

Herfeld, Catherine

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BETWEEN MATHEMATICAL FORMALISM, NORMATIVE  
CHOICE RULES, AND THE BEHAVIORAL SCIENCES: THE  
EMERGENCE OF RATIONAL CHOICE THEORIES IN THE LATE  
1940S AND EARLY 1950S

BY

CATHERINE HERFELD

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CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
AT DUKE UNIVERSITY

*Between Mathematical Formalism, Normative Choice Rules, and the Behavioral Sciences: The Emergence of Rational Choice Theories in the Late 1940s and Early 1950s<sup>1</sup>*

Forthcoming in *European Journal of the History of Economic Thought*

Catherine Herfeld

(University of Zurich)

Contact: <http://catherineherfeld.weebly.com>

## **Abstract**

This article traces a normative turn between the middle of the 1940s and the early 1950s reflected in the reformulation, interpretation, and use of rational choice theories at the Cowles Commission for Research in Economics. This turn is paralleled by a transition from Jacob Marschak's to Tjalling Koopmans' research program. While rational choice theories initially raised high hopes that they would serve as empirical accounts to inform testable hypotheses about economic regularities, they became increasingly modified and interpreted as normative approaches offering behavioral recommendations for individual agents, organizations, government, or teams. The predefined elements constitutive of these accounts, inspired by simple rules of logic were now meant to represent the basic demands of rationality, while theories of rational decision-making specified rules of conduct that were meant to shape rather than explain behavior.

**Keywords:** history of rational choice theory, Cowles Commission, normative turn

**JEL Classification:** B00, B2, B3, B4

## **1. Introduction**

A persistent criticism of rational choice theories is that the underlying model of human agents contradicts the findings of psychology. The contradiction is frequently attributed to the reluctance of economists to allow psychology to inform their theories and models, settling instead for what critics claim are inadequate explanations of human behavior (e.g., Lewin 1996). As a result, the status of economics as a science

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(it is said) can be questioned, beset as it is by the empirical problems that result. The neglect of psychology is often construed largely as an “escape from psychology,” an attempt to free economics from any dependence upon mental variables and psychological processes, consistent with an image of science increasingly inspired by mathematics (Giocoli 2003, 2005, 183), and as a refusal to collaborate with other behavioral sciences in order to maintain its privileged status as queen of the social sciences, a status that economics acquired in the 1940s and 1950s (see Backhouse/Fontaine 2010b, Pooley/Solovey 2010).

The escape from psychology has been traced back to the first half of the twentieth century, beginning with Vilfredo Pareto’s and Lionel Robbins’s skepticism of cardinal utility theory, especially about the existence of a cardinally measurable utility function and the assumption that subjective experiences could be measured (e.g., Bruni 2010, Robbins 1932). It reached its peak in the early years of mathematical economics and the increasing use of axiomatic theories of rational choice, particularly the axiomatic formulation of the expected utility principle, after the publication of the *Theory of Games and Economic Behavior* in 1944 by John von Neumann and Oskar Morgenstern. In this paper, I suggest that the reconstruction of the early Cold War history of rational choice theories as an escape from psychology is too one-sided. I attempt to answer two questions: Whether, and if so, why did mathematical economists in the 1940s and 1950s favor a formal-axiomatic theory of human behavior over a behavioral choice theory, i.e., a theory of choice drawing directly upon psychological concepts and findings?

Focusing on Jacob Marschak’s and Tjalling Koopmans’s years at the Cowles Commission for Research in Economics, the stronghold of mathematical economics in the late 1940s, I address both questions by tracing the ways in which some mathematical economists justified and used theories of human behavior. I argue that one important reason why formal-axiomatic theories of rational choice were preferred over behavioral choice theories was that they allowed mathematical economists to improve the conceptual and theoretical foundations of economics and thereby increase its scientific status, given the image of science that they were committed to. Towards making economics progress, axiomatic theories of human behavior fulfilled various conceptual and methodological roles that mathematical economists

considered pressing at the time. However, the separation between mathematical economics and the other behavioral sciences was not as clear-cut as it has often been argued with regard to the favored model of human behavior. Rather, this relationship was characterized by a protracted tension on the part of at least some mathematical economists that was manifested in their seemingly inconsistent desire to, on the one hand, pay lip service to the behavioral sciences by acknowledging the importance of empirical research on behavior, particularly that of psychologists, and, on the other hand, commit wholeheartedly to axiomatization in mathematical economics.

More specifically, some mathematical economists such as Jacob Marschak and others at the Cowles Commission were initially reluctant to modify their account of human decision-making on the basis of psychological research about actual decision-making, mainly in order to preserve the rigor and generality to which a proper science, according to them, needed to conform. At the same time, they were well aware that their axiomatic theories of rational choice could not explain or predict actual decision-making. They noted that a psychologically enriched theory would eventually be required in economics - but only after economics had established itself as scientific. Consequently, theories of rational choice - and most prominently the axiomatic formulation of the principle of expected utility - emerging in the 1940s and 1950s reflected that mathematical economists were torn between an image of science in which they were expected to provide formal-axiomatic foundations to economic theories while making the mathematical economics practiced at Cowles relevant and marketable. The changing interpretation and uses of rational choice theories were furthermore contingent upon different epistemic goals, which let economists move between various roles, i.e., providing abstract-mathematical foundations for economic theory, prescribing the rational course of behavior to consumers, firms, and investors, and explaining and predicting human behavior that would have to rely on research the behavioral sciences, and particularly in psychology.

Shedding light on this tension and on the multifaceted character of rational choice theories as reflected in the diverse roles economists played is the main contribution of this paper. By emphasizing the ways in which theories of rational choice were justified by mathematical economists, my analysis is meant to complement rather than contradict existing accounts of rational choice theories that mainly focus on the

political, social, and institutional contexts during the early Cold War period (e.g., Amadae 2003, Erickson et al. 2013, Isaac 2010, Mirowski 2002b, among others). Elaborating on what mathematical economists took to be the conceptual and methodological roles of rational choice theories and looking at the ways such theories were justified in those early years can enrich the historiography by providing a perspective not yet worked out in the literature on the emergence of rational choice theories during that period. This paper furthermore contributes to the history of the relationship between mathematical economics and the behavioral sciences during the early Cold War period in the US. Discussions about how to write the recent history of economics have stressed the importance of relating this history to the history of other social and behavioral sciences (Backhouse/Fontaine 2010a), which is particularly important in the historiography of tools, concepts, and theories that have been used across disciplines and for which theories of rational choice are a case in point.<sup>2</sup>

## 2. Rational Choice Theories as Axiomatic Theories

The history of rational choice theories has frequently been written as the prototypical instantiation of the escape from psychology in economics. While *rational choice theory* as a label has been used to refer to various distinct theories of decision-making, those theories share two core characteristics: first, they rely upon the axiomatic method and, second, they rely on a definition of rationality as consistency (e.g., Giocoli 2005). A rational choice is defined as a consistent choice among a set of alternative options. Mathematically, consistent choice is modeled by imposing a small set of axioms – such as completeness and transitivity – on a binary relation that is supposed to represent an agent’s preferences. The consistency view and the traditional utility-maximization view of rationality are conceptually equivalent (Giocoli 2005, 180). Nevertheless, the former gives a purely formal and highly general representation of human behavior. It abstracts from the individual agent’s psychological makeup and as such is as applicable to a human being as it is to a machine computing the rational decision according to a set of formal algorithms. The ‘empirical emptiness’ implied poses in turn an obstacle to explaining individual behavior with the approach (ibid., 181). While the consistency view offers “a logically exhaustive and intellectually

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<sup>2</sup> See, for example, Erickson (2010) and Isaac (2010), who consider theories of rational choice as tools that crossed disciplinary boundaries during the early Cold War years.

appealing representation of the phenomenon of choice,” it does not necessarily stand empirical scrutiny (ibid., 183). Reducing traditional value theory in economics to the ‘logic of choice’ thus shows how economists tried to rid their approach of psychology (Giocoli 2005, 183).

