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The Experiment in the History of Economics

Edited by Philippe Fontaine and Robert Leonard

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The Experiment in the History of Economics

Throughout the evolution of economic ideas, it has often been asserted that experimentation in economics is impossible. Yet, the historical record shows that the idea of "experimentation" has, in fact, been important in the field of economics, and has been interpreted and put to use in many ways. These range from the "thought experiment", where counterfactuals are explored in the mind of the theorist, to social experiments, where alternative economic arrangements have been tried out historically, and laboratory experimentation, which is currently a burgeoning field of empirical research.

This book provides testament to the great variety of ways in which experimentation has mattered in the creation of economic knowledge. The accessible essays contained within this volume will interest all those seeking to broaden their historical understanding of the discipline and will be essential reading for students who wish to acquire a greater knowledge of how economics has grown and developed.

Philippe Fontaine is a historian of postwar social science. He has published in various journals, such as *Economics and Philosophy*, *History of Political Economy*, and *Isis*, and is co-editor with Albert Jolink of *Historical Perspectives on Macroeconomics* (Routledge, 1998). **Robert Leonard** is a historian of game theory and contemporary economics. His work has appeared in various journals in economics and in the history of science, and he is author of the forthcoming *From Red Vienna to Santa Monica: von Neumann, Morgenstern and the Creation of Game Theory, 1900–1960* (Cambridge University Press).

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4 Thought- and performed experiments in Hayek and Morgenstern

Alessandro Innocenti and Carlo Zappia

> The necessary consequence of the reason why we use competition is that, *in those cases in which it is interesting*, the validity of the theory can never be proved empirically. We can test it on conceptual models, and we might conceivably test it in artificially created real situations where the facts which competition is intended to discover are already known to the observer. But in such cases it is of no practical value, so that to carry out the experiment would hardly be worth the expense ... The peculiarity of competition – which it has in common with the scientific method – is that its performance cannot be tested in particular instances where it is significant, but is shown only by the fact that the market will prevail in comparison with any alternative arrangements.
>
> (Friedrich A. Hayek 1968, p. 180)

> ...experiments are designed to enable us to predict outcomes under controlled conditions and to make it possible to conclude from those to wider applications. There are, of course, limits to experiments in economics, but in a sense any variation in taxation, in foreign exchange rates, in tariffs, etc. etc., can be viewed as a coarse experiment whose result can lead to new theories and hence to better prediction when the new occasion comes around.
>
> (Oskar Morgenstern 1972, p. 710)

4.1 Introduction

It has been argued that the emergence and early progress of experimental economics are related to the work of Oskar Morgenstern (Schotter 1992). Although he did not publish any experimental findings, Morgenstern became involved in the development of experiments in economics in at least two ways. First, most of his contributions to economics, from the 1928 volume on economic prediction to the 1970 work on the predictability of stock prices, were devoted to various aspects of empirical research, including the promotion of both econometrics and statistical analysis. Second, in conjunction with John von Neumann, he created game theory, whose way of modeling the process of decision-making lent itself to exper-

imental applications. Morgenstern was an unconventional kind of thinker, tending to anticipate "unexpected" turns in the evolution of mainstream science, rather than contribute to its systematization. And experimental economics has, indeed, been viewed as a development that has initiated an important thread in the criticism of neoclassical economics, the full implications of which are still to be drawn (Smith 1989).

However, curiously, Morgenstern's early contributions as an economist were characterized by the view that economics as a science was mainly deductive, in the sense associated with the Austrian construction of the thought experiment (Moss 1997).[1] The "method of imaginary construction," or "thought experiment," typical of the Austrian tradition, can be seen as a method to help economists avoid facing complexity in the initial steps of analysis. Morgenstern (1954, p. 484) describes thought experiments as the method of "imagining conditions that differ from the known conditions and then attempting to identify the proper factor to which the imagined variations could be ascribed." The method proves valuable mainly when the role of a single important element of a complex system is to be ascertained, for example the role of the alert entrepreneur in the Austrian theory of the competitive market.[2] The method was intended by Austrian economists as an instrument for drawing logical lessons with empirical content from imaginary constructions. In Morgenstern's (1954, p. 484) words, "this procedure consists in the drawing of implications and like other experiments may lead to the discovery of new facts." For instance, Friedrich A. Hayek considered general equilibrium theory as a thought experiment aimed at identifying the causes of intertemporal discoordination (Hayek 1941, p. 26).[3]

