1950s, Marschak had himself already worked on developing the formal axiomatic structure of choice theory further (see Marschak 1950). Even earlier, he had familiarized himself with the theory and had become excited particularly about the axiomatic version of Bernoulli's expected utility theory (see Arrow 1991, p. 140).

In 1946, after the first reviews of the *Theory of Games* by H. Simon and A. H. Copeland, an American mathematician working on the foundations of probability (his doctoral student was H. Raiffa), Marschak published an extensive summary of the book in the *Journal of*

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[63] See website of the Cowles Commission: http://cowles.econ.yale.edu/P/reports/1932-52.htm#4. [accessed on February 25, 2013].

[64] See also Mirowski (2002a) stressing the increasing interest of Koopmans in the *Theory of Games*, when it became discussed in the middle of the 1940s at the University of Chicago. As Koopmans states in a letter from Koopmans to A.H. Taub dated July 24, 1959: "I remember very well the discussions I had with von Neumann…there were several discussions in Chicago on the theory of games, just about at the time that the von Neumann Morgenstern book appeared. Reviews of that book by Hurwicz, Marschak, and Simon in various journals reflect in part the effects of these discussions" (Koopmans cited in Mirowski 2002a, p. 176).

Political Economy, of which the main goal was "to acquaint the reader with its economic results" (Marschak 1946, p. 98). In this review, Marschak presented the new "theory of rational behavior" not only as conforming to the proper standards of science (see also Cherrier 2010, p. 451), but identified the *Theory of Games* as an exemplary case of using new mathematical tools to develop economic theory in ways required for economics to make progress. The main achievement of the book according to Marschak lay "more than in its concrete results, in its having introduced into economics the tools of modern logic and in using them with an astounding power of generalization" (Marschak 1946, p. 114). He praised von Neumann and Morgenstern's "meticulous formalism", the separation between the axiomatic structure of the theory of behavior and empirical reality, the flexibility this detachment brought in its train with respect to its applicability, and the rigor and precision that an improved scrutiny of logical foundations and deductive analysis was accompanied by. That the purely formal framework of von Neumann and Morgenstern allowed for modifications of the theory and its application to structurally similar but quite distinct disciplines was immediately obvious to Marschak. What the scientist had to do is take those formal structures and modify them in such a way that they could be applied to the respective problem at hand:

> Such detached reasoning safeguards against any subconscious smuggling-in of undefined terms and operations or of assertions that have not been proved, yet had not been stated explicitly as axioms. The formal conclusions thus obtained are then materialized: they are translated into the language of the concrete field – games or economics in our case – and are thus prepared for empirical test. Not only are conclusions obtained in this way more reliable, not only do they more audibly respond to any deliberate modification of the premises, but they are also richer and better ordered and their mutual relations are revealed with greater clarity than if one had studied one empirical case after another, encumbered with every accidental detail and exposed to the risk of misunderstandings and fallacies. (Marschak 1946, p. 115)

In Marschak's view, the separation of formal structure and empirical interpretation was a virtue rather than a vice of the new theory. It reflected how economics should proceed in order to secure scientific reasoning. At least for the case of economics, Marschak concluded "Ten more such books and the progress of economics is assured" (1946, p. 115).

While it cannot be said that scholars at Cowles immediately took up on this new research field, the axiomatic method became increasingly established among the group of mathematical economists.[65] Doubtlessly, von Neumann and Morgenstern had a long-lasting influence on research at Cowles, especially, as Mirowski (2002a) for example argues, during

[65] That game theory was not immediately taken up at Cowles indicates for example the list of seminar speakers between 1943 and 1954; see http://cowles.econ.yale.edu/archive/events/seminars-cc.htm [accessed 8th of April, 2013].

Koopmans directorship.[66] As the 'Twenty Year Research Report, 1932-1952' indicates, next to activity analysis and welfare economics of the Arrow-type, one source of the "Commission's work on rational behavior lies in von Neumann and Morgenstern's *Theory of Games* […] the formal apparatus of the theory of games was taken over and applied to decision-making in many familiar situations, with a view to finding out what is the rational behavior appropriate to each".[67] But already before Koopmans, following the theory's publication, scholars at Cowles had been aware of the theory of games and it may be hypothesized that at least some of them (e.g. Marschak and Hurwicz) immediately saw the potential of axiomatic choice theory for economics.[68] Having been widely distributed among the Cowles scholars, the influence of Marschak's summary of the *Theory of Games* was not negligible in increasing the popularity of von Neumann and Morgenstern's work.[69] As Hildreth for example reports, "many economists became aware of game theory through the early review articles by Hurwicz (1945) and Marschak (1946). They explained the abstract concepts and supplemented the examples provided by von Neumann and Morgenstern" (Hildreth 1986, p. 98). By promoting the *Theory of Games*, Marschak, like Morgenstern, took on the role of mediator, acting as an intermediary between the abstract world of an axiomatic theory of behavior and a concrete field of potential application, in mathematical economics.

This interest in the new theory of games was accompanied in those early years with an equally cautious attitude at Cowles towards embracing the 'new kind of mathematics' to principally solve the basic economic problems. For example, Leonid Hurwicz, at that time research associate at Cowles, had written a (Cowles Commission) paper in 1945 entitled "The Theory of Economic Behavior", which equally contained a much more reader-friendly exposition of the theory, relating it to economic concepts. Hurwicz praised not only the

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[66] Mirowski (2002a, p. 174) takes von Neumann as the muse for Koopmans in the 1940s while he sees little impact of von Neumann's work at Cowles during Marschak's directorship. "Marschak had been acquainted with von Neumann as early as the 1920s, but there is little evidence that he ever really took to heart the full complement of Neumannesque themes, or ever managed to engage von Neumann's attention while research director at Cowles". While the cooperation and correspondence between von Neumann and Koopmans might have been more intense than with Marschak, the latter had doubtlessly an undeniable influence in establishing the *Theory of Games* and the mathematics contained in it at Cowles.

[67] Another influential work was Abraham Wald's *Statistical Decision Functions* published in 1950*;* see "Economic Theory and Measurement – A Twenty Year Research Report, 1932-1952"; Cowles Foundation website http://cowles.econ.yale.edu/P/reports/1932-52.htm#6 [accessed on April 2, 2013]. The Research Report of the Cowles Commission stated three different origins of the work on rational behavior. Arrow's work that founded social choice theory as well as Koopmans's work on activity analysis and linear programming, i.e. on optimal production decisions on efficient input-output combinations constitute two other directions towards which 'rational choice theory' traveled, which can, however, not be treated here.

[68] Von Neumann himself had promoted their new theory at the Commission, giving a seminar on the 25 and 26 of May 1945 entitled 'Theory of Games and Economic Behavior'; see http://cowles.econ.yale.edu/archive/events/seminars-cc.htm [accessed on April 2, 2013].

[69] The discussion culture of distributed papers at Cowles suggests that it had been read and discussed widely among scholars like L. Hurwicz, T. Koopmans, K. Arrow, L. Klein, D. Patinkin, and H. Simon.

book's outstanding importance beyond economics,[70] but also remarked on its being "essentially constructive: where existing theory is considered to be inadequate, the authors put in its place a highly novel analytical apparatus designed to cope with the problem" (Hurwicz 1945, p. 909). According to Hurwicz, the book presented a solution to the problem of defining rational behavior always 'just' as maximizing utility, a problem already well known since Cournot's work on duopoly: "There is no adequate solution of the problem of defining 'rational economic behavior' on the part of an individual when the very rationality of his actions depends on the probable behavior of other individuals" (Hurwicz 1945, p. 909).

At the same time Hurwicz voiced criticism of some of the book's aspects, reflecting an economic point of view at that time, which would not yet imply an unconditional commitment to game theory. Hurwicz believed that, apart from the potentials which the generality of this new approach offered in leading "to revamping, and enriching in realism, a good deal of economic theory […], the results [of the *Theory of Games*] are far from the degree of concreteness desired by the economic theorist" (Hurwicz 1945, p. 923). Reflecting the focus on econometric work of the Cowles Commission at the time, Hurwicz questioned the unconditional embrace of axiomatization and rigor at the expense of solving actual problems in the economy. While a model providing economists with the general properties of an economic system, of whose analysis monopoly, duopoly and perfect competition would just be special cases, would be more than welcome by the profession, "such a model is not yet in sight" (ibid, p. 932). In its absence, traditional economic theory would still be useful: "economics cannot afford the luxury of developing in the theoretically most 'logical' manner when the need for the results is as strong as it happens to be the case of the ups and downs of the employment level!" (ibid.). Furthermore, while Hurwicz appreciated for example the theoretical derivation of coalitions from an axiomatic basis, "we do have empirical knowledge which can be used as a substitute (again imperfect) for theory" (ibid, p. 924). In sort, Hurwicz argued that while departing economic reasoning from empirical observations would frequently include logically unnecessary assumptions and would entail sacrificing mathematical proof, it would still be useful to address economic problems.

The Role of Individual Choice for Mathematical Statistics

While game theory was only one and not the most important branch of research out of several that people at the Cowles Commission were working on, a major concern that became apparent already during Marschak's directorship was to define in a theoretically rigorous manner rational economic behavior under uncertainty. One reason might have been that

[70] "The scope of the book is much broader. The techniques applied by the authors in tackling economic problems are of sufficient generality to be valid in political science, sociology, or even military strategy" (Hurwicz 1945, p. 909).

Marschak became increasingly committed to the definition of economics in terms of individual choice and its underlying logic (Cherrier 2010, p. 447), a step that made him a pioneer rather than a latecomer in committing to Robbins' definition in the second half of the 20th century (Backhouse and Medema 2009c). A more important reason, however, was a theoretical problem that Marschak's research program posed: The program mainly encompassed statistical estimation and testing of structural relationships. More specifically, the general agenda intended for the Commission was the postulation of economic relations restricted by economic theories and their subsequent systematic testing and estimation by statistical tests on the basis of data sets (e.g. Hoover 2006, p. 241).[71] By placing emphasis not only on data-based testing but also on how theory could inform hypothesis-formulation and estimation of model-parameters, the role of individuals entered this work on two levels: at the level of data gathering and testing and at the level of theory, hypothesis formulation and parameter estimation. Furthermore, the problem of statistical inference was itself conceived as being one of "rational behavior of the statistician" (Hurwicz 1948, p. 437), demanding a clear formulation of what a 'rational choice' entailed.