The introduction of the axiomatic method brought with it a shift towards defining rational choice as consistent choice. Yet, the main motivation behind the introduction was to ground economics upon axiomatic foundations. When Jacob Marschak took up the directorship of the Cowles Commission in 1943, the axiomatic method had not yet been widely adopted among economists (see also Cherrier 2010, 451). Its gradual adoption took place only from the mid-1940s onwards (Grant/van Zandt 2009, 22), when mathematical economists began to draw more extensively upon mathematical logic, and in particular fixed-point techniques, set theory, and topology (e.g., Weintraub 2002).<sup>3</sup> Applying tools and concepts from logic was understood as a systematic way to explicate scientific concepts, allowed economists to use logical rules of inference, and enabled rigorous reasoning (Stigum 1990). Using the axiomatic method amounted to the “organization of a deductive system in a strictly axiomatic form” (Blanché 1973, 162). It signaled a concern with the formulation and ‘ordering of first principles’, their internal logical coherence as well as of formal structures derivable from them, aspects already broadly appreciated in the natural sciences, mainly in physics. Axiomatic theories of rational choice were offering such first principles. They differed from the traditional accounts of cardinal and ordinal utility theory in the kind of mathematics used, the role attributed to mathematics, and the problems to which their application could help to find a solution. Pointing to those differences allows us to see that from the very beginning ambiguity surrounded the interpretation and methodological status of theories of rational choice that emerged in the late 1940s and early 1950s. Rational choice theories were an instance of early mathematical formalism in economics and not necessarily to be interpreted as empirical theories of individual human behavior.

Axiomatic theories of rational choice gained increasing legitimacy in economics after the publication of von Neumann and Morgenstern’s *Theory of Games and Economic*

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<sup>3</sup> Only in the middle of the 20th century did economics become increasingly a modeling science (e.g., Knuuttila/Morgan 2012).

*Behavior* in 1944. The heart of the *Theory of Games* was the minimax theorem, a prerequisite for modeling rational choice under strategic uncertainty as minimizing the potential costs of a worst-case outcome in situations of risky prospects, and an axiomatic formulation of Daniel Bernoulli's expected utility principle (Leonard 2010, Weintraub 2002). The axiomatic version of the expected utility principle would eventually stabilize in economics (and other disciplines) as expected utility theory, the most influential theory of decision-making under risk (Moscati 2016a). For von Neumann and Morgenstern, economics had failed to formulate its problems in a clear way because of the excessive yet unsuccessful use of mathematics: "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" (von Neumann/Morgenstern 1953 [1944], 4). Furthermore, economists had used the wrong kind of mathematics, i.e., infinitesimal calculus and linear algebra, and thus could offer only mathematical assertions rather than formal proofs. This had created difficulties without improving the understanding of the complex problems economics had to cope with, which was mainly the problem of social interaction. Drawing upon axiomatic set theory and topology would address those difficulties.<sup>4</sup>

Von Neumann and Morgenstern did not consider their axiomatic 'theory of rational behavior' as their key contribution to economics and were not concerned with formulating a decision theory (Giocoli 2005, Leonard 1995). Apart from the fact that the book contained very few references to recent economics (Hurwicz 1945, 924), they apologized for bypassing the problem of utility albeit acknowledging it as a "subject of so great a conceptual importance" (von Neumann/Morgenstern 1944, 16). The formulation of the expected utility principle was opportunistic. Because it allowed for deriving numerical utilities by an operationalist procedure of using probability values instead of introspection, it primarily served as a tool to bridge the conceptual gap between the new approach to the problem of strategic interdependence – game theory – and the traditional characterization of rationality as constrained maximization of monetary payoffs (Giocoli 2006, 102 f.). It was the minimax

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<sup>4</sup> 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 in economics, one that partly enabled the development of axiomatic theories of rational choice. Backhouse and Medema (2009a, 2009b) claim that Robbins's definition was closely linked to the acceptance of the axiomatic method in economics.



principle for two-person, zero-sum games that von Neumann and Morgenstern considered to be their core characterization of rational behavior (Giocoli 2005, 2006). The minimax principle as an axiomatic theory of interactive choice was entirely psychology-free, according to von Neumann. He had chosen the minimax rule as the solution concept because it allowed each player to make rational decisions independently of the other player's decisions. As such, von Neumann could pursue his normative agenda. However, it was its by-product - expected utility theory – that would become dominant in economics.

The average neoclassical economist of the late 1940s and 1950s was not interested in those new developments in mathematical economics at Cowles. At Princeton, not in the economics department but in the mathematics department, particularly strong in topology and algebra, the Institute for Advanced Study and the mathematics department provided the stimulating environment for discussing and further developing the “new mathematics of society” of von Neumann and Morgenstern (Leonard 2010, 6). To make game theory fruitful for economics and popularize the axiomatic version of the principle of expected utility as well as other axiomatic theories, mathematical economists were needed to promote the usefulness of such theories and the new set of logical tools used in connection with them, as well as any results derived from the theories. Jacob Marschak was one such promoter. Inspired by the *Theory of Games*, Marschak fostered a shift in focus towards the expected utility principle as a new decision theory, and Cowles, more generally, became the stronghold for a research program in mathematical economics that had axiomatization as one core ingredient.

### **3. The Role of Rational Choice Theories in the Cowles Research Program**

What would stabilize as the dominant decision theory in economics in the second half of the 20th century had been developed mainly by von Neumann and had gained a foothold in economics mainly through Morgenstern (Moscati 2016a, 2016b). After 1944, the expected utility principle would soon develop 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. The place where axiomatic choice theories and in particular von Neumann and Morgenstern's book received immediate attention was the Cowles Commission for Research in Economics, located at the

University of Chicago until the early 1950s. From 1943 onwards, the commission was directed by Jacob Marschak, who was succeeded by Tjalling Koopmans in 1948. Most researchers at Cowles were econometricians and mathematical economists, a still small but growing community that had found its home among like-minded economists (Weintraub 2002).

The *Theory of Games* immediately caught Marschak's attention. By the 1950s, Marschak would himself further develop the set of axioms of von Neumann and Morgenstern's expected utility principle (Marschak 1950). But already much earlier, he had familiarized himself with their book and, unlike von Neumann, saw its potential for economics (Arrow 1991, 140).<sup>5</sup> In 1946, after the first reviews of the *Theory of Games* by Herbert Simon and the American mathematician and probability theorist A. H. Copeland, Marschak published an extensive review of the book in the prominent *Journal of Political Economy* with the goal of "acquaint[ing] the reader with its economic results" (Marschak 1946, 98). Marschak not only presented the new "theory of rational behavior" as conforming to the proper standards of science (see also Cherrier 2010, 451), but identified the *Theory of Games* as the main reason for that. Marschak took the *Theory of Games* to be an exemplary case of using much needed 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, 114).

Marschak praised several things about the book: von Neumann and Morgenstern's "meticulous formalism", the separation between the axiomatic structure of the theory of behavior and empirical reality, the flexibility that this separation allowed regarding the theory's applicability, and the rigor and precision that would come with an improved scrutiny of logical foundations and deductive analysis in economics.

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

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<sup>5</sup> Milton Friedman and Leonard Jimmy Savage were also among the early supporters of von Neumann and Morgenstern's formulation of the expected utility principle (see Moscati 2016a).

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, 115).

In Marschak's view, the initial separation of formal-axiomatic structure and empirical, e.g., psychological, interpretation was a virtue rather than a vice of the axiomatic version of the expected utility principle. A purely formal-axiomatic framework of von Neumann and Morgenstern allowed for the application of expected utility theory to structurally similar but different problems across disciplines. What the scientist had to do to relate the theory to reality was to take this highly abstract framework and modify it in such a way that it was applicable to the respective problem at hand. As such, albeit abstract, it could in a second step become empirically useful. At least in the case of economics, Marschak concluded that "ten more such books and the progress of economics is assured" (ibid.). However, Cowles scholars did not immediately take up this new research and some were skeptical about von Neumann and Morgenstern's contribution (Hurwicz 1945). If scholars at Cowles engaged with the *Theory of Games*, they mainly discussed the minimax solution and the concept of a two-person, zero-sum game.<sup>6</sup> Reflecting on econometrics as the major focus of the commission at the time, others were more skeptical of the relevance of such research. Hurwicz questioned the unconditional embrace of axiomatization and rigor at the expense of solving actual problems of the economy.