This essay argues that a comparative analysis of Hayek's and Morgenstern's contributions can suggest why methods as different as thought- and performed experiments have been advocated in economics. In the mid-1930s, the two Austrian economists shared the view that the empirical content of economics was to be sought outside equilibrium theory, by applying the deductive method. In the 1950s, Morgenstern's renewed call for empirical analysis actually signaled a shift towards an inductive approach, endorsing the methodological value of direct, performed experiments, in contrast to Hayek's persistent reliance on a deductive approach to empirical analysis, and on the thought experiment as its main instrument. Essentially, we maintain that they held a common methodological stance in the 1930s, but Morgenstern later embraced an inductive approach while Hayek continued to reject it.[4] It is worth specifying that, in stressing the contrast between Hayek and Morgenstern as exponents of diverging methodologies, our assessment of their respective positions inevitably neglects other differences between the two authors.[5]

We shall consider Morgenstern's definition of a thought experiment, quoted above, as a point of reference. It is certainly a clear-cut, but also quite restrictive, definition. Morgenstern, it seems, had in mind qualitative

results concerning the movements of endogenous variables with respect to controlled variations in data, something which might resemble an exercise in comparative statics in a formal model. It is true that the methodology of thought experiment has been applied by Austrian economists not only to comparative statics but also to comparisons between the competitive market and imaginary alternative frameworks.[6] However, to contrast the routes taken by the two Austrian economists, we have found it preferable to embrace Morgenstern's definition.

The essay is structured as follows. Section 4.2 argues that a specific episode in the middle of the 1930s, namely a quite subterranean discussion about the assumption of perfect foresight in economics, was indicative of the two writers' growing dissatisfaction with the concept of equilibrium. The outgrowth of the exchange between Hayek and Morgenstern is interpreted as a plea, shared by both, for empirical analysis. Section 4.3 contends that, in spite of the deductive approach shared by the two economists, their respective articles on perfect foresight (Morgenstern 1935, and Hayek 1937) laid the basis for future disagreement on experiments. Section 4.4 deals mainly with Morgenstern's later turn towards an inductive approach and his recommendation of experimental investigation in economics, as opposed to the canonical use of thought experiments. There follows an examination of the intellectual process leading to Morgenstern's position, as stated in the passage quoted at the beginning of the essay. Section 4.5 considers Hayek's reliance on a deductive approach in his subsequent analysis of the market process and of competition as a discovery procedure. The rationale of Hayek's faith in the method of thought experiment in the 1960s, with specific regard to his contention that complex phenomena cannot be dealt with by means of empirical analysis in the form of performed experiments, is briefly investigated.

4.2 Perfect foresight and thought experiments in the 1930s

In the 1920s and 1930s, the role of time in economic theory was controversial, especially in the Austrian circles of Ludwig von Mises and Hans Mayer. These were two distinct groups of young economists, including Hayek and Morgenstern respectively.[7] Both groups were aware that a better understanding of the way individuals plan their actions in an intertemporal setting was required if the shortcomings of stationary equilibrium were to be overcome. In Lachmann's (1990) words, the economists working on the Austrian variant of the neoclassical approach in the 1930s, after dealing with the subjectivity of values, were striving to incorporate the subjectivity of expectations into economic theory.

Despite the common origin of Hayek's and Morgenstern's efforts to make room for expectations by incorporating a time dimension, their investigations produced conflicting results. Hayek, who is hailed as the first economist to introduce the notion of intertemporal equilibrium

(see Milgate 1979, who refers to Hayek 1928), made his first attempt at explicitly incorporating expectations in equilibrium theory in his 1933 Copenhagen Lecture on "Price Expectations, Monetary Disturbances and Malinvestment" (published in German in 1935). From the very beginning of this essay, he was at pains to define the conditions for expectational equilibrium:

> It has become clear that, instead of completely disregarding the time element, we must make very definitive assumptions about the attitude of persons toward the future. The assumptions of this kind which are implied in the concept of equilibrium are essentially that everybody foresees the future correctly and that this foresight includes not only the changes in objective data but also the behaviour of all other people with whom he expects to perform economic transactions.
> (1935, pp. 139–140)[8]

This passage struck Morgenstern, who, in his essay "Perfect Foresight and Economic Equilibrium" (1935, p. 171), considered it as an exemplar of the erroneous opinion, prevailing among those dealing with the time element, that "the theoretical perfection of equilibrium could not be obtained without the assumption of complete foresight by the economic subjects and the entrepreneurs."[9] Engaged as he was in reformulating equilibrium through the application of time and expectations (see section 4.3 below), Morgenstern argued that, despite being generally viewed as a prerequisite for equilibrium over time, the meaning of "full," "perfect" or "correct" foresight was far from obvious.[10] Morgenstern's statement of what perfect foresight means in equilibrium theory is worth quoting:

> The individual exercising foresight must thus not only know exactly the influence of his own transactions on prices but also the influence of every other individual, and of his future behaviour on that of the others, especially of those relevant for him personally.... The impossibly high claims which are attributed to the intellectual efficiency of the economic subjects immediately indicate that there are included in this equilibrium system not ordinary men, but rather, at least to one another, exactly equal demi-gods, in case the claim of complete foresight is fulfilled ... If "full" or "perfect" foresight is to provide the basis of the theory of equilibrium in the strict specified sense ... then a completely meaningless assumption is considered. If limitations are introduced in such a way that the perfection of foresight is not reached, then these *limitations are to be stated very precisely*.
> (Morgenstern 1935, p. 173)[11]

Here, Morgenstern is not simply questioning the realism of perfect foresight; he is also interested in examining "somewhat closely the conditions which result if full foresight is posited and *especially if there results reciprocal inclusion of foresight about the probable behaviour of others*." Morgenstern seems to accept that the hypothesis of perfect foresight can be a useful tool for analyzing the behavior of an individual taken in isolation. But once the "Robinson Crusoe economy" is left aside and the interactive aspects of economic action are considered, Morgenstern strikes a skeptical note:

> The fact is that a calculation of the effects of one's own future behaviour always rests on the expected future behaviour of others, and *vice versa. This can be observed empirically every time.* However, the chain of surmised mutual 'reactions' breaks off comparatively soon; often too, they play no excessive role because of the power of external data of a physical nature. This may be the case on certain markets, for example as the stock-exchange. With unlimited foresight, it is something else.
> (1935, p. 173)

At this point, Morgenstern (1935, pp. 173–174) takes up the paradox of Holmes and Moriarty[12] featuring in a previous work of his. The paradox is an argument in support of the view that "an endless chain of reciprocally conjectural reactions and counter-reactions ... can never be broken by an act of knowledge but always only through an arbitrary act – a resolution." It follows that "unlimited foresight and economic equilibrium are thus irreconcilable with one another." Morgenstern's use of the paradox clearly evokes the methodology of thought experiment. The analysis of Holmes's chain of deductions is intended to show that the "empirical" information obtained from a mental, not a performed, experiment allows the theorist to rule out certain theoretical conjectures, which, in this instance, concern how equilibrium is to be defined in an interpersonal decision problem over time. The unfolding paradox is essential for Morgenstern's claim that it is logically inconsistent to attempt to solve the problem of strategic interaction over time by assuming perfect foresight.

To move on to how Hayek reacted to Morgenstern's allegations, the relevant text is "Economics and Knowledge." Hayek presents this article as an attempt to make equilibrium analysis applicable to intertemporal price relationships. After giving the celebrated definition of equilibrium as a state of coordination of individual plans,[13] Hayek (1937, p. 42) comments: "These considerations seem to throw considerable light on the relationship between equilibrium and foresight, which has been somewhat hotly debated in recent times." The footnote reference is to Morgenstern's 1935 paper. It is arguable, therefore, that the discussion on the meaning of correct foresight which follows is, at least in part, an answer to Morgenstern:

> Correct foresight is then not, as it has sometimes been understood, a precondition that must exist in order that equilibrium may be arrived at. It is rather the defining character of a state of equilibrium ... M

> need foresight for this purpose be perfect in the sense that it need extend into the indefinite future or that everybody must foresee everything correctly. We should rather say that equilibrium will last so long as the anticipations prove correct and that they need to be correct only on those points, which are relevant for the decisions of individuals. But on this question of what is relevant foresight or knowledge, more later.
>
> (Hayek 1937, p. 42)

As regards Hayek's partial revision of the relationship between equilibrium and foresight, announced in the 1939 English reprint of the 1933 lecture, two points of clarification are brought forward. On the one hand, correct foresight is now precisely viewed as neither an assumption nor a pre-condition for equilibrium, but as the defining characteristic of equilibrium itself. On the other hand, correct foresight is not equivalent to perfect foresight, for the requirements for correct foresight are, in a sense, less strict. Both these arguments implicitly refer to the questions raised by Morgenstern. In particular, the second point hints at the limited cognitive and computational capabilities of individuals; hence "demi-gods" are not necessarily involved. This clarification can be interpreted as entailing that, *contra* Morgenstern, accuracy in the anticipation of all future events is not required. It is only necessary to anticipate the events that could imperil the validity of the theories of the workings of the economy held by individuals.[14] However, Hayek did not endorse Morgenstern's point concerning the logical inconsistency of equilibrium with perfect foresight. Hayek's revision of the notion of equilibrium shows how the "tautological propositions of pure equilibrium analysis" regarding the actions of a single individual could consistently be applied to the explanation of economic interrelations.