The importance that Marschak began to place on individual choice behavior in econometrics was not new. At the beginning of his career, Marschak had worked mainly on monetary theory and econometrics, subjects in economics that are mostly concerned with aggregate phenomena, large data sets and questions of measurement. Yet, in his early work on demand theory and the measurement of elasticity that he undertook for his habilitation in Berlin and Heidelberg, Marschak had acknowledged the importance and usefulness of data about individual households such as family budgets for deriving the demand function and as such generate the aggregate demand curve not from market data but from looking more closely at the individual level.[72] During his time at the Oxford Institute of Statistics from 1935 until 1939, Marschak had developed a theory of money demand under uncertainty, which was not only an early instance of mathematical modeling, but also an attempt to cope with the idea of decision making under uncertainty with methods from mathematical statistics. Arriving on a Rockefeller Fellowship in New York and taking up a position at the New School for Social Research, Marschak held a small econometrics seminar at which T. Haavelmo, J. Schumpeter, W. Leontief, F. Modigliani, A. Wald, T. Koopmans, P. Samuelson and K. Arrow were regular attendees (Cherrier 2010; Hildreth 1985, p. 8; Louca 2007, p. 44). In developing further his 'structural estimation method' with nearly a likeminded staff at the Cowles

[71] Their concern about mapping theory and data had been also called the 'identification problem' (Hoover 2006). Whether this already entailed theory to be absolutely prior to data was under dispute, but generally it was held that theory should inform the formulation of the relationships being tested, which presupposed the existence of a theory.

[72] Marschak completed his habilitation thesis entitled 'Elastizität der Nachfrage' (engl. elasticity of demand) in 1930.

Commission, Marschak, in line with Haavelmo (1944), placed an emphasis on probability theory and theories of behavior under uncertainty.[73]

Marschak understood economic relationships (the 'economic structure') to be fundamentally stochastic in character. Structural equations were used to describe those economic relationships. The formulation of the economic relationships that described the whole economy had to be in probabilistic terms, given random disturbances.[74] The formulation of the set of structural equations that would represent those relationships had to rely upon probability distributions. Thereby, one major role of theory was to inform the formulation of the relations in form of equations that would in turn be tested, for example concerning human behavior (of a specific group of people), technologies or legal rules. Economic predictions were, as such, fundamentally probabilistic.[75] Another important role was to estimate the parameters used in the model. The report states:

> The statistical estimation of the structural relations [- human behavior, technology, legal rules] is the '*filling of empty boxes of economic theory*'. The theory is a set of hypotheses. Most of these hypotheses state which variables enter which structural equations, or state certain inequalities […]. This is based, essentially, on experience independent of the material that is to be used in estimation. This experience may include statements on rational (i.e. utility-maximizing) behavior and on deviations from it; on a plausible psychology of anticipations; on technological data; etc.[76]

The research report for the years 1942-1946 envisaged that the Commission primarily aimed at improving the methods for numerical statistical estimation of structural relations and their changes. To ultimately predict effects of, first, alternative policies under non-controlled conditions and, second, because of the absence of experiments, the non-controlled conditions themselves, new tools would be necessary. Thus, one of the problems that Marschak was concerned with when von Neumann and Morgenstern published their axiomatic choice theory was how to find an improved theory of behavior under uncertainty that would allow for the formulation of structural relations about behavior and changes of structure. As the Commission's report stated:

> In postulating structural economic relations that have to be submitted to the statistical procedures of testing or estimation, one possibility is to accept, as a first approximation, the assumption of rational economic theory: that people behave so as to maximize their profits (or, more generally, their satisfaction) according to the best knowledge they have. When trying to make use of theory for econometric purposes, one soon discovers, however, that much clarification is required to give the

[73] At the Cowles Commission, Marschak hired Haavelmo, Koopmans, Arrow, L. Klein, E. Domar, D. Patinkin and H. Simon, L. Hurwicz, G. Kantona, H. Gregg Lewis and Oscar Lange among others (Hildreth 1985, p. 8).
[74] See 'Cowles Commission for Research in Economics, Five-Year Report, 1942-1946, The University of Chicago, folder 'Cowles Commission for Economic Research 1946-1947, box ?, RFA, p. 3.
[75] Ibid.
[76] Ibid., p. 5, italics in original.

> hypotheses a form amenable to statistical tests. In particular, behavior in conditions of uncertainty has only just begun to be analyzed by students of investment. What is the rational behavior for a firm that has only imperfect information, e.g., only a finite sample, from which to estimate the properties of its future product or of its future market? Or, if the firm does not use its imperfect information in the way that the rational statistician would advise it to follow, what are the most plausible psychological patterns according to which modern man's expectations of the future are based on current and past information? What behavior can be predicted when a seller faces only a small number of competitors and buyers? Finally: given a theory of behavior (rational or not) of single individuals in the markets for single commodities, what is the best method of combining these innumerable atomistic structural relations into a useful system of relations between national aggregate-total income, savings, profits, employment, price and wage level, interest rate, etc. Such a system must be both manageable and accurate enough to be applied in prediction and policy decisions. Inasmuch as construction of economic models and estimation of structural relations has so far proceeded without waiting for complete answers to fundamental problems of economic behavior, the work has been on a somewhat intuitive basis. To improve our theoretical postulates means revising basic economics.[77]

References to Hurwicz's and Marschak's reviews of the *Theory of Games* indicate that axiomatic choice theory was already one step ahead of traditional marginal utility theory and as such promised to become towards a solution to this problem.[78]

That the problem of theoretically grounding individual human behavior remained one ongoing concern beyond Marschak's directorship becomes readily apparent in a research plan of the Cowles Commission that Marschak had drafted and sent to the Rockefeller Foundation in 1947. Marschak placed the 'revision of economic fundamentals' at its core, i.e. implying

> continued thinking on hypotheses regarding human behavior in the field of economics. This part of activities includes the revision of hypotheses on the behavior of firms and households in conditions of uncertainty and of imperfect markets; the problem of 'aggregation' ('transition from micro-to macro-economics'). It will probably spill over into the no-man's land between economics and political science: the study of the behavior of legislators, administrators under the impact of economic conditions (thus relaxing the assumption used to far, which treated fiscal policy as an 'exogenous variable'.[79]

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[77] See 'Cowles Commission for Research in Economics, Five-Year Report, 1942-1946, The University of Chicago, folder 'Cowles Commission for Economic Research 1946-1947, box ? , RFA, p. 12.

[78] Questions for further study included "What are the optimal methods of aggregation- i.e. the most suitable techniques of forming averages ("index numbers') which would bring the theory of single forms and households to bear upon the political economy of a nation? How should rational economics of the behavior of single firms and households be developed or corrected to become realistic in conditions of uncertainty?"; ibid., p. 14.

[79] The other three elements of the research program were a) developing statistical tools for uses in economic research; b) constructing dynamic economic models of different degrees of completeness an detail; and c) attacking long-run problems, such as the effects of technological, demographic and institutional changes (Attachment of letter from Marschak to Willits, 'General Outline of Research Plans' 22 May 1947, folder 'Cowles Commission for Economic Research 1946-1947, box ?, RFA.

Furthermore, the long-term research agenda that Marschak envisaged for the Cowles Commission was already heavily focused on the development of applicable mathematical tools. In an additional memorandum to the Rockefeller Foundation, Marschak stated: "The main endeavor of the Cowles Commission is to formulate with logical precision theories of practical relevance, and to submit them to the test of facts and figures. The required econometric methods (mathematical and statistical techniques in the service of economics) are being worked out systematically".[80] Marschak considered the Cowles Commission as the "only institute in the world formulating economic theories precisely and submitting them to mathematical tests", which he held to be the task of good science.[81] Given the context of the 'measurement-without-theory'-debate and Koopmans' directorship, Marschak in fact had already laid the basis for a further shift of the Commission's work towards axiomatics in the sense of the second approach identified in section 2.1.[82]

During the late 1940s and early 1950s, axiomatic choice theory mainly traveled within the Commission and among other institutions, such as RAND, through the individuals that were involved in work at those research sites. More specifically, much of the development of axiomatic choice theory at Cowles took place rather isolated from the economics profession and was not representative for the work in economics more generally; the work at Cowles was exotic and difficult to follow. Already under Marschak's directorship, the work at the Cowles Commission was perceived as being highly abstract, relying heavily upon formal-mathematical tools. Furthermore, the problems identified by the Cowles Commission were not necessarily those shared by the economics profession as a whole. As example, consider that within the process of the Cowles Commission's application for a long-term grant from the Rockefeller Foundation, Joseph H. Willits, then the director of the Social Science Division, asked Marschak to make the highly formal work and results of the Cowles Commission "accessible to larger groups of economists" (Marschak to Willits, 26 May 1948).[83]

Without going into the detail of the rivalry and increasingly distant relationship between Cowles and the Chicago economics department (see Düppe and Weintraub 2012, p. 22; Weintraub and Mirowski 1994, p. 264), the way in which the problems and future course of the Commission was perceived during that time among for example Chicago economists

[80] See 'Memorandum to Rockefeller Foundation' 23 May 1947, folder 'Cowles Commission for Economic Research, 1946-1947', box ?, RFA.
[81] See 'Interview of Jacob Marschak by 'JHW' [Joseph Willits] of the Rockefeller Foundation, 21 January 1947, folder 'Cowles Commission for Economic Research 1946-1947, box ?, RFA.
[82] I do not mean to say that the axiomatic approach was necessary for the structural estimation method cultivated at Cowles. Axiomatics and expected utility theory, however, provided one way to theoretically ground behavior under uncertain prospects.
[83] See folder 'Cowles Commission for Economic Research, 1946-1947', box ?, RFA.

becomes apparent in a memorandum written by Milton Friedman upon request from Willits.[84] In this memorandum, Friedman identified 'two articles of faith' the Commission held: first, to "construct a comprehensive quantitative model for the economy as a whole from which it will be possible to predict with a reasonable degree of accuracy the future course of economic activity; second, the belief that this quantitative model should take the form of a system of simultaneous equations of a special kind (namely, stochastic difference equations)".[85] While Friedman did not reject the Commission's general goal (to formulate a "general theory of economic fluctuations"), he questioned whether the economics profession was at a stage at which this problem could seriously be addressed. With explicit qualification, Friedman stated: "I believe that we are not yet ready for the development of a general model, that it will take decades of careful monographic work in constructing foundations before we shall be ready to put up the kind of superstructure that the Cowles Commission hopes to create full blown".[86] As the 'measurement-without-theory' debate between Koopmans and R. Vining, defending the empiricist position of A. Burns and W. Mitchell at the NBER reflected, the method cultivated and problems addressed at Cowles were not to everyone's liking.[87]

Friedman's belief was reinforced by the scholars the Commission had hired – he did not consider them as working in economics but rather pursuing a highly abstract mathematical undertaking that might not really identify those problems of primary importance for the profession. It is worth quoting Friedman at length on this issue:

> The Cowles Commission staff itself has included able people who have an almost religious belief in the unique correctness of their approach. Koopmans is intelligent, careful, and scientifically minded. Kenneth Arrow is ingenious and well trained as a mathematical economist and statistician. Don Patinkin is one of the best of the recent crop of students at Chicago and is well trained as an economist, though he has had so little experience elsewhere as to be immature and hardly capable of a sound judgment about the value of the Cowles approach, Klein, a former member of the staff, is a highly original economic logician. Haavelmo, another former associate, is also highly original and some of his work is of considerable importance. People of the intellectual caliber of those listed in the preceding paragraph should be given every opportunity to do the kind of work they believe in. Nonetheless there are certain special characteristics of the group of people listed that leads me to retain considerable confidence that their experiment will fail. Almost without exception, the people listed are primarily mathematicians or statisticians rather than economists and have had no occasion to do careful scientific quantitative work on a limited segment of the economy. Koopmans, who strikes me as perhaps the ablest and most promising of the Cowles staff

[84] Willits had sought for advice based upon professional competence to make an informed decision about a grant application that Marschak had submitted at the Rockefeller Foundation.
[85] See 'Memorandum from M. Friedman to J.H. Willits, 26 May 1948, folder 'Cowles Commission for Economic Research, 1946-1947', box?, RFA.
[86] Ibid.
[87] See for example Weintraub and Mirowski (1994, p. 264), stressing the rivalries of the 1940s in which the Cowles Commission frequently played a role.