Despite such skeptical voices, Marschak heavily promoted the expected utility principle. Having been widely distributed among the Cowles scholars, Marschak's review played a pivotal role in shifting attention towards the expected utility principle as a core contribution of von Neumann and Morgenstern and acknowledging the problem of uncertainty in decision-making. Marschak's review of the *Theory of*

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<sup>6</sup> In 1949, for instance, Cowles scholars such as Kenneth Arrow, Herbert Simon, Leonid Hurwicz, Leonard Savage, and Marschak discussed game theory and its application in a series of seminars; see <http://cowles.yale.edu/commission-seminars> [accessed March 14, 2017].

*Games* increased the popularity of the expected utility principle as new way to address conceptual and foundational questions in economics via axiomatics and formalization (see also Hildreth 1986, 98). Furthermore, Marschak had “explained the abstract concepts and supplemented the examples provided by von Neumann and Morgenstern” so that the applicability of those new concepts to economic problems became more obvious (ibid.).<sup>7</sup> His own contribution to the axiomatization of the principle (Marschak 1948a, 1948b, 1950) fostered this promotion. Parallel to that, mathematical formalism, the axiomatic method, and Bourbakism became more widely appreciated at Cowles, reaching its peak under Koopmans’ directorship and with Debreu joining the staff in 1950 (Weintraub/ Mirowski 1994, Weintraub 2002, 119 ff.).

The long-lasting influence of the *Theory of Games* and Bourbakism at Cowles would be fostered by Koopmans’ research focus and only become visible from the 1950s onwards (Mirowski 2002a, 2002b).<sup>8</sup> While the long-term research agenda that Marschak envisaged for the commission was heavily focused on the development of applicable mathematical tools, Marschak still acknowledged that empirical investigation of human behavior would be needed to ultimately realize this agenda and make mathematical economics relevant for addressing real-world economic problems. In a 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” and are theoretically tested with the goal of making accurate predictions, a two-fold procedure he took to be the task of good science.<sup>9</sup>

But Marschak was not only convinced that new mathematical tools were needed for economics to progress. He also aimed at fixing a set of conceptual and theoretical

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<sup>7</sup> 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.yale.edu/commission-seminars> [accessed March 14, 2017].

<sup>8</sup> See also Christ (1952), who identifies two additional origins of the work on rational behavior, namely, Arrow dissertation and Koopmans’ work on activity analysis and linear programming, which cannot be treated here (see Weintraub 2002, Düppe/Weintraub 2014).

<sup>9</sup> See ‘Memorandum to Rockefeller Foundation’ 23 May 1947, folder ‘Cowles Commission for Economic Research, 1946-1947’, box 216S, RAC.

issues with a mathematical theory of human behavior, which partly explains his fascination with expected utility theory. Upon his arrival at Cowles, Marschak undertook a fundamental reorientation of the commission's research program reflected in three categories, under which the studies undertaken at the commission were subsumed: 1) wartime peace control and rationing; 2) studies in economic behavior; and 3) unemployment and business cycles. This research orientation placed the focus on econometrics, i.e., roughly the application of mathematical statistics to empirical problems by using the structural estimation method (Weintraub 2002, 118). Economic theory informed the structural equations in this program, and theories of human behavior played a specific role. Marschak was interested in a formal-mathematical theory of human behavior, but that theory would in a second step have to be empirically useful. Especially studies in economic behavior that analyzed the behavior of consumers, entrepreneurs, firms and investors required new mathematical tools to improve econometric analysis.

More specifically, a major concern that became apparent during Marschak's directorship was to define in a theoretically rigorous manner rational economic behavior under uncertainty. Research into decision-making under uncertainty was motivated, at least in large part, by a theoretical problem posed by Marschak's research program. 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, 241).<sup>10</sup> Marschak understood economic relationships (the 'economic structure') to be fundamentally stochastic in character. Structural equations were used to describe those economic relationships. The equations had to be formulated in probabilistic terms, given random disturbances.<sup>11</sup> One major role of economic theory was to inform the formulation of those equations, whose predictions would in turn be tested, for example concerning human behavior (of a specific group of people), technologies, or legal rules. By placing emphasis not only on data-based

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<sup>10</sup> Their concern about mapping theory and data had also been 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.

<sup>11</sup> See Cowles Commission for Research in Economics (1946).

testing but also on how theory could inform hypothesis formulation and the estimation of model parameters, the individual agent 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.<sup>12</sup> An accurate theory of human behavior was thus important.

The importance that Marschak placed on conceptualizing individual behavior under uncertainty in econometrics was not new. In his early work on demand theory and the measurement of elasticity for his habilitation in Berlin and Heidelberg, Marschak had acknowledged the importance and usefulness of data about individual households such as family budgets to generate the aggregate demand curves.<sup>13</sup> 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 a first instance of mathematical modeling, but also an attempt to cope with decision-making under uncertainty with methods from mathematical statistics. Arriving in New York on a Rockefeller Fellowship and taking up a position at the New School for Social Research, Marschak held a small econometrics seminar with Trygve Haavelmo, Joseph Schumpeter, Wassily Leontief, Franco Modigliani, Abraham Wald, Tjalling Koopmans, Paul Samuelson, and Kenneth Arrow as regular attendees (Cherrier 2010; Hildreth 1986, 8; Louça 2007, 44). In developing further his ‘structural estimation method’ with almost the same group at Cowles, Marschak, in line with Haavelmo (1944), again increasingly emphasized the importance of probability theory and theories of human behavior to inform hypotheses about consumer, investment, and producer behavior under uncertainty.<sup>14</sup>

Another important role of economic theory and thus of theories of human behavior in Marschak’s research program was estimating the parameters used in a structural equation model. Marschak considered rational choice theories but also psychological studies that investigate systematic deviations from the rational course as equally

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<sup>12</sup> See, e.g., Hoover (2006, 240 f.), who points 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.

<sup>13</sup> Marschak completed his habilitation thesis, which was entitled ‘Elastizität der Nachfrage’ (in English: elasticity of demand), in 1930.

<sup>14</sup> At the Cowles Commission, Marschak hired Haavelmo, Koopmans, Arrow, Lawrence Klein, Evsey Domar, Don Patinkin, Simon, Hurwicz, George Katona, H. Gregg Lewis, and Oscar Lange among others (Hildreth 1986, 8).

important in that context. In a research report for the years 1942-46 summarizing the major goals at the time, he stated:

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.<sup>15</sup>

According to the research report, the commission primarily envisaged an improvement of 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, an improved theory of behavior would benefit the formulation of structural relations about behavior and changes of structure.

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.<sup>16</sup>

A theory of rational choice that aimed at accounting for uncertainty, albeit conceptually reducing uncertainty to risk, fitted the bill, albeit only as a very first step:

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 hypotheses a form amenable to statistical tests.<sup>17</sup>

Marschak acknowledged that the principle of expected utility maximization was only a point of departure for economic reasoning. He proclaimed that eventually

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<sup>15</sup> See Cowles (1946, 5; italics in original).

<sup>16</sup> See Cowles (1946).

<sup>17</sup> Ibid.

introducing “plausible psychological patterns” about, for example, expectations would ultimately become relevant to formulating hypotheses about savings behavior, investment behavior, and consumption behavior under uncertain conditions, as well as to forming expectations, whose relevant parameters would themselves be estimated and ultimately inform the structural equations that were supposed to characterize the economy (ibid, 14 ff.). But to introduce psychological results about actual behavior would be to revise the foundations of economics, and such a revision could happen only one step at a time; the principle that people maximize their expected utility as a rough and ready approximation of actual behavior was useful as a first step – nothing more and nothing less.