Hayek also refers to Morgenstern's paper in the 1946 essay "The Meaning of Competition." He states that "complete knowledge of the relevant factors on the part of all participants in the market" is the most "critical and obscure" condition assumed by the theory of perfect competition. On this, Hayek concludes with a remark on Morgenstern's point which one cannot find in the 1937 paper: "I shall here not go into the familiar paradox of the paralysing effect really perfect knowledge and foresight would have on all action." It is apparent that Hayek is maintaining that the logical problems associated with the notion of equilibrium with "really" perfect foresight do not concern his own 1937 notion of equilibrium with correct foresight. In particular, he continued to use equilibrium with "correct" foresight as a useful logical instrument in *The Pure Theory of Capital*, that is, in his last attempt to formulate the model of real economy on which his theory of the trade cycle was based (Hayek 1941).

Although with different emphases, in their essays of the mid-1930s both Hayek and Morgenstern, after dealing with the notion of equilibrium over

time, explicitly addressed the question of the empirical content of economic theory. They shared the view that this empirical content was to be sought outside equilibrium theory. In fact, they seem to outline a two-step procedure to develop economics. The first step of the procedure aimed to make economics an exact science: the typical neoclassical practice of using, in Morgenstern's (1935, p. 169) words, "neither exact nor complete statements about the assumptions underlying the theory of general equilibrium" should be replaced with a method of reasoning, such as the axiomatic method, which allowed for an exact recognition of all the implications of a given set of propositions, as well as an exact derivation of theorems from propositions. As has been noted (Leonard 1995, p. 313), the 1935 essay signals that, under the influence of a new mentor, Karl Menger, Morgenstern was eventually escaping the influence of Mayer, who, following the Austrian tradition of Carl Menger, firmly opposed the use of mathematics in economics.[15] Even if, unlike Morgenstern, he did not contribute directly to this development, Hayek (1937, p. 35) pointed to the same issue when he explained that

> my criticism of the recent tendencies to make economic theory more and more formal is not that they have gone too far but they have not yet carried far enough to complete the isolation of this branch of logic and to restore to its rightful place the investigation of the causal processes, using formal economic theory as a tool in the same way as mathematics.

The second step of the procedure aimed to specify the empirical content of economics. However brilliant the development of equilibrium theory by means of the axiomatic method, the process through which equilibrium can be obtained in actual economies remains to be investigated; and this task necessarily entails an examination of what foresight and knowledge mean for individuals acting in actual economies (see in particular Morgenstern 1935, p. 178, and Hayek 1937, p. 46).[16]

In the 1930s, however, Hayek and Morgenstern differed with respect to the way in which this empirical content was to be investigated. A first point to note is that both authors were deductive in approach. In particular, they were trying to deal with the assumption of incomplete knowledge in actual economies by applying mental chains of deductions and conjectures. That is to say, both were thinking in terms of thought experiments: they altered one relevant variable at a time and then speculated on the effects on the economy. But they assumed different starting points for their thought experiments. On the one hand, Hayek relied on his own definition of equilibrium as a state of plan coordination over time, a reliance which made the exercise sensible, in a way not unlike comparative static analysis. On the other hand, Morgenstern was not convinced at all that a plausible notion of equilibrium with foresight could be devised; thus he

showed limited confidence in the mental deductions he was drawing from the assumption of incomplete knowledge.

4.3 In search of a role for empirical analysis in economics

Although Morgenstern's 1935 essay is usually referred to for its critical part, it features a constructive part as well. Morgenstern is aware that, if equilibrium theory is to be preserved, an alternative formal structure must be devised to deal with the intricate relationships between economic phenomena involving interpersonal decisions.[17] As will be substantiated below, *Theory of Games and Economic Behavior*, to which Morgenstern contributed mainly through his criticism of neoclassical economics in the first chapter, can be interpreted as the chief outcome of Morgenstern's search for "exactness" in the analysis of market interactions among individual agents. However, the 1935 essay contains no substantive hint at the formal representation emerging later. Here Morgenstern (1935, pp. 175–179) devises an informal model in which he assumes, first, individual agents endowed with different degrees of knowledge. These agents are interested in subsets of the environment and constantly adjust their "opinion" about the environment, until "there is no longer any improvement in the sense of constant welfare." Second, Morgenstern postulates the existence of highly knowledgeable individuals endowed with "purely theoretical knowledge of [economic] relationships," who are able to evaluate the "overall" consequences of their behaviour. But he fails to explain how this purely theoretical knowledge differs from perfect foresight.[18]