> and who probably plays a crucial role in determining the direction their work takes, has fundamentally a theoretical mind and inclination, and came to economics relatively late from mathematics and statistics. He has taught himself a great deal of economics, but his economics is not really part of him. He is likely to do good work in elaborating the mathematical implications of stated premises, in developing statistical techniques, and in expounding complicated mathematical and statistical techniques. But I have no great confidence in his judgment about realistic economic problems or about techniques for attaining sound knowledge of economic processes.[88]

Friedman concluded to "increase the value of the Cowles Commission experiment […] the Cowles Commission should be urged to provide translations of their basic work from their increasingly specialized jargon into a form in which it will be accessible to, and capable of judgment by, the great bulk of economists".[89] This clearly indicates the rather skeptical or even pessimistic attitude from economists outside the Cowles Commission.[90]

That Marschak's staff working in the directions indicated above consisted not of economists and econometricians but of mathematicians instead was a fundamental aspect of work on axiomatic choice theory that allowed for its multidimensional character. Furthermore, the Commission, as a kind of "hybrid institution between […] a university and a national laboratory" (Düppe and Weintraub 2012, p. 18), provided a creative environment where "[e]ach research associate is a creative thinker in his own right, exposed to criticism and suggestions of other members of this community of scholars, all working on closely related problems but approaching them with different techniques or from different backgrounds. Our tasks exceed the powers of a single person and require very close cooperation of minds, and coordination of their efforts".[91] This was the ideal environment in which a theory like axiomatic choice theory could be modified and developed further. It could travel around within the Commission among scholars working on different problems.

Decision-Making under Uncertainty and the Norms of Rationality

Another modification of rational choice theory that is reflected in the context of the Cowles Commission and the immediate work of Marschak is the interpretation and use of axiomatic choice theory as a normative account, prescribing the rational course of behavior in decision situations under risk and uncertainty. While with von Neumann and Morgenstern's work, axiomatic choice theory had initially raised hopes for the prospects of an empirically

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[88] See 'Memorandum from M. Friedman to J.H. Willits, 26 May 1948, folder 'Cowles Commission for Economic Research, 1946-1947', box?, RFA.
[89] Ibid.
[90] As Düppe and Weintraub (2012, p. 22) for example point out, Friedman was a particularly outspoken opponent of the "theoretical approach to data-mining".
[91] See 'Explanations to the proposed budget for research at the Cowles Commission' 22 May 1947, folder 'Cowles Commission for Economic Research, 1946-1947', box ?, RFA.

successful theory of human behavior, from the 1950s on, axiomatic choice theory became increasingly applied as a normative theory, a calculating tool that providing guidelines to individuals, firms and governments to optimal solution for a decision problem.[92] As such, axiomatic choice theory specified rules of conduct and thereby *shaped* rather than *explained* human behavior.[93]

The problem of individual and social decision-making under uncertainty thrived at the Cowles Commission in the 1950s under Koopmans' directorship. One example from the early 1950s is a project on theories of decision-making under uncertainty. Upon request of the Association of Casualty and Surety Companies, Koopmans considered the possibility of pursuing such a project and stated in a letter to Frank Lang, the manager of the research department of this association:

> It seems to us that basic research could be devoted to the problems of how best to meet uncertainty, both from the individual and from the social point of view. As an empirical counterpart of such a study of principles, it could then be investigated how actually individuals, business organisations, and government deal with important areas of uncertainty. That is, a well-rounded study should deal both with the norm of rational behavior, if such a norm exists, and with the empirical study of actual behavior.[94]

Around the same time, Koopmans asked Hurwicz to formulate a possible framing of such a project

> It could involve both the decisions of the individual under uncertainty (such as studied by Jascha) and a theory of best social decisions under uncertainty, as related to your topics. Uncertainty might be technological (weather, illness, accidents, or mechanical failure) or even uncertainty about decisions taken by others or by anybody in the future, and thus include the whole problem of business cycle explanation and policy.[95]

Such a study, according to Koopmans, would "frankly be theoretical in the first instance", not empirical (ibid.). Hurwicz was undoubtedly one of the major theoretical statisticians that contributed to this project. Other individuals involved in the project were K. Arrow, G. Debreu, R. Radner, and Erling Sverdrup.[96] Yet, that Hurwicz was rather unreliable with

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[92] Hands (2012) provides a preliminary version of this claim.

[93] This transition has given rational choice theory, and economics more generally, a 'normative turn', which suggests telling the history of rational choice theory as an account of how rational behavior has, over time, become reinterpreted as rule-following behavior. This observation is historically interesting in its own right, but has also fundamental implications for the assessment of rational choice theory as a theoretical framework. Yet, a more precise characterization of this normative turn is in order.

[94] Koopmans to Lang, May 8, 1950, folder 'uncertainty', box 156, JMP.

[95] Koopmans to Hurwicz, April 5, 1950, folder 'uncertainty', box 156, JMP.

[96] See http://cowles.econ.yale.edu/P/reports/1932-52.htm [accessed on March 2, 2013]. Kenneth Arrow had a considerable background in mathematics, mathematical statistics and mathematical economics. Leonid Hurwicz was primarily a mathematical statistician and economist. Roy Radner had an undergraduate education in mathematics and undertook his PhD in mathematical statistics at the University of Chicago, and Erling Sverdrup was a mathematical statistician. Other's who were working

respect to his engagements could have been one reason why ultimately Marschak became the principle investigator over the Cowles project on decision-making under uncertainty.[97]

By April 24, 1950, Marschak had set up a draft memorandum entitled "Economics of Uncertainty: A Research Project" in which he laid out the problems arising as the 'logical sequence of three steps':

> Step I: How should rational people – especially business firms - make decisions in the face of uncertainties: i.e. what is a firm's best decision, given its goal, its resources, and the uncertain information at its disposal?; Step II: How do people actually respond to available uncertain information in making decisions?; Step III: How can one utilize the knowledge about actual responses of people to uncertainties – e.g., the actual behavior of the customers and competitors of a firm, or the actual behavior of citizens of a nation - to achieve the decisions that are best from the point of view of the firm, or of the nation".[98]

Marschak himself noted in his report that Steps I and III are normative and result in "efficient methods for business firms and governments".[99] Step I would provide a benchmark for Step II to measure the irrational deviations observable in reality. Step II would be descriptive in nature: "its purpose is to observe the most frequent habits of behavior of various decision-makers (including business firms and governments), habits that are possibly obsolete, inefficient, irrational".[100] This empirical information about "typical behavior of modern people under conditions of uncertainty" would then be used in Step III to prescribe the 'best' course of action from the actor's point of view.[101] The theoretical characterization of individual behavior (by e.g. an axiomatic choice theory) in Step I was justified as point of departure:

> An 'ideal' businessman who uses modern statistical methods in controlling the quality as well as the forecasting the market of his product, and who acts upon this information in the most intelligent fashion may not exist – just as the perfectly healthy individual is a mere ideal of medical science which uses it to describe the deviations in the functioning of organs.[102]

The project "Decision-Making under Uncertainty" was ultimately supported by a grant of the U.S. Office of Naval Research in July 1951.

The project on decision-making under uncertainty reflected some of the more general changes that took place with Koopmans succeeding Marschak as the director of the

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on axiomatics more generally at the time at Cowles were Israel N. Herstein, a mathematician, partly collaborating with the Princeton mathematician John Milnor on a paper entitled 'An Axiomatic Approach to Measurable Utility' (1953).

[97] See http://cowles.econ.yale.edu/archive/people/directors/marschak.htm [accessed on March 2, 2013].

[98] See 'Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak', 24 April, 1950, folder 'uncertainty, box 156, JMP; p. 1 f.

[99] Ibid.

[100] Ibid., p. 2 f.

[101] Ibid., p. 2 ff.

[102] Ibid., p. 2 f.

Commission in 1948. Most importantly, Koopmans initiated a shift in focus towards more theoretical work and the preceding steps of model-building and empirical testing.[103] Furthermore, as Marschak had already announced, a stronger emphasis was also placed upon choosing the appropriate mathematical tools – again, Cowles at the time was the heartland of mathematical economics. And while the application of mathematical models was doubtlessly an important aim, it was not the priority of research at Cowles in the 1950s, an aspect that was particularly reflected in the motto 'Theory and Measurement', which Koopmans adopted for the Commission in 1951. Finally, while axiomatic choice theory had not occupied a role in the front seat so far, correspondence between individuals like Marschak, Arrow, Hurwicz, Baumol and Koopmans reveals that talking of 'rationality', 'rational choice' and 'rational behavior' became a commonly accepted language.[104] Rational choice theory, at least as a label, had thus become established at Cowles around this time to denote axiomatic choice theory.

From 1948 on, the core of the general research program of the Commission became subsumed under the headings of "actual behavior" and "rational behavior".[105] The former was directed at empirically observable behavior, to describing behavior that could actually be observed and of finding the general laws it was governed by in specific situations; the latter became understood as optimizing behavior, that is the behavior constituting the optimal course of action for an individual or a group to realize a given particular goal.[106]

> The study of rational behavior is the attempt to discover what kind of behavior on the part of individual or group in specified circumstances would most completely achieve the goals pursued; it presupposes that the goals are known and stated in objective terms, and that their probable achievement or lack of achievement as a result of following a particular pattern of behavior can be discovered. Studies of these two types may be called 'descriptive studies' and 'prescriptive studies', respectively.[107]

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[103] See e.g. Hoover (2006, p. 240 f.), pointing out the use of a priori economic theory as the source for identifying the relevant causal factors in economic structures in the late 1940s and early 1950s.

[104] The terms 'rational choice' or 'rational choice theory' were not part of the common vocabulary and became increasingly prominent only from the 1950s on, mainly parallel to the work of Gary Becker at Chicago, with whom the term 'rational choice theory' is closely associated. For example, a JSTOR full-text search of articles in the mainstream American economic journals *American Economic Review* (1911-2009), *Economica* (1921-2006), *Econometrica* (1933-2009) returns 11 hits for the phrase 'rational choice theory', of which the oldest is Leonid Hurwicz's and M. K. Richter's article *Ville Axioms and Consumer Behavior* published in *Econometrica* in 1979. Including the journals *The Journal of Economic Perspectives* (1987-2009) and *The Journal of Economic Literature* (1969-2009), which are a little bit more flexible with respect to the content they publish, leads 36 hits of articles published, again, between 1979 and 2009. This rather surprising result reveals the underrepresentation of this terminology in some of the most popular journals of mainstream economics (Halkos and Tzeremes 2012).