Repeated references to Hurwicz’s and Marschak’s reviews of the *Theory of Games* in the Cowles research reports in the mid-1940s provide evidence that they considered formal-axiomatic choice theories, such as expected utility theory, as being already one step ahead of traditional marginal utility theory. It was an improvement in that von Neumann and Morgenstern had tried to take uncertainty seriously, albeit reducing uncertainty to calculable risk, and that their account promised to get closer to a solution of the problem of finding an adequate theory of decision-making under uncertainty. The role and status of expected utility theory changed again in this light; its status as an empirical decision theory approximating human behavior was contingent upon Marschak’s research agenda and views about what rational choice theories in economics should accomplish in this context.<sup>18</sup>

The problem of theoretically grounding individual human behavior remained one ongoing concern for Marschak, justifying his preference for formal-mathematical theories of rational decision-making. In a research plan for the commission drafted for the Rockefeller Foundation in 1947 and in later writings as well, Marschak placed the ‘revision of economic fundamentals’ at its core, i.e., requesting

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

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<sup>18</sup> This reading of Marschak also reflects in his concern with the measurability of utility and his subsequent attempts to resurrect expected utility, not only as a normative, but also as an empirical theory.



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 so far, which treated fiscal policy as an 'exogenous variable' ...).<sup>19</sup>

In light of Marschak's concerns about revising the foundations of economics, the 'measurement-without-theory' debate between Tjalling Koopmans and Rutledge Vining, and the image of science at Cowles increasingly inspired by mathematics, Koopmans as director would foster a further shift at the commission not towards behavioral but towards formal theories of rational choice.<sup>20</sup>

#### **4. Placing Basic Research at the Core of the Cowles Research Program**

As director of the commission, Koopmans placed even greater emphasis on basic over applied research to increase the scientific status of economics. While this phase of basic research was supposed to eventually be followed by another phase of empirical research, theoretical studies were increasingly prioritized. A simple but mathematically sophisticated theory of rational choice would be the appropriate starting point for such a program; pursuing extensive and interdisciplinary studies into the facets and complexities of real human behavior to formulate the basic principles of decision-making would be far too difficult if not even a simple version of the problem could be formulated precisely. Illustrative of this direction towards purely theoretical research into human decision-making under Koopmans' directorship was the project of decision-making under uncertainty that began in the late 1940s and early 1950s. Upon request of the Association of Casualty and Surety Companies, Koopmans considered the possibility of pursuing such a project and wrote 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

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<sup>19</sup> See attachment of letter from Marschak to Willits, 'General Outline of Research Plans' 22 May 1947, folder 'Cowles Commission for Economic Research 1946-1947', box 216S, RAC.

<sup>20</sup> I am not arguing that the axiomatic approach was necessary for the structural estimation method. Axiomatics and expected utility theory, however, provided one way to theoretically ground behavior under uncertain prospects. It should also be remarked that much of the development of theories of rational choice under uncertainty at Cowles took place rather isolated from the economics profession and was perceived as being highly abstract and exotic. Furthermore, the method and problems addressed in econometrics were not necessarily those shared by the economics profession as a whole.

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.<sup>21</sup>

Koopmans asked Hurwicz to formulate a possible framing of such a project, on the individual as well as on the aggregate level.

It could involve both the decisions of the individual under uncertainty (such as studied by Jascha [Marschak]) 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.<sup>22</sup>

Pointing to the need for studying human behavior empirically was at this point only Koopmans paying lip service to agents interested in ultimately applying those models of decision-making, such as insurance companies. But in this phase of basic research, studies of decision-making under uncertainty would, according to Koopmans, “frankly be theoretical in the first instance,” and the developed theories would not be straightforwardly applicable.<sup>23</sup> Hurwicz was undoubtedly one of the major theoretical economists who contributed to this project. Others were Kenneth Arrow, Gerard Debreu, Roy Radner, and Erling Sverdrup,<sup>24</sup> with Marschak as the principal investigator. In a draft memorandum entitled “Economics of Uncertainty: A Research Project” dated April 24, 1950, Marschak laid out the problems arising in studying decision-making 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 -

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<sup>21</sup> Koopmans to Lang, May 8, 1950, folder ‘uncertainty’, box 156, JMP.

<sup>22</sup> Koopmans to Hurwicz, April 5, 1950, folder ‘uncertainty’, box 156, JMP.

<sup>23</sup> Ibid.

<sup>24</sup> See <http://cowles.yale.edu/sites/default/files/files/pub/rep/r1932-52.pdf> [accessed September 12, 2016]. 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 completed his PhD in mathematical statistics at the University of Chicago, and Erling Sverdrup was a mathematical statistician. Others who were working on axiomatics more generally at the time at Cowles were two mathematicians, Israel N. Herstein and Princeton’s John Milnor; the two collaborated on a paper entitled ‘An Axiomatic Approach to Measurable Utility’ (1953).

to achieve the decisions that are best from the point of view of the firm, or of the nation”.<sup>25</sup>

Marschak himself noted in his report that Steps I and III are normative and result in “efficient methods for business firms and governments”.<sup>26</sup> Step I would provide a benchmark for Step II to measure the irrational deviations observable in reality. Step II would be descriptive in nature, and one would expect that to be the place in which economics and psychology were considered to overlap: The purpose of research in Step II would be “to observe the most frequent habits of behavior of various decision-makers (including business firms and governments), habits that are possibly obsolete, inefficient, irrational”.<sup>27</sup> This empirical information about the “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.<sup>28</sup> The theoretical characterization of individual behavior (by, e.g., some version of rational choice theory) in Step I was justified as the point of departure:

An ‘ideal’ businessman who uses modern statistical methods in controlling the quality as well as in 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.<sup>29</sup>

Investigating human psychology and the variety of human motives was, if anything at all, investigating *economic* behavior, and also then only if it advanced basic theoretical research in economics. Given the early stage in which economics found itself, foundational research should be the emphasis of the Cowles research program.

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<sup>25</sup> See ‘Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak’, 24 April, 1950, folder ‘uncertainty’, box 156, JMP; 1 f.

<sup>26</sup> Ibid.

<sup>27</sup> Ibid., 2 f.

<sup>28</sup> Ibid., 2 ff. It should be noted that, generally, normativity can then enter the picture in two interrelated ways. First, *interpreting* a rational choice theory as a normative theory, the preference axioms and the choice rule are our accepted rationality standards. We take for example transitive preferences as something that we value and that our preferences should satisfy if we want to be considered rational. Second, when we *use* a theory as a normative theory: it tells us how we *ought* to choose in order to behave rationally, for example, choose the option with the highest expected utility. The theory becomes an instrument to formulate norms that secure the rational course of behavior, rather than having the status of an empirical theory that allows for the formulation of hypotheses about actual behavior. In both cases, normativity is in the first instance disconnected from morality, in that the normativity involved does not entail the prescription of the ethically appropriate course of behavior.

<sup>29</sup> See ‘Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak’, 24 April, 1950, folder ‘uncertainty’, box 156, JMP; 2 f.

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

From 1948 onwards, the core categories for the Cowles research program were “actual behavior” and “rational behavior”, which suggested that empirical studies of human behavior would play an equally important role but in fact did not. The former was directed at empirically observable behavior, at describing behavior that could actually be observed and finding the general laws it was governed by; the latter was understood traditionally as optimization, i.e., the behavior constituting the optimal course of action for an individual or a group to realize a given goal. Studies subsumed under both categories were called ‘descriptive studies’ and ‘prescriptive studies’, respectively.<sup>30</sup> As in the project on decision-making under uncertainty, descriptive studies would be essential for prescriptive studies of rational behavior, as the rational chooser had to take into account how other people behave. As stated in the Twenty-Year Research Report:

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.<sup>31</sup>

The project of decision-making under uncertainty matched perfectly with Marschak’s goal for the commission and bridged the gap between the theoretical and the empirical dimension. Formal theories of rational choice provided a theoretical grounding for the

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<sup>30</sup> See Christ (1952).

<sup>31</sup> Cowles Commission (1952, 48; italics in original).

formulation of hypotheses and the estimation of axiomatically defined concepts such as utility. As Marschak put 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.<sup>32</sup>

However, while the three-step procedure—i.e., 1.) how people should behave; 2) how people do behave; and 3) how can knowledge from 2) be used to formulate recommendations as in 1)—required the investigation of psychology, actually conducting empirical research was not of primary concern at Cowles. Empirical studies were undertaken at, or were made in cooperation with, other institutions, or they drew on surveys and on publicly available data. 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 the University of Illinois and thus effectively outsourced the empirical ‘step’ II to institutions and researchers outside Cowles.<sup>33</sup> Furthermore, to study deviations from the rational course of action, Marschak suggested analyses of balance sheets of insurance companies, portfolios of investment trusts, and price data made available by the research departments of firms, banks, or the government. And he expected that 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”.<sup>34</sup> 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”.<sup>35</sup> Thus, Marschak in fact paid only lip service to the psychologists in that investigations into actual behavior under uncertainty were neither undertaken at the commission nor in extensive collaboration with psychologists.