[105] On the Commission's website, the additional topics 3) *statistical methods* that included the decision function approach to finding the best estimates of parameters in econometric models, 4) *mathematical tools*, and 5) *special studies* can be found; see Cowles Commission website: http://cowles.econ.yale.edu/P/reports/1932-52.htm#4. [accessed on February 25, 2013].

[106] Ibid.

[107] Ibid.

Furthermore, acknowledging the fact that even rational behavior is undertaken in a world of actual human behavior that could - but does not have to - be rational, descriptive theories of human behavior would fulfill the crucial function in ultimately prescribing the rational course of behavior by building upon how other people behave. As indicated in Marschak's proposal, the Cowles website stated:

> There is a good deal of overlapping between the descriptive and the prescriptive studies for the following reasons. First, in setting up models of actual behavior in a world where monetary and material matters are of great importance, it is convenient and is often a good approximation to reality to assume, as a basis for such models, that individuals and firms do behave rationally. Thus, the assumption of rationality enters into many theories of actual behavior. Second, in order to prescribe what one individual or group should do in order to achieve his or its goals, the economic doctor must know how *other* individuals and/or groups will behave in the future, and in particular how they will respond to the actions of his patient. This requires knowledge about the actual behavior of others, whether it is rational or not. Thus prescriptive studies draw on the results of descriptive studies. Because of this two-way overlapping the distinction must be regarded as an expository device, and it must be remembered that an accepted description or prescription may become inapt if either the prescriptions or descriptions upon which it is based turn out to be incorrect.[108]

The project of decision behavior under uncertainty matched perfectly with the goal of the Commission and bridged the gap between the two dimensions. Axiomatic choice theory provided a theoretical grounding for the formulation of hypotheses and the estimation of axiomatically defined concepts such as utility. As Marschak puts it in his memorandum:

> The empirical worker may wander aimlessly in a chaos of trivial, so-called 'findings' if he is not provided with well-thought out theoretical hypotheses to be tested, and well defined quantities to be measured; at the same time, the empirical worker keeps his theorizing colleague from flying up into the clouds.[109]

While the three-step procedure included an empirical dimension, requiring the investigation of human beings' psychology, actually conducting empirical research was not of primary concern at Cowles. This is indicated by the fact that, while Marschak had mostly deferred responsibility for experimental studies to e.g. the Institute of Social Relations at Harvard, the Survey Research Center at the University of Michigan and to the University of Illinois and had effectively largely outsourced the empirical 'step' II to institutions and researchers outside of the Commission.[110] And to study deviations from the rational course of

[108] Ibid.
[109] See 'Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak', 24 April, 1950, folder 'uncertainty, box 156, JMP; p. 3.
[110] Behavior that became tested by researchers at the Commission was for example inventory behavior of forms, import demand, financial behavior patterns, demand elasticities for imports, etc.

action, Marschak suggested analyses of balance sheets of enterprises including insurance companies as well as price data, whereby deviations from the rational (prescribed) course of behavior (from step I) "would probably, on the average, characterise the less successful enterprises, or the small stockholder or speculator".[111] The empirical insights gained could then be "applied to develop rational techniques for firms or governments, in ways depending on the nature of the firms concerned and on the national goal of the government".[112] Thus, not only the final purpose of empirical investigation was normative, providing a framework that would direct a person towards the rational course of action. Also, the empirical was frequently outsourced and not mainly undertaken by researchers at the Cowles Commission, which fostered its theoretical orientation.

Yet, transdisciplinarity in approaching decision-making was an aspect that was stressed by Marschak. In his draft, Marschak points to the need to approach the "subject from different angles" through cooperation and the "interchange of ideas" with the aim of

> working out of general techniques for rational decisions based on uncertain information about nature and society; the empirical observation of actual, possibly irrational responses of individuals making up the modern society; and the study of rational techniques for specific business and government decisions that would utilize the empirical knowledge just mentioned.[113]

This transdisciplinarity was also characteristic for other research sites were scholars worked with axiomatic choice theory, such as at the RAND Corporation. It was a prerequisite for axiomatic choice theory to travel between interdisciplinary working arrangements that enabled excellent scholars to work on similar questions, but approaching them from distinct perspectives was a necessary prerequisite. Axiomatic choice theory could travel between Cowles and other research sites, such as RAND, where creativity and excellence were equally practiced by those very people, who were travelling themselves back and forth between those institutions (Marschak himself became a consultant at RAND in 1950). It traveled among a network of scholars to different research sites.[114]

4. Marschak Caught between Logic of Choice and Behavioral Sciences

Axiomatic choice theory became modified and transformed not only by disagreement about its substantial ingredients. Its change was also part of a larger modification process that

[111] See 'Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak', 24 April, 1950, folder 'uncertainty, box 156, JMP; p. 6 f.
[112] Ibid., p. 7.
[113] Ibid., p. 3.
[114] For a historical account of the development of 'rational choice theory' at the RAND Corporation, see Amadae (2003).

took place within an institution such as Cowles, be that with respect to a research strategy or through a changing incentive structure that had an impact on the problems that researcher began to consider important. At the Cowles Commission for example the normative shift that became revealed in projects such as decision-making under uncertainty was paralleled by the American behavioral science movement. While I do not want to speculate here on the impact of this movement on the general course of the Cowles Commission, I hypothesize that it had an impact on Marschak and how he perceived the role and status of axiomatic choice theory. It led him to new research sites such as the 'Center of Advanced Study in the Behavioral Sciences', taking axiomatic choice theory with him. I argue that the shift from economics to the behavioral sciences, which has pointed out for example by Cherrier (2010), ultimately resulted from the interaction of Marschak with the new research area, while Marschak's conception of good science still remained in line with the general course of the Cowles Commission. Marschak's initial resistance to, and his slow shift in the direction of, the behavioral sciences becomes revealed in Marschak's interaction with the Ford Foundation, one of the most important supporters of the advancement of behavioral sciences (Pooley and Solovey 2010, p. 200).

Adapting the Cowles Research Program to Fit the Behavioral Sciences

While the Cowles Commission had become the heartland for mathematical economists, the behavioral science movement was already well under way in the beginnings of the 1950s (e.g. Pooley and Solovey 2010). In 1951 (-1957), the Ford Foundation established what became known as the 'Behavioral Science Program' in Pasadena (California), which had as one of its major goals to establish and promote what had been labeled 'research in the behavioral sciences' in the United States (e.g. Gaither et al. 1949, p. 94). This division, called 'Program Area V', was committed to issues of 'Individual Behavior and Human Relations', and focused on supporting *research* in this field and not on developing some sort of *action plan* in order to guide the behavioral sciences into a specific direction (Berelson 1968). The aim was to move "a set of academic disciplines in a new direction" (Hauptmann 2012, p. 164).[115] More specifically, its goal was "to increase knowledge of human behavior through basic scientific research orientated to major problem areas covering a wide range of subjects, and to make such knowledge available for utilization in the conduct of human affairs" (Berelson 1968, p. 42).

[115] Program Area V was one of five program areas that came out of an extensive process of establishing the guidelines and further activities of the Ford Foundation in line with its more general objectives of devoting extensive financial resources into promoting human welfare (Berelson 1968, p. 42); other Program Areas were Area I "The Establishment of Peace", Area II "The Strengthening of Democracy", Area III "The Strengthening of the Economy", and Area IV "Education in a Democratic Society" (see Gaither et al. 1949).

The study of actual individual behavior began to play a fundamental role in the Ford Foundation's mission for Program Area V. As the Foundation committed to the idea that the study of human conduct would produce knowledge fruitful and necessary for improving the welfare of individuals and of society, one of the major objectives of Program Area V was to scientifically study the factors of influence and determination of human behavior, the values, individual "beliefs, needs, emotional attitudes and other motivating forces; the origins, interactions, and consequences of such values; and the methods by which this knowledge may be used by the individual for insight and rational conduct" (Gaither et al. 1949, p. 90 ff.). More specifically, the goal was to investigate into and elicit the fundamental and general principles that govern human conduct in various situations – personal or social – which an individual can find itself in (Gaither et al. 1949, p. 90 ff.), and to thereby arrive at a "scientific understanding of why people behave as they do" (Berelson 1968, p. 42). Those principles should be approached in an interdisciplinary fashion and subsequently become scientifically formulated, empirically tested and ideally verified through a commitment to the scientific method (Berelson 1968).The ultimate aim was thus to "support scientific activities designed to increase the knowledge of factors which influence or determine human conduct, and to extend such knowledge for the maximum benefit of individuals and of society" (Gaither et al. 1949, p. 90).

The division was led by Bernard Berelson, a former professor of library science who had earned his PhD at Chicago in 1941 and was later remembered as having been the main person responsible for institutionalizing the label and concept of 'behavioral sciences' in the United States (e.g. Sills 1981). The term 'behavioral sciences' entered academia only in the 1950s, a process that had been initiated in 1948 by H. Rowan Gaither. Gaither, a prominent attorney and co-founder of the RAND Corporation in 1948 who became president of the Ford Foundation in 1953 (until 1956),[116] had assumed the Ford Foundation's Study Committee to determine and advise the Board on mission and structure of the foundation (Pooley and Solovey 2010).[117] Gaither and his study team had identified the social sciences as one major area of interest and decided on the term 'behavioral sciences' to label a program that aimed at promoting the social sciences (ibid., p. 202 ff.).[118] After Gaither et al. had introduced the term in their study report for Program Area V (Gaither et al. 1949, p. 94), the Ford Foundation, through the persona of Berelson, aimed not only at reviving, but also at filling it with actual content (Berelson 1968; Sills 1981). In a first step, this meant supporting the production of knowledge about human nature, which, in a second step, would be made fruitful to serve

[116] See RAND homepage http://www.rand.org/about/history.html [accessed March 26, 2013].
[117] See Ford Foundation homepage http://www.fordfoundation.org/about-us/history/presidents [accessed March 26, 2013].
[118] The term 'social science' was to be avoided because of its frequent confusion with 'socialism', a term which one would not be related to in the American of the 1950s (Hauptmann 2012, p. 164).

human needs and address social problems (Berelson 1968, p. 42). For that purpose, the concept of 'behavioral sciences' became transformed by the Foundation: "an administrative arrangement became intellectually institutionalized" (Berelson 1968, p. 43). And economics was supposed to be a part of that process.