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<sup>32</sup> See ‘Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak’, 24 April, 1950, folder ‘uncertainty’, box 156, JMP; 3.

<sup>33</sup> Behavior that was tested by researchers at the Commission was for example inventory behavior of firms, import demand, financial behavior patterns, demand elasticities for imports, etc.

<sup>34</sup> See ‘Economics of Uncertainty: A Research Project – Draft of a Memorandum by J. Marschak’, 24 April, 1950, folder ‘uncertainty’, box 156, JMP; 6 f.

<sup>35</sup> *Ibid.*, 7.

Economists retained an ambiguous attitude towards other social scientific disciplines with respect to transdisciplinarity in approaching decision-making. Marschak repeatedly pointed 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.<sup>36</sup>

While this transdisciplinarity would eventually have to materialize in research, he thought that a mathematical theory of choice under risk would require more time. Theoretical research was still prioritized and emphasis was placed on an increased conceptual precision, which at an early stage required disciplined specialization. Many of the mathematical economists concerned with the theory of individual rational behavior under risk felt it was necessary to first develop a precise and general theory of rational behavior, and therefore did not actively engage with psychological research or undertake such studies at this stage.

When it came to studying actual behavior, or integrating psychological results into their models, Marschak and his colleagues also referred to feasibility, an aspect that mainly concerned the kind of data they were drawing upon. Economic data was comprised of aggregate data, gathered through index measures and calculated averages. Consumption and investment behavior for example was predicted on the basis of (aggregated) income data, data gathered on the basis of a price index, etc. The quantifiability of choice was thus another justification for narrowing the scope of applying rational choice theories to traditional economic behavior. Those preconditions as well as the discipline-specific problems they faced at Cowles during this period were an entirely different consideration than a psychologist or a behavioral scientist more generally would have had when thinking about a theory of human behavior.

Moscatti (2016a) documents how around 1950, not only Marschak and Leonard Savage but also Paul Samuelson became convinced of a normative interpretation of

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<sup>36</sup> Ibid., 3.

expected utility theory for prescribing the rational course of behavior in risky situations, changing its status once again. Their normative interpretation, however, did not aim at prescribing moral behavior, a concern at the time for example in welfare economics. Rather, it aimed at prescribing the rational choice in a particular situation by clearly determining how the relationship between the ends an agent aimed for and the means to achieve them should look like (see also Hands 2015). By telling the agent how to behave, normative expected utility theory targeted this relationship without any moral connotations. Marschak became a key protagonist in defending this normative interpretation (Marschak 1948, 1950, 1951), mainly when realizing that its status as an empirical theory of human behavior could not be justified. For some time, it was used in various contexts to provide guidelines and formulate recommendations for consumers, producers, and governments to reach the optimal solution of a decision problem (see also Erickson et al. 2013). As studies of actual behavior were in fact decreasing at Cowles, prescriptive studies began to dominate. While this ‘normative turn’ cannot be reconstructed here in detail, the important point here is that the status of rational choice theories and expected utility theory in particular did not fully stabilize in the 1940s and 1950s, and depended on its interpretation and epistemic role, which continued fluctuating in mathematical economics at the time.

## **5. Theories of Human Behavior Between the Logic of Choice and the Behavioral Sciences**

As the previous sections suggest, the problems tackled at Cowles in the late 1940s increasingly concerned basic research and from Marschak’s and Koopmans’ point of view did not immediately suggest an engagement with psychological research and a behaviorally more adequate theory of decision-making. Yet, some mathematical economists had the chance to actively engage with psychologists and the behavioral sciences more generally when in 1951 the Ford Foundation established the so-called ‘Behavioral Sciences Program’ in Pasadena, California. One of the major goals of the program was to promote what became referred to as ‘research in the behavioral sciences’ in the United States (e.g., Gaither et al. 1949, 94). In this context, serious efforts were made to support collaboration between economists and the behavioral

sciences to investigate in an interdisciplinary manner the different facets of human motivation, while retaining their commitment to mathematics.

The program 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 resulting so-called Gaither report, named after Rowan Gaither, who led the study committee that planned the activities at Ford, structured the activities of the foundation according to five program areas: “The Establishment of Peace” (Area I); “The Strengthening of Democracy” (Area II); “The Strengthening of the Economy” (Area III); “Education in a Democratic Society” (Area IV); and “Individual Behavior and Human Relations” (Area V) (Gaither et al. 1949). Those areas were the result of an extensive process of establishing the guidelines and further activities at Ford in line with its more general objectives of promoting human welfare (Berelson 1968, 42).

‘Program Area V’ focused on supporting scientific research in the behavioral sciences. It was, however, not meant as a normative program for developing some sort of action plan in order to guide the behavioral sciences in a specific direction (Berelson 1968). Rather, the self-proclaimed goal was “to increase knowledge of human behavior through basic scientific research oriented 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, 42). Thereby, 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 was 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 that influence and determine 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, 90 ff.).

More specifically, the goal of Program Area V was to investigate and elicit the fundamental and general principles that govern human conduct in various situations,



whether individual or social (Gaither et al. 1949, 90 ff.), and to thereby arrive at a “scientific understanding of why people behave as they do” (Berelson 1968, 42). In line with the general conviction of the fruitfulness of interdisciplinarity, 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 (ibid.). 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, 90). The scientific study of individual and group behavior was thus a central focus of the Ford Foundation in supporting the progress of the social and behavioral sciences.

The ‘Behavioral Sciences Program’ reflected a commitment to interdisciplinarity and to the use of mathematics to rigorously investigate human decision-making, a methodological strategy that could principally be thought of as being in line with the methodological commitments at Cowles. At the same time, as the Gaither report of the study group showed (Gaither et al. 1949), two Program Areas, the economics/business division (Area III) and the Behavioral Sciences Program (Area V), were largely influenced by an empirical approach to the social sciences.<sup>37</sup>

Berelson was fundamentally convinced of behavioral empiricism, a position strongly reflected in the direction the Behavioral Sciences Program would go in the near future.<sup>38</sup> While the final report emphasized ‘economic behavior’, it showed “the group’s focus on empirical research into actual behavior, in explicit contrast with neoclassical economists’ assumption of rationality” (Pooley/Solovey 2010, 223). Furthermore, the ultimate focus on empirical science strongly opposed the theoretical direction that Koopmans had followed at Cowles and the Bourbakian kind of axiomatics that had been adopted by people like Debreu (Weintraub/Mirowski

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<sup>37</sup> Neither ‘behavioral empiricism’ nor ‘behavioral sciences’ as Berelson and company understood the terms, is to be confused with behaviorism, denoting a specific school in psychology, or, more generally, with the idea of creating a unified discipline called ‘behavioral science’ (see also Berelson 1968).

<sup>38</sup> That Berelson strongly favored the “General Scientific Approach” to behavior becomes, for example, reflected in a shortened version of his *Human Behavior* originally written with Gary A. Steiner, in which Berelson stressed his naturalist position that, as in other sciences, the scientific method would be the most appropriate method for behavioral sciences (Berelson/Steiner 1967, 6 ff.).

1994).<sup>39</sup> Yet, Koopmans and Marschak nevertheless planned to apply for a grant from Program Area V, a step that – again – reflected their rather ambiguous attitude towards psychology and the behavioral sciences more generally for which correspondence began in 1951.<sup>40</sup>

That Koopmans had approached the Foundation through the Behavioral Sciences Program and not through Program Area III, the economics/business division, was most likely on the suggestion of Berelson, Gaither and Hans Speier, who had been head of RAND's Social Science Division since 1948 and who had become a member of the Study Committee.<sup>41</sup> Berelson and Gaither considered basic "research of [the] mathematical type" favorably.<sup>42</sup> They aimed to foster the integration of the social sciences, which they thought was "made possible precisely by the mathematical (logical) approach."<sup>43</sup> They also admired the international orientation of the Commission, and they focused on the cooperation of the scientists at Cowles who would become leaders in mathematical economics. Those aspects of the Cowles approach were thus appreciated in Program Area V.

In their application, Koopmans and Marschak had to bridge a gap between their own methodological and epistemological convictions as well as the general orientation of research done at the Cowles Commission and the Ford Foundation's general mission. The statement of purpose in their grant application 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.<sup>44</sup>

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<sup>39</sup> Mathematical economics of the Bourbakian kind was not popular in the economics profession in the early 1950s but "spread throughout American graduate education in economic theory" as the research of Cowles scholars influenced major economics departments (Weintraub 2002, 119).

<sup>40</sup> One important reason why the Cowles Commission applied for a grant from the Ford Foundation was to replace the 'military' contracts (RAND and ONR) with '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; 4.

<sup>41</sup> Koopmans to Marschak, 26 July 1951, folder 'The Ford Foundation', box 99, JMP.

<sup>42</sup> Marschak to Koopmans and W. B. Simpson, 21 August 1951, folder 'Ford Foundation', box 99, JMP. Simpson was the Executive Director of the Cowles Commission during that time.

<sup>43</sup> Ibid.

<sup>44</sup> Rockefeller Archive Center, Ford Foundation records, central index 1950-1974, master index cards - projects, (File A-324).

This statement appeared to be in line with the general course at Cowles. In Koopmans' own words it meant that "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".<sup>45</sup> Yet, the correspondence between Marschak and Koopmans reflected a going back and forth on their strategy for approaching the foundation and suggests that they had to modify the aims and activities at Cowles considerably to make their work fit into the mission of Area V. For example, a letter from Koopmans to Marschak, dated July 26, 1951, shows their need for an adjustment strategy towards interdisciplinarity and a broad research focus:

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.<sup>46</sup>

At the same time, it becomes evident that this openness towards interdisciplinarity would not entail working closely together with other social scientists, including psychologists. In a report of a meeting with Speier dated August 21, 1951, Marschak admitted that they would work with other behavioral economists only if the latter had the appropriate training.

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

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<sup>45</sup> Koopmans to Gaither, 14 September 1951, folder 'The Ford Foundation', box 99, JMP.

<sup>46</sup> Koopmans to Marschak, 26 July 1951, folder 'The Ford Foundation', box 99, JMP.

with economists, mathematicians or any other bright and imaginative people.<sup>47</sup>

That Speier's concerns about taking empirical work and psychology seriously in projects funded by the Ford Foundation were potentially decisive for the success of the commission's application led Marschak to reflect on how their abstract work could be linked to empirical interdisciplinary work of interest to the 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"<sup>48</sup>), Marschak highlighted how prioritizing empirical work would favorably present to the foundation a conception of economic behavior as something more than rational behavior, while factually holding on to the rationality principle and the mathematically formulated logic underlying 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.<sup>49</sup>

In the same report, it becomes equally clear, however, that promising to extend the scope of economics and a turn towards more empirically oriented work was again nothing more than Marschak paying lip service to Speier and the staff of the foundation:

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

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<sup>47</sup> 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; 2, italics in original.

<sup>48</sup> Ibid.

<sup>49</sup> Ibid.

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.<sup>50</sup>

The need to align their research proposal with the Ford Foundation's more general goal of establishing the behavioral sciences becomes even clearer in a draft of the application itself dated September 1951. Marschak and Koopmans stressed the "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 broad definition of economics as "a science of best choice from a given range of alternatives in the light of clearly defined values".<sup>51</sup> They promised that "this support [by the Ford Foundation would] 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".<sup>52</sup> Yet, since Berelson and his colleagues believed that mathematical methods would play a decisive role in studying human decision-making in all the behavioral sciences, their mathematical approach was not an aspect that would have taken Marschak and Koopmans away from the course pursued by Ford.

Marschak and Koopmans's grant proposal focused on the importance of studying 'economic behavior,' which they promised to study in an interdisciplinary fashion. Besides stressing the importance of realism and empirical research, Marschak adopted a Robbins-like view of the scope of economics that allowed him to present the work of the Commission as fundamentally already transdisciplinary and strongly focused on individual choice:

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 applies to much of social science generally, with other classes of alternatives and wider evaluation and objectives. In the light of this formal similarity, the

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<sup>50</sup> Ibid., 3.

<sup>51</sup> As shown by the preserved index card tracking the application process, 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).

<sup>52</sup> See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics' September 1951, folder 'The Ford Foundation', box 99, JMP; 13.

boundaries between the various social sciences become vague and unimportant.<sup>53</sup>

Because of their concern with the problem of human 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".<sup>54</sup> Marschak took the economic approach as just one among several to study the complex subject of human behavior. For him, then, "[t]he contribution of economic theory [to the study of behavior] is the formal treatment of optimization problems, while the substance of the models may be drawn from the entire range of social sciences".<sup>55</sup> The economic perspective could ultimately be expanded beyond the study of 'economic' behavior, shifting the direction of the commission further "towards greater generality and relevance to a wider class of social phenomena."<sup>56</sup> At the same time, Marschak recognized the importance of justifying the relevance of Cowles research and emphasized the commission's goal of making newly developed theoretical tools capable of generating knowledge of society and thus practically useful.<sup>57</sup> On the whole, the portrait of the commission painted in the grant proposal was fundamentally different from the course Marschak and company had taken until then. Yet, the scope of the proposal – from the beginning, the application was for a long-term period of 10 years – indicates that Koopmans and Marschak would have to be serious about their statements at least to a certain extent.<sup>58</sup>

It appears obvious that a factual turn towards a behavioral theory of human behavior would ultimately have implied a profound shift in the commission's original focus: a shift from econometrics and theoretical research on formal theories of human decision-making under uncertainty more towards the objectives of the Ford Foundation. For example, while the proposal refers to von Neumann and Morgenstern's *Theory of Games*, this reference is less enthusiastic than in Marschak's

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<sup>53</sup> Ibid., 7.

<sup>54</sup> Ibid.

<sup>55</sup> Ibid., 8.

<sup>56</sup> Ibid., 14.

<sup>57</sup> Ibid., 7.

<sup>58</sup> Rockefeller Archive Center, Ford Foundation Records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324).

review of the *Theory of Games* that he had written in the 1946, stressing how its realism could be improved by formalizing the limitations of intellectual and information-handling processes of participants,<sup>59</sup> a dimension of research that would require psychological justification. Highlighting the gap between formal theories of decision-making and actual psychological research and actually pursuing collaboration in a serious way might have been one way for Marschak to adapt the Cowles program to the objectives of the foundation.<sup>60</sup>

Yet, while one focus of the commission's research program included the empirical study of actual economic behavior and an increasing relevance of their research,<sup>61</sup> the proposed formal-axiomatic approach to studying individual behavior at Cowles still differed profoundly from other 'empirical sciences' interested in the nature of behavior and the approach that ultimately motivated the behavioral sciences program. For Marschak and Koopmans, exemplary cases for applying mathematical methods to decision-making in the proposal were still Kenneth Arrow's *Social Choice and Individual Values* (1951), John von Neumann and Oskar Morgenstern's game theory, John Nash's work on bargaining, and Norbert Wiener's work on cybernetics. They held the view that those works introduced rigor, method, and clarity into the social sciences that their discipline needed to become scientific.<sup>62</sup>

Furthermore, besides justifying expected utility theory as a normative theory, the apparent interest in empirical research by Marschak and Koopmans covered up their continuing concern for foundational work. One of the motivations expressed in the application, which they took to be shared with "many other empirical sciences", was the curiosity "to explore and understand the physical, technological, human, and social environment in which life is lived".<sup>63</sup> However, 'empirical' did not only mean to investigate human motivation and individual behavior on the basis of questionnaires, experiments, and interviews, but primarily involved "theoretical studies of the implications of assumptions made for purposes of empirical analysis".<sup>64</sup>

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<sup>59</sup> Ibid., 15.