The 'Behavioral Science Program' reflected a commitment to interdisciplinarity and to the use of mathematics to rigorously investigate into human decision-making. Furthermore, as the final report of the study group showed (Gaither et al. 1949), both Program Areas, the economics/business division (Area III) and the Behavioral Sciences Program (Area V), were largely influenced by an empiricist approach to the social sciences.[119] Berelson, who would later become professor of behavioral science at the Graduate School of Business of the University of Chicago, was fundamentally convinced of behavioral empiricism, a position strongly reflected in the direction the 'Behavioral Science Program' would go in the near future.[120] While the report laid a heavy emphasis on 'economic behavior', it showed "the group's focus on empirical research into actual behavior, in explicit contrast with neoclassical economists' assumption of rationality" (Pooley and Solovey 2010, p. 223). Furthermore, the ultimate focus on empirical science strongly opposed the theoretical direction that Koopmans had followed with the Cowles Commission and to the Bourbakian kind of axiomatics that was practiced at Cowles by people like Debreu (Weintraub and Mirowski 1994).[121] Yet, Koopmans and Marschak at the Cowles Commission nevertheless planned to apply for a grant at Program Area V and correspondence began in 1951.[122]

That Koopmans had approached the Foundation through the 'Behavioral Science Program' and not through Program Area III, the economics/business division, was most likely on suggestion of Berelson, Gaither and Hans Speier, the former head of RAND's Social Science Division who had become a member of the Study Committee.[123] Berelson and

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[119] Neither 'behavioral empiricism' nor 'behavioral sciences' as Berelson and co. understood the term are to be confused with either behaviorism, denoting a specific school in psychology, or, more generally, with the idea to create a unified discipline 'behavioral science' (see also Berelson 1968).

[120] That Berelson strongly favored the "General Scientific Approach" to behavior becomes for example reflected in a shortened version of his *Human Behavior* originally written together with Gary A. Steiner, in which Berelson stressed his naturalist position that the scientific method, as in other sciences, would be the most appropriate method for behavioral sciences (Berelson and Steiner 1967, p. 6 ff.).

[121] That mathematical economics of the Bourbakian kind was not generally popular at that time throughout the economics profession becomes indicated for example by the fact that Debreu did not get tenure at Yale University in the early 1950s (e.g. Düppe 2012, p. 429).

[122] One important reason for why the Cowles Commission applied for a grant of the Ford Foundation was to replace "the 'military' contracts (RAND and ONR) by 'peaceful' ones; see 'Report of Meeting with H. Speier by Marschak to Koopmans and W.B. Simpson, 21 August 1951, folder 'Ford Foundation, box 99, JMP; p. 4.

[123] Koopmans to Marschak, 26 July 1951, folder 'The Ford Foundation', box 99, JMP. That Speier suggested how little the 'Economics Section' is prepared "for action". Furthermore, the section might not be interested in theory and as such might be unsympathetic to the work at Cowles, as Bissell – head

Gaither considered basic "research of mathematical type" favorably, they aimed at fostering the integration of the social sciences that "made possible precisely by the mathematical (logical) approach", they admired the international orientation of the Commission and placed focus on the cooperation of the scientists at Cowles who would become leaders in the field of mathematical economics.[124] Those aspects were appreciated at their Program Area V.

The purpose statement of the grant application registered in the Ford Foundation archives reads as follows: "Study of social organizations by formal logical and mathematical methods and by support of individual and team work by mathematicians, social and economic theorists, and statisticians; organizations of research conferences on national and international scale; and the provisions of fellowships for young social science students".[125] In Koopmans own words this meant: "support is sought for the Commission's basic research in economic and social behavior, and for a program of development of human resources in the logical and mathematical approach to the social sciences".[126] While this statement appeared to be in line with the general course of the Commission, going back and forth on their strategy for approaching the Ford Foundation, the correspondence between Marschak and Koopmans suggests that they had to modify the aims and activities of the Cowles Commission considerably to make the work of their work fit into the mission purpose of Area V of the Ford Foundation. For example, a letter from Koopmans to Marschak, dated July 26, 1951, shows their need for an adjustment strategy:

> Our feeling is that, while we should fully mention and make a point of the instances in which our work has become broader than economics or spilled over into related Social Sciences, we should not attempt to disguise the fact that the present personnel consists preponderantly of economists. I would suggest the problem of decentralization of decisions in the organization of society as a good example of a broad theme which we expect to approach from various sides and in various forms. It also illustrates the futility of classifying our work in 'economics.' While the theory of prices is traditionally the core of economics, prices as a device for decentralization of decisions are just as much a topic in organization theory.[127]

Equally, in a report dated August 21, 1951 Marschak collected his impressions for Koopmans from a conversation with Speier about their application.

> I explained to Speier that our present staff does not include social scientists other than economists. <u>But I did not press this point.</u> I think the application should be worded in a way emphasizing the 'integration'

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of the economics section – does not care "for theory at all" according to Marschak (Marschak to Koopmans and Simpson, 21 August 1951, folder 'Ford Foundation', box 99, JMP.

[124] Marschak to Koopmans and Simpson, 21 August 1951, folder 'Ford Foundation', box 99, JMP. W.B. Simpson was the Executive Director of the Cowles Commission during that time.

[125] Rockefeller Archive Center, Ford Foundation records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324).

[126] Koopmans to Gaither, 14 September 1951, folder 'The Ford Foundation', box 99, JMP.

[127] Koopmans to Marschak, 26 July 1951, folder 'The Ford Foundation, box 99, JMP.

> aspect, with economics interpreted as 'science of optimizing' (like the Theory of Games and Economic Behavior') in a broad sense. On the other hand, while trying to 'integrate' the subject-matter, we need not 'integrate' the personnel, i.e. we are not compelled to employ sociologists or psychologists (unless they are very good, of the Herbert Simon sort) and can continue to get along with economists, mathematicians or any other bright and imaginative people.[128]

That Speier's concerns about taking empirical work and psychology seriously were potentially decisive for the application brought Marschak to reflect on how the abstract work being conducted at Cowles could be linked to a potential interest in empirical interdisciplinary work of interest for the Ford Foundation. Besides remarking how useful it had been to have read Simon's 'Some Strategic Considerations in the Construction of Social Science Models' before the meeting (as "[i]t contains good examples of translation of socio-psychological hypotheses (including economic ones) into mathematical language"), Marschak highlighted what this prioritizing of empirical work meant for favorably presenting the scope of economics and the methodological status of the logic of rational choice:

> In the light of my talk with Speier, it seems to me that we have to emphasize that our interests cover the whole field of behavior, of different degrees of 'rationality' (or 'economicality'), but with high rationality as a point of departure; and that our personnel will include any people capable of mathematical and logical clarity and imagination.[129]

It becomes equally clear however that at that time, it was probably no more than Marschak paying lip service to Speier:

> If, in appointing a staff member, a joint appointment with another department of the University proves necessary, we may well extend our practice (limited so far to Departments of Economics and Mathematics, and the Committee of Statistics) to include in the first place, divisional appointments in Social Science. They are symbolical of 'integration' so dear to the heart of administrators of Universities and Foundations. In addition, appointments jointly with psychology might be considered, provided they do not force upon us more participation in empirical work than we are, at that time, willing to perform. (I should not, however, express too strongly our 'anti-empirical' bias – on the contrary, I should emphasize 1) our past record in the empirical study of aggregative behavior equations, and our interest in economic cross-section studies, 2) our willingness to be consulted on the models (hypotheses) and on the design of questionnaires and of experiments in behavior.[130]

The need of the Cowles Foundation to align their research proposal with the Ford Foundation's more general goal to support the establish the behavioral sciences becomes even clearer in a draft of the application dated September 1951. Marschak and Co. stressed the

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[128] See 'Report of Meeting with H. Speier by Marschak to Koopmans and W.B. Simpson, 21 August 1951, folder 'The Ford Foundation, box 99, JMP; p. 2, italics in original.
[129] Ibid.
[130] Ibid, p. 3.

"vanishing boundaries between economics and other social sciences", the fruitfulness of applying the "mathematical approach to social science", the importance of the theoretical and empirical study of human behavior, the need for interdisciplinary work, and the definition of economics as "a science of best choice from a given range of alternatives in the light of clearly defined values".[131] They promised that "this support [by the Ford Foundation] be used for further development and strengthening of the Commission's research on economic behavior and social organisation, thus eliciting the best contribution it can make towards the emergence of an integrated social science".[132]

The focus of the grant proposal was placed on the importance of studying 'economic behavior'. Koopmans and Marschak justified the turn towards studying individual behavior in an interdisciplinary fashion by referring to the aforementioned normative interpretation of axiomatic choice theory. By distinguishing normative and realistic approaches to behavior, the proposal listed for example Kenneth Arrow's dissertation, John von Neumann and Oskar Morgenstern's game theory, John Nash's work on bargaining, and Norbert Wiener on cybernetics as desirable attempts for applying mathematical methods in the social sciences that, at the same time, introduced rigor, method and clarity into the social sciences such that they could hold up to the high standards of the physical sciences.[133] Referring to examples such as Arrow's *Social Choice and Individual Values* as well as to more recent work on activity analysis (e.g. by Koopmans "Activity Analysis of Production and Allocation"), both being in the first instance of normative character, Koopmans and Marschak stressed that activity analysis provided simple descriptions of technologies available that could serve as basis for a "normative discussion of efficient allocation in a social sense" and allowed for developing "[r]ules of action to achieve efficiency in a society with decentralized decision-making".[134] That problem, which Koopmans and Marschak considered being the "central problem of our time",[135] made a shift towards actual individual decision making now even more important. "To obtain a realistic theory of organization, it will be necessary to give expression to habitual responses, attitudes, loyalties and antagonisms in organizations by formal hypotheses about individual behavior" and develop models of technologies available to the individual.[136] Furthermore, appraisals of social scientific models would be desirable

[131] As the preserved index card tracking the application process shows, the application was ultimately submitted on September 14, 1951; see Rockefeller Archive Center, Ford Foundation records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324).
[132] See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics' September 1951, folder 'The Ford Foundation', box 99, JMP; p. 13.
[133] See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics' September 1951, folder 'The Ford Foundation', box 99, JMP; p. 5.
[134] Ibid, p. 6.
[135] Ibid, p. 14.
[136] Ibid, p. 15.

"with a view to defining and measuring parameters describing individual behavior, attitudes, abilities, responses".[137]

That this characterization implied a profound shift for the Cowles Commission's original focus more towards the objectives of the Ford Foundation appears obvious. For example, while the proposal refers to von Neumann and Morgenstern's theory of games, this reference is less enthusiastic than in Marschak's review of the 1940s, stressing how its realism could be improved by formalizing the limitations of intellectual and information-handling processes of participants,[138] a dimension of research that would require psychological justification. Furthermore, while one proclaimed focus of the Commission's research program included the empirical study of actual economic behavior,[139] the proposed approach to studying individual behavior at Cowles differed profoundly from other 'empirical sciences' interested in the nature of behavior, such as psychology, neurology, anthropology, etc. More specifically, the Commission did not question the use of axiomatics as a basis for formulating a theory of human behavior in Marschak's Step I. Instead, as mentioned above, an important role of empirical findings about how people actually behaved was to secure the effectiveness of policy recommendations that were to be developed around a normative account of rational choice in Step III.