<sup>60</sup> Note that in the middle of the 1950s, Marschak began to actively collaborate with psychologists.

<sup>61</sup> See Cowles Commission for Research in Economics (1952).

<sup>62</sup> See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics' September 1951, folder 'The Ford Foundation', box 99, JMP; 5.

<sup>63</sup> Ibid.

<sup>64</sup> See 'Draft – Application to the Ford Foundation by the Cowles Commission for Research in Economics' September 1951, folder 'The Ford Foundation', box 99, JMP.

Instances of such studies were Lawrence 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).<sup>65</sup> The latter allowed for shedding light on the empirical background of theoretical frameworks such as theories of decision-making under uncertainty and accordingly allowed for their potential modification. Yet, those works were not comparable to actual empirical research on individual decision-making undertaken for instance in experimental psychology.

The adjustment of the Cowles Commission's research program to the explicit goals of the Ford Foundation would have far-reaching implications for the "realistic and normative" studies on theories of "economic" decision-making at Cowles.<sup>66</sup> That this adjustment 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 grant was rejected on January 14, 1954.<sup>67</sup> The official justification was that too many demands on the Foundation's resources limited the range of the program.<sup>68</sup> But its procedure reveals the way in which mathematical economists, while committed to their mathematical theories of rational choice to adequately address the conceptual and foundational problems they were concerned with, revealed a rather ambiguous attitude towards the behavioral sciences.

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<sup>65</sup> Ibid. Examples for markets that became analyzed were demand and supply for livestock products (C. Hildreth) and markets with forward trading such as corn, wheat, and cotton market to study speculation (H. S. Houthakker), among others. The analysis of input-output ratios stimulated by Wassily 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 who had appointments at other academic centers (e.g. Modigliani at the University of Illinois undertook a project on expectations and business fluctuations); see Cowles Commission for Research in Economics (1952).

<sup>66</sup> Ibid., 1.

<sup>67</sup> Rockefeller Archive Center, Ford Foundation Records, Central Index 1950-1974, Master Index Cards - Projects, (File A-324).

<sup>68</sup> Marschak to Koopmans, 18 August 1952, folder 'The Ford Foundation', box 99, JMP. Other reasons were that the project was located at the borderline of economics and behavioral sciences, and the activities would over time possibly overlap with a new economic activities division that was about to be set up under Carroll's leadership. See 'Report [presumably by Koopmans] on Conversation with Berelson, New York office of Ford Foundation', 20 October 1952, folder 'The Ford Foundation', box 99, JMP, 2; see also 'Report of Conversation between Berelson and Clifford Hildreth', 17 February, 1953, folder 'unlabeled', box 99, JMP.



## 6. Setting Priorities: Turning Economics Into a Science

In their analysis of why the Ford Foundation's efforts to foster cooperation between the behavioral sciences and economics failed, Pooley and Solovey (2010, 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 [Ford's Behavioral Sciences Program's] overture reflected and reinforced the growing postwar hegemony of the neoclassical approach" (ibid, 230). There are several reasons why this history between economics and the "behavioral sciences movement" is more complicated than Pooley and Solovey's narrative suggests (see, e.g., Cherrier 2013), and one is surely the highly ambiguous attitude of this group of mathematical economists towards research from psychology and other behavioral sciences in the late 1940s and early 1950s. Economists did not categorically reject the idea that psychological research might have importance for economic theory. But they still considered their priority to be basic research, requiring the maintenance of disciplinary boundaries.

The continuing interaction between Marschak and the Ford Foundation was only one case in point. 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".<sup>69</sup> The group 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".<sup>70</sup> Gaither and Berelson clearly considered economics to be a behavioral science. And already in July 1952, Thomas Carroll, Gaither's cousin and head of Project Area III (the economics/business division), contacted Marschak requesting

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<sup>69</sup> Carroll to Marschak, 16 July 1952, folder 'unlabeled', box 99, JMP.

<sup>70</sup> Ibid.

support in identifying ways for potential cooperation between economics and the behavioral sciences.<sup>71</sup>

The advisory group envisaged directly fostering a mutually beneficial relationship between the two areas, mainly with a focus on their approaches to human behavior. In an attached memorandum of a first meeting in May 1952, the group had considered cooperation as “highly desirable” because it would prove “valuable both in the development of new theories about 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”.<sup>72</sup> The advisory group traced the origins of 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 the discipline of modern economics.<sup>73</sup> The idea behind the advisory group was that the “historical separation” between economics and the behavioral sciences should be dissolved.<sup>74</sup>

The problems the advisory group envisioned for cooperation across disciplines clearly focused on the study of individual or group behavior.<sup>75</sup> Economics was supposed to benefit by empirically enriched behavioral foundations. As noted in the memorandum, projects would be supported with the ultimate “aim of ... [applying] to

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<sup>71</sup> Individuals who 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), and Joseph Spengler (Department of Economics, Duke University). Representatives of the Ford Foundation were Berelson, Richard Bissell, and David McClelland.

<sup>72</sup> See memorandum entitled ‘The Ford Foundation – Behavioral Sciences Division, Advisory Group on Economics and the Behavioral Sciences’, July 1952, folder ‘unlabeled’, box 99, JMP; 1.

<sup>73</sup> Ibid., 2.

<sup>74</sup> Ibid.

<sup>75</sup> Besides investigating such things as the “political decisions of farmers” or the “investment decisions of firms” and developing 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.*, 5.

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”.<sup>76</sup> Obvious to the group was that interdisciplinary collaboration might be exacerbated by limited resources, by the absence of facilities and appreciation of interdisciplinary work, by semantic difficulties or distinct conceptual terminology 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”.<sup>77</sup> Yet, this did not diminish their hope for developing strategies that would foster collaboration and at the same time avoid those difficulties.

That the content of this memorandum indicated a shift toward the study of actual behavior 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.<sup>78</sup> The general tenor was as follows: “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”.<sup>79</sup> Marschak was generally 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.

Besides Marschak, other leading economists signaled their openness to the foundation’s project. Among them were Morgenstern, Armen Alchian, and Frederick Mosteller; Herbert Simon himself was part of the advisory group. When this group met in July 1952 at RAND to discuss Carroll’s memorandum, Marschak also saw “the common ground among the fields” as lying “in the area of the theory of choice”.<sup>80</sup> According to Marschak, economists could “bring to this problem their well-developed

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<sup>76</sup> Ibid.

<sup>77</sup> Ibid.

<sup>78</sup> Ibid., 6.

<sup>79</sup> Ibid.

<sup>80</sup> Simon to Carroll, 1 August 1952, folder ‘unlabeled’, box 99, JMP.

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”.<sup>81</sup> Marschak was more and more convinced that studying individual behavior as a major scientific objective should become one of the key research areas of this undertaking. In correspondence with Berelson and Carroll, he began to insist that “[e]conomics is itself a behavioral science.”<sup>82</sup> Yet, both institutions held diverging views on the kind of mathematics to be used in the behavioral sciences. Berelson compared the work at Cowles with Nicolas Rashevsky’s mathematical biology; Rashevsky, in Berelson’s view, was “playing with mathematical models”, thereby exhibiting a “disregard for the specific content of a social problem” and “putting the form before the content”—practices of which he was skeptical.<sup>83</sup> This comparison was telling, as Rashevsky’s work was doubtlessly received as highly mathematical and abstract, especially because the first principles he formulated for biology were little, if at all, based upon any empirical findings (Gibson 1952).

Marschak, however, defended the kind of mathematics that some mathematical psychologists were using, such as for example that of C. H. Coombs (mathematical psychology) and R.M. Thrall (mathematics) at the famous University of Michigan seminar.

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

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<sup>81</sup> Ibid.

<sup>82</sup> Marschak to Carroll, 5 August 1952, folder ‘unlabeled’, box 99, JMP.