Another motivation expressed in the application, which they took to be shared with "many other empirical sciences", was the "curiosity that desires to explore and understand the physical, technological, human, and social environment in which life is lived".[140] Thereby, however, 'empirical' did not only mean to investigate human motivation and individual behavior on the basis of questionnaires, experiments, and interviews,[141] but primarily involved "theoretical studies of the implications of assumptions made for purposes of empirical analysis".[142] Instances of such studies were L. Klein's work on constructing systems of aggregative behavior equations, work on disaggregation through the study of individual markets or industrial classes, and work on cross section data collected by sample surveys and focusing on specific types of decisions and expectations (e.g. consumer choice).[143] The latter

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[137] Ibid, p. 16.
[138] Ibid, p. 15.
[139] See "Economic Theory and Measurement – A Twenty Year Research Report, 1932-1952": Research Activities, July 1, 1951-June 30, 1952; see: http://cowles.econ.yale.edu/P/reports/1932-52-b.htm [accessed 10th of April, 2013].
[140] Ibid.
[141] For example, Koopmans points out that by 'economic theory', upon which systems of structural equations rely, is meant a combination of a) principles of economic behavior derived from general observation (introspection, interview, experience) of human motivations; b) knowledge or legal and institutional rules; c) technological knowledge; d) careful construction of the definition of variables (Koopmans 1949, p. 125).
[142] Ibid.
[143] Ibid. Examples for markets that became analyzed were demand and supply for livestock products (C. Hildreth), markets with forward trading such as corn, wheat and cotton market to study speculation

for example allowed for shedding light on the empirical background of theoretical frameworks such as axiomatic choice theory for behavior under uncertainty and accordingly allowed for their potential modification.

Besides stressing the importance of realism and empirical research for the Ford proposal, Marschak adopted a Robbins-like view of the scope of economics in the proposal that allowed him to present the work of the Commission as fundamentally transdisciplinary.

> If economics is defined as the science of best choice from a given range of alternatives in the light of clearly defined values, the same definition and the same formal structure of theory applied to much of social science generally, with other classes of alternatives and wider evaluation and objectives. In the light of this formal similarity, the boundaries between the various social sciences become vague and unimportant.[144]

Because of their concern with the general problem of decision-making, the Commission's research program was unintentionally interdisciplinary according to Marschak: "without a systematic intention to that effect, the Cowles Commission studies […] have brought us in a situation where we no longer know whether we are working in economics, in organization theory, in sociology, or just in social science generally".[145] Thereby, "[t]he contribution of economic theory is the formal treatment of optimization problems, while the substance of the models may be drawn from the entire range of social sciences".[146] Ultimately, the direction of the Commission should shift further "towards greater generality and relevance to a wider class of social phenomena" and the topics under study, while not to integrate into a single research program, "aim at a better understanding of society as a prerequisite for its ultimate improvement".[147] He stressed the Commission's ultimate goal of making those methods and models applicable to contribute to knowledge of society and make them practically useful.[148] On the whole this portrait of the Cowles Commission given in the grant proposal was fundamentally different from the course Marschak and Co. had taken until then. The scale of the proposal – from the start, the application was for a long-term grant ($250.000 in a period

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(H.S. Houthakker), among others. The analysis of input-output ratios stimulated by W. Leontief was also part of the field of empirical research at Cowles. Those studies, however, were frequently not conducted directly at the Commission, but in collaboration with affiliated scholars that were appointed at other academic centers (e.g. Modigliani at the University of Illinois undertook a project on expectations and business fluctuations); see "Economic Theory and Measurement – A Twenty Year Research Report, 1932-1952": research activities, July 1, 1951-June 30, 1952; See: http://cowles.econ.yale.edu/P/reports/1932-52-b.htm [accessed 10th of April, 2013].

[144] See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics', September 1951, folder 'The Ford Foundation', box 99, JMP; p. 7.

[145] Ibid.

[146] Ibid, p. 8.

[147] Ibid, p. 14.

[148] Ibid, p. 7.

of 10 years) - indicates that Koopmans and Marschak were quite serious about their statements.[149]

The adjustment of the Cowles Commission's research program had far-reaching implications for the nature and status of axiomatic choice theory in economics and also on how it was taken up by other disciplines. The summary of the grant application clearly reveals the attempt of Koopmans and co. to focus research at the Commission on the study of human behavior, to elicit their interdisciplinary approach and to promote economics as a behavioral science:

> In recent years rapid progress has been made in the construction of mathematical models to guide the theoretical and empirical study of human behavior. The Cowles Commission for Research in Economics has during the past ten years participated in this development by pioneering studies, both realistic and normative, of economic behavior. In realistic studies of economic growth and fluctuations, its principle has been to account for the movements of economic aggregates in terms of plausible hypotheses regarding behavior of individuals. In normative studies, it has distinguished and analyzed objectives or values (social or individual), the opportunities presented by technology and by natural and human resources, and the rules of action by individuals that best serve these objectives within these opportunities.[150]

That this turnover was not yet fully credible on the part of the Cowles Commission might have been one reason why the grant was ultimately not approved. After several delays, the application was finally rejected on January 14th, 1954. The official justification was that too many demands on the Foundation's resources limited the range of the program.[151] [152]

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[149] Rockefeller Archive Center, Ford Foundation Records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324).
[150] Ibid, p. 1.
[151] Rockefeller Archive Center, Ford Foundation Records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324). I could not obtain more information about the final grant rejection so far, because in the 1960s the Ford Foundation destroyed all correspondence and additional materials on grants that had been rejected and only preserved application papers of successful grants.
[152] Marschak to Koopmans, 18 August 1952, folder 'The Ford Foundation', box 99, JMP. Other reasons that Berelson for example had provided in one and the same conversation for the delay of processing the application were that the area of the application, i.e. 'Mathematical methods in Social Science' was very general; that because it laid at the borderline of economics and behavioral sciences, the activities would over the time possibly overlap with the new economic activities division that was about to be set up under the lead of Carroll. Berelson denied that the Commission's location at the University of Chicago would be an obstacle; finally, Berelson mentioned that because of the application for long-term support, which would have the character of an endowment, the policy for supporting institutions in that way rather than projects required first a decision on the highest level of the Ford Foundation. See 'Report (by Koopmans?) on Conversation with Berelson, New York office of Ford Foundation', 20 October 1952, folder 'The Ford Foundation', box 99, JMP, p. 2; see also 'Report of Conversation between Berelson and Clifford Hildreth' 17 February, 1953, folder 'unlabeled', box 99, JMP.

Economics as a (Mathematical) Behavioral Science

In their analysis of the Ford Foundation's efforts to foster cooperation between the behavioral sciences and economics and why they failed, Pooley and Solovey (2010, p. 201) argue that (neoclassical) economists were "uninterested, skeptical, and even dismissive of what they took to be an immature and faddish initiative". They conclude that "[e]conomists' critical reply to the [Behavioral Science Program of the Ford Foundation] overture reflected and reinforced the growing postwar hegemony of the neoclassical approach" (ibid, p. 230). While the history between economics and the behavioral science movement appears more complicated than their narrative suggests (see Cherrier 2013), what can be observed is that Marschak's interests drifted from the economics profession towards the behavioral sciences, and in this transition he was taking axiomatic choice theory with him. The increasing intensity of interactions between Marschak and the representatives of the Ford Foundation reveals the process behind this shift.

During the process of the grant application to the Ford Foundation, Marschak had become more strongly involved in the Foundation's procedures to support the behavioral sciences in American academia. In May 1952, Gaither and Berelson had founded the 'Advisory Group on Economics and the Behavioral Sciences' to discuss "the Foundation's general objective for attempting to establish more fruitful relationships between economics and behavioral sciences in order to expand and deepen the intellectual maturity of both fields and to solve important public problems".[153] The group had committed to a broad definition of 'behavioral sciences' as "[a]ll those intellectual activities which contribute to the scientific understanding of human behavior, such as social and clinical psychology, cultural anthropology, sociology and parts of political science, economics and business, law, history, philosophy, biology and certain humanistic fields".[154] Gaither and Berelson clearly considered economics to be a behavioral science. And already in July 1952, Thomas Carroll, cousin of Gaither and head of Project Area III (the economics/business division), contacted Marschak requesting support in identifying ways for potential cooperation between the two fields.[155]

The group envisaged fostering a mutually beneficial relationship between the two areas, mainly with focus on their approaches to human behavior. In an attached memorandum of a first meeting in May 1952, the advisory group had considered a cooperation as "highly desirable" because it would prove "valuable both in the development of new theories about

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[153] Carroll to Marschak, 16 July 1952, folder 'unlabeled', box 99, JMP.
[154] Ibid.
[155] Individuals that were part of the group were Thomas Carroll, Carroll Gaugherty (School of Commerce, Northwestern University), James Duesenberry (Economics Department, Harvard University), Fritz Roethlisberger (Graduate School of Business Administration, Harvard University), Herbert Simon (Department of Industrial Management, Carnegie Institute of Technology, Joseph Spengler (Department of Economics, Duke University. Representatives of the Ford Foundation were Berelson, Richard Bissell and David McClelland.

economic behavior and in 'mopping up' operations designed to document and refine earlier theories […] [I]t would [also] be valuable in providing new data and new techniques for the behavioral sciences".[156] The advisory group traced the separation between economics and the other behavioral sciences back to a highly specialized discipline of economics concerned with a set of particular economic problems that were "defined in limited terms for purposes of simplicity and manageability. [Economists] invented their own psychology and sociology, as needed", which were subsequently accepted and institutionalized in university departments and as such finalized in what became traditional economics.[157] Furthermore, the group identified what they called a "pecking order" within the social sciences, led by economics on the top, pushing the behavioral sciences into the backseat and hampering cooperation between economics and other disciplines.[158] The difficulty of this "historical separation" between economics and the behavioral sciences should be dissolved.[159]

The kind of problems the advisory group envisioned as a basis for cooperation clearly placed the focus on the study of human behavior. In one way or another, the projects listed in the memorandum as worthy of support were nearly all concerned with the behavior of individual agents or groups.[160] Economics could benefit in that economic models and theories could be based upon empirically enriched behavioral foundations. As noted in the memorandum, projects would be supported with the ultimate "aim of […][applying] to economics generalizations about behavior that are drawn from social psychology and sociology and also, perhaps even more importantly, to use economic phenomena as one of the areas of empirical data out of which a generalized theory of human behavior can be built".[161] That an interdisciplinary collaboration might be exacerbated by limited resources, by the absence of facilities and appreciation of interdisciplinary work, by semantic difficulties that might hamper communication between disciplines ("e.g., 'motivations' in psychology and 'wants' in economics") and by the "unevenness of intellectual development among the disciplines" was obvious to the group. Yet, this did not diminish their hope in developing strategies that would foster collaboration and at the same time avoid those difficulties.

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[156] See Memorandum entitled 'The Ford Foundation – Behavioral Sciences Division, Advisory Group on Economics and the Behavioral Sciences', July 1952, folder 'unlabeled', box 99, JMP; p. 1.
[157] Ibid., p. 2.
[158] Ibid.
[159] Ibid.
[160] Besides investigating into the "political decisions of farmers" or the "investment decisions of firms", into "the perceived probability" and develop further the theory of consumer behavior, the group listed projects such as the "relation of economic behavior to personality types and values", the "motives and incentives in economic behavior", the "study of pressure groups with particular reference to how they affect economic equilibrium", the internal structure of organizations and, maybe most 'psychological', the "relation of values to economic behavior. What are the key relevant values? Which ones are easiest to change? What have been the changes historically in importance of such values as the 'work doctrine' associated with puritan and non-conformist religious groups?"; ibid, p. 5.
[161] Ibid.

That the content of this memorandum indicated a shift toward the study of actual behavior, which would not have favored a large number of the projects at the Cowles Commission, was quite explicit: "Preference might be given to those projects which propose to study actual behavior of persons in economic situations as opposed to the end products of behavior (e.g. 'dollar' behavior)", the intention was to make "use of actual observation" as opposed to a priori axiomatic premises of rational choice – even if interpreted as a normative theory.[162] The general tenor was: "If understanding human behavior is a major scientific objective, it might as well be studied in roles of important decision making since what we learn there will be applicable in other situations as well, and we will at the same time gain understanding which may be of practical use in solving the problems themselves".[163] Notably, Marschak was in favor of such a program, thereby in effect supporting the advisory group's analysis of his profession and the group's goals as legitimate; a position presumably seldom associated with economists at the Cowles Commission.

Besides Marschak, other leading economists were open to the outlined project of the Foundation. Among them were A. Alchian, O. Morgenstern and F. Mosteller; H. Simon himself was part of the advisory group. When this group met in July 1952 at RAND to discuss Carroll's memorandum, Marschak saw "the common ground among the fields" as lying "in the area of the theory of choice".[164] According to Marschak, economists could "bring to this problem their well-developed theories of rational choice, but the interdisciplinary approach is needed to bring about a proper balance of emphasis on the rational and non-rational aspects of choice".[165] Marschak was convinced that studying individual behavior as a major scientific objective should become one of the key research areas of this undertaking.

Between ongoing meetings between Marschak and the advisory group, it was only natural that, with respect to the then outstanding grant application, Berelson and others of the Ford Foundation voiced skepticism of the work being done at the Cowles Commission. While Berelson and Co. had looked favorably at studying of behavior with formal-mathematical methods, they considered mathematics only as a means to the end of an improved scientific study in the social sciences. In correspondence with Berelson and Carroll, Marschak began to insist that "[e]conomics is itself a behavioral science",[166] while Berelson associated the work of the Cowles Commission to be rather close to scholars like Nicolas Rashevsky that were "playing with mathematical models", thereby meaning "the disregard for the specific content of a social problem", and thus "putting the form before the content", which would have been

[162] Ibid., p. 6.
[163] Ibid.
[164] Simon to Carroll, 1 August 1952, folder 'unlabeled', box 99, JMP.
[165] Ibid.
[166] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP.

an activity that Berelson clearly rejected.[167] According to Berelson, a scientific undertaking "should start with a specific content problem" rather than with an interest in specific properties of abstract models.[168]

Whether the divergence of the predominant views held in the two institutions, Cowles and Ford, with respect to the role of mathematics had ultimately also been a decisive reason for rejecting the grant can only be speculated on. What becomes clear, however, is that Berelson wished to review the application against the backdrop of the results he expected from a survey among economists about how strategies to fuse economics and the behavioral sciences that the Advisory Group initiated.[169] Yet, the symbolic comparison with Rashevsky's work was telling. Rashevsky, a theoretical physicist of the department of physiology at the University of Chicago, had worked mainly on problems in mathematical biology. At that time he was considered as the pioneer of theoretical biology and mathematical biophysics, which he initiated by the end of the 1940s (Cull 2007). When Berelson made his remark, Rashevky had just published two books that tackled in different ways the link between mathematical biology and social behavior, in which he attempted to apply the approach of his mathematical biology to social phenomena, which, in his first book, was deductive from purely formal postulates and; in his second book, he attempted to support those postulates and their consequence by neurobiological concepts.[170] As such, Rashevsky's work was doubtlessly received as highly mathematical and abstract, especially because first principles were little, if at all, based upon any empirical findings (see e.g. Gibson 1952). Thus, Berelson's comparison of work at Cowles with Rashevky's mathematical biology provided a sense of the kind of abstract work the Cowles Commission was perceived as doing and which the Foundation actually rejected.

In the early 1950s, Marschak still appeared as a representative for the Cowles Commission and highly abstract mathematical work. For example, he defended the views of C. H. Coombs (mathematical psychology) and R.M. Thrall (mathematics) at the famous University of Michigan seminar, arguing that the Foundation could not discriminate against particular mathematicians and/or the ways in which mathematics became applied to the social sciences.[171] As the mathematics profession favored the formal beauty over the content of a

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[167] Marschak to Koopmans, 18 August 1952, folder 'The Ford Foundation', box 99, JMP.
[168] Ibid.
[169] Ibid.
[170] See Rashevsky's *Mathematical Biophysics* (rev. ed. 1948), *Mathematical Theory of Human Relations* (1948) and *Mathematical Biology of Social Behavior* (1951) respectively. That this was not a fully absurd comparison is already indicated by the fact that Rashevsky referred to von Neumann and Morgenstern (see Rashevsky 1951, p. 144).
[171] To organize the University of Michigan seminar Coombs and Thrall had received a Ford Grant to organize an eight-week seminar on interdisciplinary approaches to measurement and decision making, which was held in Santa Monica in the summer of 1952 – the 'Santa Monica Seminar'. The seminar's

problem, cooperation between mathematicians and non-mathematicians could not be enforced. By reporting to Koopmans, Marschak expressed agreement:

> I supported Coombs in his items […]: to avoid speaking pro domo sua I have chosen to defend the empiricism of the late Zipf and of Stewart of Princeton (both were derided), equating them with Tycho Brahe and the National Bureau and pleading that nobody can vouchsafe in advance for the sterility of this approach. I also supported Rashevsky: the 'playing with models' may be a way to express unambiguously what till now has remained vague; though R. might indeed benefit from closer cooperation with 'content' specialists – such as sociologists or biologists, similarly to what H. Simon is doing.[172]

This last remark of Marschak, however, elicits a tendency that would later become more apparent in his views and work: his urge to address the problems by what he considered as the most appropriate tools and not continue with the tools as *l'art pour l'art.* And while Marschak in this letter dismissed standard psychology books of C.L. Hull and G.A. Miller on behavioral psychology as "very clumsy, vague and tiresome",[173] he meant thereby the way in which mathematics had been used by social scientists, when no 'real' mathematician was present. Were a social scientist appropriately trained in mathematics and able to make use of this training in a qualified way, Marschak would have been the one to approved of any further interdisciplinary collaboration among disciplines.

Already in those early 1950s, Marschak appears to be on a track different than Koopmans. His positive attitude towards the main points of the Ford Foundation advisory group's memorandum (he judged the memorandum as "excellent"[174]) became even more apparent in a five-page long letter to Carroll dated August 1, 1952. Marschak went so far as to define economics explicitly as a behavioral science, thereby defending a broad understanding of economics and the "economic approach" as going beyond the traditional boundaries of quantifiable choice problems.[175] As a subsequent 'Digest of Replies' of all the responses by economists to the advisory group showed, Marschak was nearly the only economist that expressed unconditional agreement with the group's analysis of the disciplinary division between economics and the other (social) sciences, as well as the "current needs" for strengthening the collaboration between two fields.[176] In this letter, his writing differed remarkably from the more 'adaptive' language that he had used in the grant proposal. At a

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integrated theme was the use of mathematical methods in the social sciences and had resulted from a regular interdisciplinary seminar at the University of Michigan (Heukelom 2010; Smith 1992, p. 260).

[172] Marschak to Koopmans, 18 August 1952, folder 'The Ford Foundation', box 99, JMP; italics in original.

[173] Ibid.

[174] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP

[175] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP

[176] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP; 'Digest of Replies to Letter Concerning Report of Advisory Group on Economics and the Behavioral Sciences', folder 'unlabeled', box 99, JMP; p. 6.

follow-up conference that succeeded the 'Digest of Replies', organized by the advisory group on December 1st, 1952 at the University of Chicago, Marschak finally turned against some of his fellow economists. While Kenneth Boulding for example argued that economics were an aggregative science, Marschak countered that even if economics were not yet a behavioral science, "it ought to be one".[177]

From Marschak's point of view, the cross-fertilization between economics and other behavioral sciences could be fruitful in that economists could contribute a "particular point of view", namely optimization and certain quantitative methods. And in turn economists would receive from other behavioral sciences a better insight into the actual nature of decisions and thereby abandoning for example their usual neglect of the probabilistic character of human action, already a commonplace for experimental psychologists.[178] As such, it was not in "borrowing of propositions" but in the "marriage of methods" to formulate those propositions that Marschak saw the mutual benefit of interdisciplinary work in order to better tackle specific and disciplinary-independent problems: "in setting up a model (a hypothesis) and in designing an empirical test of it, by experiment or otherwise", sharing a common commitment to the scientific method and to the use of mathematics, statistics and logic as the common language.[179] For Marschak, the distinctiveness of the behavioral sciences made the use of the "Esperanto of mathematical and logical symbolisms [..] all the more effective".[180] Problems surrounding uncontrolled data and measurement procedures as well as the factor of uncertainty characterizing the social and human world were aspects that every the behavioral science would have to cope with and that required the precision, rigor and neutrality that only mathematics would be able to offer. So, even as the problem focus changed for Marschak, he still continued to consider axiomatic choice theory, the most mathematical theory of human behavior, as the appropriate framework to address those new problems – and carried it with him where he went.

Taking Axiomatic Choice Theory to the Behavioral Sciences

From the middle of the 1950s onwards, Marschak gradually turned away from economics and shifted towards research in behavioral sciences (Cherrier 2010). He began to work on a theory of teams with Roy Radner, contributed to a theory of organization and information, and undertook collaborative work on experimentation with Donald Davidson. Whether

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[177] See 'Notes on a Conference Organized by the Advisory Group of the Ford Foundation on Economics and the Behavioral Sciences held on December 1, 1952 at the University of Chicago', folder 'unlabeled', box 99, JMP; p. 1.
[178] Ibid, p. 1 ff.
[179] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP; 'Digest of Replies to Letter Concerning Report of Advisory Group on Economics and the Behavioral Sciences', folder 'unlabeled', box 99, JMP; p. 6.
[180] Ibid, p. 3.