<sup>83</sup> Rashevsky, a theoretical physicist, had worked on problems in mathematical biology. 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, Rashevsky had just published two books that tackled the link between mathematical biology and social behavior. He had attempted to apply the approach of his mathematical biology to social phenomena, which, in his first book, was deduced from purely formal postulates. In his second book, he attempted to support those postulates and their consequences by neurobiological concepts. 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, 144).

from closer cooperation with ‘content’ specialists – such as sociologists or biologists, similarly to what H. Simon is doing.<sup>84</sup>

This last remark, however, reveals Marschak’s increasing tendency towards supporting empirically meaningful work that would later become more apparent in his views and his research: his urge to address economic problems by what he considered the most appropriate tools and not just continue using mathematical tools in such a way as von Neumann had rejected as *l’art pour l’art*. And in dismissing in the same letter the standard psychology books of C. L. Hull and G. A. Miller on behavioral psychology as “very clumsy, vague and tiresome”,<sup>85</sup> he thereby referred to the way in which mathematics had been used by social scientists, when no ‘real’ mathematician was present. Only a social scientist appropriately trained in mathematics and able to use this training in a qualified way would be accepted by Marschak for further interdisciplinary collaboration.

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 abandon for example their usual neglect of the probabilistic character of human action, already a commonplace for experimental psychologists.<sup>86</sup> As such, it was not in the “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; the benefit was in “in setting up a model (a hypothesis) and in designing an empirical test of it, by experiment or otherwise”, and sharing a commitment to the scientific method and to the use of mathematics, statistics and logic as the common language.<sup>87</sup> For Marschak, the distinctiveness of the behavioral sciences made the use of the “Esperanto of mathematical and logical symbolisms ... all the more effective”.<sup>88</sup> Problems surrounding uncontrolled data and measurement

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<sup>84</sup> Marschak to Koopmans, 18 August 1952, folder ‘The Ford Foundation’, box 99, JMP; italics in original.

<sup>85</sup> Ibid.

<sup>86</sup> Ibid., 1 ff.

<sup>87</sup> 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; 6.

<sup>88</sup> Ibid., 3.

procedures as well as the uncertainty characterizing the social world were aspects that the behavioral sciences would have to cope with and that required the precision, rigor and neutrality that only mathematics could offer. But the concern for foundational issues already prevalent in von Neumann and Morgenstern remained dominant among economists.

To inquire into how best to improve the relationship between economics and the behavioral sciences, in July 1952 the advisory group had asked a number of representative scholars in their field to offer their opinion on that matter.<sup>89</sup> A digest of replies on the basis of 55 scholars showed that the reaction of the economists involved, among them Milton Friedman, Kenneth Boulding, Marschak, Lawrence Klein, Simon Kuznets, Abba Lerner, Harold Hotelling, Theodore Schultz, Morgenstern, Mosteller, Herbert Simon, and Modigliani, while generally sympathetic towards the proposal,<sup>90</sup> was skepticism. Getting down to concrete ways in which such a proposal could be realized raised doubts. The economists stressed the priority and the focus of research on economic behavior, which would be quantifiable and applicable to economic data. While they acknowledged that interdisciplinarity would be important in the social sciences, disciplinary specialization would first be needed to increase the scientific status of economics. They considered collaboration between disciplines desirable but rejected interdisciplinarity for its own sake. Rather, they argued for problem-oriented research and for collaboration that emerged naturally if problems were shared.

The economists stressed that empirical research was important. Yet, they argued that the development of formal techniques and conceptual precision should still be prioritized, given the current stage in the social sciences.<sup>91</sup> And while they agreed that rational choice theories have their limitations, they thought them useful mainly as a mathematical account of human agency, as normative theories, and as allowing for basic research in a discipline whose first goal should be to become a science. Albeit the behavioral sciences movement promised a mathematical yet psychologically more

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<sup>89</sup> 18 economists, 10 scholars of business administration, 8 representative of research centers and projects, 7 sociologists, 3 political scientists, 2 economic historians, 2 psychologists, one agricultural economist, a historian, and a mathematical statistician, a university president and a university chancellor responded.

<sup>90</sup> 'Digest of Replies to Letter Concerning Report of Advisory Group on Economics and the Behavioral Sciences', folder 'unlabeled', box 99, JMP; 27.

<sup>91</sup> Ibid.

realistic theory of human behavior, a goal that the economists did not categorically reject, the latter believed that the first order of business for a discipline was to precisely describe discipline-specific problems and formulate discipline-specific concepts; any collaboration and interdisciplinarity should grow naturally from problem-oriented research if needed. The economists favored rational choice theories that allowed for the use of conventional (average) data on incomes, consumption, production inputs and outputs, etc.; a focus on basic research; the development of precise concepts and theories; and the possibility for quantification. All those took priority over interdisciplinarity and a more complex theory of human behavior; otherwise, economists might get distracted before they could shape their discipline towards becoming science they thought it should. Key criticisms of expected utility theory such as the Allais paradox (Allais 1953) further fostered the theory's temporary normative interpretation, whose dominance, however, did not persist for long.

During the late 1950s and the 1960s, while officially remaining within the economics profession, Marschak increasingly focused on issues that he took to be concerns of all the behavioral sciences. He began to collaborate with psychologists and philosophers on empirical testing of probabilistic theories of choice (Davidson/Marschak 1959, Becker/DeGroot/Marschak 1963, 1964) and increasingly considered interdisciplinarity in economics as a virtue rather than a vice. And while he considered economics at first as a science based upon the 'pure logic of choice', he subsequently became convinced that economics was a behavioral science that ought to acknowledge findings from other disciplines. Yet, Marschak never really abandoned his image of science and the need for mathematical tools, both of which he inherited from his years at the Cowles Commission in the 1940s and 1950s. Rather, having been a mathematical economist and econometrician, Marschak's professional journey reveals an ongoing tension that partly originated in his attempt to use mathematical methods on the one hand and in his interest in psychological insights on the other hand. By attempting to bridge the gap between the highly formal theory of rational action that allowed for making economics more 'scientific' and psychologically more sophisticated conceptions of human behavior that were being developed in psychology and the behavioral sciences, his attitude towards psychology until the middle of the 1950s reveals, to a certain extent, an ambiguity that

characterized much of the way in which mathematical economists thought about psychology at the time.

## **7. Conclusion**

The major aim of this paper was to better understand whether and if so why mathematical economists favored formal-axiomatic theories of rational choice over behavioral choice theories in the early Cold War period. In tracing the justifications given by mathematical economists at the Cowles Commission, it has been shown that rational choice theories emerged within the context of diverging roles that mathematical economists working with them expected them to play. While particularly von Neumann did not consider his and Morgenstern's axiomatic version of the expected utility principle as their core contribution to economics, Marschak and other mathematical economists at the Cowles Commission considered such axiomatic choice theories as a necessary first step towards increasing the scientific status of economics. Those mathematical economists aimed at developing the discipline of economics according to an image of science that was nurtured by the scientific method but also by a strong philosophical position on the role of formalism inspired by mathematics. As von Neumann and Morgenstern's behavioral axioms had left room for interpretation, their axiomatic version of the expected utility principle was perceived as not only offering a mathematically precise and approximately accurate description of economic decision-problems under risk; mathematical theories of rational behavior, such as expected utility theory, were also judged as having the potential to improve the formulation of behavioral hypotheses and the estimation of structural equations in order to understand through econometric models the macro-level behavior of an economic system. Largely under Koopmans' reorientation of the commission's research focus, rational choice under uncertainty was studied theoretically and – in the case of expected utility theory - justified by Marschak and others as a normative theory that investors, consumers, and producers could use to make rational decisions.

Given the various roles that rational choice theories were expected to play, not all mathematical economists categorically rejected psychological results but rather displayed an ambiguous attitude towards research on human behavior undertaken in psychology and other behavioral sciences. While they did not make a serious effort to



integrate psychological findings into their formulation of behavioral principles until the early 1950s, some mathematical economists such as Marschak repeatedly referred to the need to ultimately draw upon psychological research to justify their behavioral principles. Furthermore, some admitted that economics would eventually have to study *actual* behavior to explain and predict individual decision-making. Several representatives of the profession in principle agreed that economics should eventually be considered a behavioral science, and even wanted to fuse the axiomatic method with behavioral choice theories to bridge the gap between the various roles those theories played and the tension he faced. As such, the separation between mathematical economics and other behavioral sciences was not as clear-cut as often argued.

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