Marschak's turn towards the more empirically grounded behavioral sciences can be interpreted as an exemplary case for a process in which either the agendas of patrons have a direct and shaping impact on the research that becomes executed[181] or whether his open attitude towards the direction pursued by the Ford Foundation just resulted from an already existent view and inherent interest that were met by the Foundation cannot be answered here. However, his interests ultimately shifted and are probably most adequately represented by the famous 'Interdisciplinary Colloquium on Mathematics in the Behavioral Sciences' that Marschak initiated when he moved to UCLA in 1960, taking up a joint appointment at the economics department and the School of Business Administration. I would hypothesize that the roots of those interests can be sought in the early 1950s.[182]

What remained with Marschak was his commitment to the mathematical/axiomatic method. For any behavioral problem, the main task of the behavioral scientist was to provide the mathematician with a precise formulation of the problem or method. "To present a behavioral model, or the method for testing it, in an unambiguous language so that a mathematician can work upon it, is in itself a fruitful challence [sic.] for the inhabitants of the Towers of Babel known as Social Science Buildings".[183] For Marschak, the main potentials of axiomatic choice theory still lied in its use as a normative theory in decision-making under uncertainty, as a method that would provide the structural characteristics of optimal decision-making, while the psychologist would supply the preferences and cognitive capacities that people have;[184] the mathematical economist would provide the "economic principle of consistent choices (also known as 'rational behavior principle')" from which reasoning could depart.[185] Marschak acknowledged that the low descriptive and predictive power of the principle even within the scope of economics.[186] However, coupled with the findings of the social psychologist, the "cross-disciplinary group studying [for example] speculators will be looking for a realistic compromise between the picture of mathematical statisticians engaging in speculation and the picture of the stampeding buffaloes".[187] Addressing questions such as "how would the buyers, sellers, bankers, stockholders, workers, famers behave if they consistently made choices that are best from their own point of view", Marschak saw the task

[181] See for example Cherrier (2013), who analyses the impact of the Ford Foundation program on economics by examining the changing relationship between Ford officials, their academic advisors and economists in the early 1950s.
[182] Marschak had moved, together with the Cowles Commission (then 'Cowles Foundation'), to Yale in 1955, were he stayed as a professor of economics until 1960.
[183] Marschak to Carroll, 5 August 1952, box 99, folder 'unlabeled', JMP; p. 3.
[184] One example for Marschak's views on how experimentation on actual behavior can inform optimal decision making can be found in Marschak (1964).
[185] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP; 'Digest of Replies to Letter Concerning Report of Advisory Group on Economics and the Behavioral Sciences', folder 'unlabeled', box 99, JMP; p. 2.
[186] See e.g. Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP.
[187] Marschak to Carroll, 5 August 1952, folder 'unlabeled', box 99, JMP; p. 2.

of the psychologist to offer a theory of actual behavior that would, by specifying the "givens" (i.e. the preferences, beliefs), enable the economist to formulate the problem of optimality and offer an adequate, i.e. best/efficient/optimal solution.[188] Thus, "[w]hile the economist will contribute his particular method of looking for rationality, his psychological colleague will enlighten him on how to design a series of experiments reproducing the essential elements of the investment situation".[189]

What had changed compared to his remarks in the context of the 'uncertainty-project' was Marschak's justification for using the axiomatic method, which was no longer based primarily on arguments in favor of epistemic values such as consistency, simplicity and formal elegance. According to Marschak the justification of axioms had to be sought in the psychology of the individual, a step that shifted a lot of attention towards actual individual choice. He argued that while relying on the axiomatic method, the aim of the economist should be to look for "vestiges of rational, or consistent, behavior in the study of actual behavior itself – to correct and to be corrected by his colleagues from other fields".[190] He saw that it would require not only an open mind but also a shift away from "traditional and current practice".[191] According to him, economists working in the marginalist tradition were limiting themselves to "the study of choices concerning material goods only", i.e. to "quantifiable problems" and thereby "limiting the field of choice to commodities that occur in indefinitely divisible amounts (continuous quantities), in order to apply certain elegant but unduly special methods (the so-called marginal analysis, a translation of differential calculus into words)".[192] His focus was "to the detriment of studies of 'qualitative' choices such as a business firm's choice of the best industrial location, the best technological process or the best organisational set-up".[193] The axiomatic method, the suggested replacement of marginal analysis, occupied for Marschak not the primarily theoretical construct as it had been used in the work of the Cowles Commission. Rather, it constituted the mutually enriching bridge between economics and the behavioral sciences to come one step closer to actual human behavior.

> Since other behavioral sciences often deal with phenomena not expressible in terms of continuous quantities, the thinking habits of a traditional economic theorist prevent him from seeing the common ground that exists between economists and the theory of human action in general.[194]

Thereby, Marschak believed in the importance of studying individual behavior for "descriptive economics", which was mostly concerned with "aggregative, anonymous 'end

[188] Ibid, p. 2 ff.
[189] Ibid, p. 2.
[190] Ibid, p. 4.
[191] Ibid, p. 1.
[192] Ibid, p. 1 ff.
[193] Marschak to Carroll, 5 August 1952, box 99, folder 'unlabeled', JMP; italics in original; p. 2.
[194] Ibid.

products of behavior'".[195] The actual psychological make-up of individual agents mattered for the explanation of aggregates because individual behavior in large parts determined aggregate behavior:

> The historical data on aggregate incomes, average prices, inputs and outputs of industries, and on other such end-products of human behavior should be supplemented wherever possible by 'cross-section' studies on the behavior of individuals, either non-experimental (family budgets, attitude questionnaires) or, still more desirable though more difficult, experimental. In fact, no serious economist uses historical data (time series) and their 'behavior' (such as periodic 'cycles', relations between prices and demand, etc.) without attempting some kind of explanation in terms of human actions, be this explanation one of home-cooked commonsense or of sophisticated theory. It is hoped that the cooperation with psychologists and others will cure economists of bad habits such as the mechanistic observation of 'patterns' of historical series (practiced, for example, by Wall Street astrologists).[196]

The ultimate step to profess his concern with behavior was when Marschak visited the 'Center for Advanced Study in the Behavioral Sciences', one of the institutional milestones of the Ford Foundation finally established in the fall of 1954. Under the head of Program Area V, the foundation set up this new research site at Stanford University. It was intended to embody the foundation's and Henry Ford's "optimism about the ability of basic science to help understand human behavior".[197] In an interdisciplinary fashion, young researchers from different social scientific disciplines were to be brought together to approach issues of human behavior from a wide range of perspectives, complementary to a narrow university training to enable collaboration and synthesis of knowledge rather than specialization. Initially, the idea was that of a training-, not of a research center.[198] The priority was laid on scientific investigation into economic behavior and only in a second step on applied work, "so that applications would not be misguided and would be broadly generalizable".[199] Marschak's visit of the Center in 1956 changed his life, as he wrote to Berelson in 1957: "the fellowship at the center has cured me of much of my previous parochialism, by exposing me to certain views that have originated outside my field, yet bear directly on my own work" (Marschak quoted in Cherrier 2013, p. 25).

Marschak's later work did not have a profound impact on the economics profession. His work on *Homo Stochasticus*, i.e. on stochastic decision theory, was more appreciated in psychology and his theory of teams was never really taken up by economists (Arrow 1978,

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[195] Ibid., p. 3.
[196] Ibid., p. 3 ff.
[197] Mission statement on the website of the center: http://www.casbs.org/early-years-and-mission [accessed 25th of February, 2013].
[198] Marschak to Koopmans, 18 August 1952, folder 'The Ford Foundation', box 99, JMP.
[199] Mission statement on the website of the center: http://www.casbs.org/early-years-and-mission [accessed 25th of February, 2013].

1991; Cherrier 2010). He worked on economics of information and a theory of organization, as well as on the formulation of decision rules inspired by psychological evidence. An appreciating letter by George A. Miller, a MIT psychologist, may be indicative of the general attitude of economists with which Marschak was confronted probably more often than not when he presented some ideas of his theory of teams:

> I enjoyed very much the clarity and incision of your presentation at Harvard. I was quite amazed to see how the great economists of that academic institution completely missed the point of what you had accomplished and persisted in discussing yesterday's problems. Perhaps I should feel comforted that even such great men as they can be caught in a rut.[200]

It was Marschak's progressive attitude of seeking to come to grips with the most pressing problems that led him to take seriously the empirical study of human behavior. Axiomatic choice theory remained a means, not the end. As such, he was a mediator between disciplines, a person that draws rather than pulls the strings, the node in a network, so to say, using mathematics as the language that binds all together.

5. Conclusion: Rational Choice Theory Traveling

The previous analysis is meant to contribute to a narrative of how axiomatic choice theory traveled - from Princeton through Chicago and Santa Monica to Stanford and Los Angeles. It was neither a theory developed primarily by economists, but rather was introduced to economics as it was introduced to other disciplines. Nor was it originally intended to become primarily a psychological theory of individual behavior. Rational choice theory in its various manifestations was travelling between different mathematical formalisms; it was being applied to prescribe rules of proper behavior, as representation of behavioral hypotheses, and as measurement device to capture individual values. It traveled, but as much as it spread throughout the different research sites and among a network of scholars concerned with distinct problems, it also stayed within a particular research site, preserving a particular culture by retaining a specific formulation that would allow addressing problems of concern in this particular institutional context. New modifications of axiomatic choice theory did not fully replace old versions of it, which prevents a reconstruction of its travels as a continuous process. Rather, different versions of axiomatic choice theory existed parallel to one other.

In his article "Mathematical Models, Rational Choice, and the Search for Cold War Culture", Erickson (2010, p. 392) draws attention to the "tangled histories" of rational choice theories and the "exceptional diversity of activities and agendas" that have become subsumed

[200] Miller to Marschak, 29 April 1953, folder 'unlabeled', box 150, JMP.

under the label ‘rational choice theory’. While the common narratives of the development of the social sciences in the Cold War period offer a partial explanation of the emergence of game theory, axiomatic theories of human behavior spread throughout numerous disciplines including evolutionary biology and political science as a result of “a diverse collection of debates about the nature of ‘rationality’ and ‘choice’ that marked the Cold War era” (Erickson 2010, p. 386). Erickson points to the importance of taking the diversity of Cold War culture in American social science seriously when we want to understand the wide-spread development and application of mathematical models of rational choice.

Marschak’s professional development is just one case that allows us to address the more general theme of formulating the biography of a framework – axiomatic choice theory – that became extensively used from the 1940s and which reflects this diversity of culture in American social science. Primarily developed by a mathematician, axiomatic choice theory was applied to a broad range of problems and modified with respect to various ends in mind. Its attractiveness was in large parts due to its flexibility and adaptive qualities. Versions of axiomatic choice theory, namely “[g]ame theory, utility theory, and social choice theory provided mathematical tools that could be reworked to engage with any number of debates over the nature of ‘rationality’ and ‘choice’ that often unfolded independently of state funding, but that nevertheless were characteristic of American intellectual culture during this period” (Erickson 2010, p. 388). Theories of rational choice could well be characterized as “a kind of mathematical currency” during the cold war period, “flowing along lines of cultural contact and facilitating exchange, producing flashes of understanding between communities whose comprehension of each other’s intellectual worlds was otherwise significantly limited” (Erickson 2010, p. 392). Like a currency, axiomatic choice theory could cross boundaries of disciplines and became adopted and further developed at different research sites – the Cowles Commission, RAND Corporation, the Center of Advanced Study in the Behavioral Sciences, the Michigan group, and others - by a specific group or community and within a specific research culture. By revealing the diversity of manifestations of axiomatic choice theory within the context of social networks and within particular research sites, this account of axiomatic choice theory allows us to explain the disunity and confusion surrounding current debates about rational choice theory.

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