Although Morgenstern (1935, p. 175) states his intention to investigate the importance of foresight and knowledge for actual economies, he brings forward only vague suggestions.[19] Individuals endowed with a certain "degree of foresight," which is based on varying amounts of insight into economic relationships, usually revise expectations in response to environmental changes. This means that expectations should be viewed as data of the economy and that, as for other data, comparative statics are necessary to understand their role in actual economies. Thus, Morgenstern was groping for a research strategy that ultimately resembles an exercise in comparative statics without any consistently defined state of equilibrium to start from. Moreover, after the "cogent examination" of interpersonal decision problems in *Theory of Games and Economic Behavior*, Morgenstern seemed no longer interested in defining, and, what is more important, using a notion of general equilibrium over time for analysing the empirical content of economic theory.

After 1945, Morgenstern switched his attention to the second step of the procedure. Arguably, one reason for this change of interest can be found in the 1935 essay, which concludes with a reference to the need for a more inductive approach supported by statistical data:

> a great number of empirical studies may have to be made ... in order to obtain some kind of a picture about the range of the element of expectations ... It would, for example, be quite conceivable to submit as the adequate data concrete transactions, going on to prove what this result would be, compared with the actual, had different coefficient of expectations been set up.... On the basis of these empirical studies and by means of the materials of experience ... concrete theorems may be handled in such a way that there are discovered expectations and foresight factors, which have been included but generally unexpressed.
>
> (Morgenstern 1935, p. 183)

As will be argued in the following section, Morgenstern's endorsement of the experimental turn in economics derived from the methodological option he favored in the process of giving economic theory empirical content.

As for Hayek's approach, his viewpoint is clearly stated at the very beginning of "Economics and Knowledge:"

> I shall contend that the empirical element in economic theory – the only part which is concerned not merely with implications, but with causes and effect, and which leads therefore to conclusions which, at any rate in principle, are capable of verification (or rather falsification) – consists of propositions about the acquisition of knowledge.
>
> (Hayek 1937, p. 33)

In Hayek's view, economics can claim the status of empirical science only to the extent that it is capable of analyzing the conditions under which "the knowledge and the intentions of the different members of society are supposed to come more and more in agreement" (Hayek 1937, p. 45), as the economy is supposed to be, by assumption, in equilibrium. Indeed, it is in the 1937 essay that one first finds Hayek's claim about the importance of dispersed non-price knowledge, as well as of its diffusion through the market process; this was to become a main recurrent theme not only of his work (Hayek 1946, 1968), but of the modern Austrian school in general (Vaughn 1994, Ch. 4).

Hayek's investigation of the disequilibrium process through which individual knowledge is used, and through which it changes, can be considered as his version of the second step of the two-step procedure. However, although he makes constant reference to the empirical aspect of his analysis, Hayek never sets out to verify (or falsify by means of empirical counterexamples) the propositions put forward. In fact, he advocates (1937, p. 55) a strictly deductive approach:

> in stressing the nature of the empirical propositions of which we must make use if the formal apparatus of equilibrium analysis is to serve for

> an explanation of the real world, and in emphasising that the propositions about how people will learn, which are relevant in this connection, are of fundamentally different nature of those of formal analysis, I do not mean to suggest that there opens here and now a wide field for empirical research. I very much doubt whether such investigation would teach us anything new.

Here, Hayek's lasting faith in the principles of the Austrian school (as compared to Morgenstern's "heterodoxy") is clearly expressed. As briefly recalled in the introductory section, deductivism has been a feature shared by almost all scholars of the Austrian school, beginning with Carl Menger's attack on the German historical school.[20] In Hayek's case, he commits himself to a deductive approach even in the pursuit of the empirical content of economic theory.[21] The rationale for Hayek's commitment to deductivism was the subject of elaboration and revision in later works, as shown in section 4.5.

4.4 Morgenstern and the postwar rise of performed experiments

The postwar debate on the relation between theory and empirics in economics was influenced by the first performed experiments. The Santa Monica conference of 1952 played a crucial part in this story; it has been viewed as the birthplace of experimental gaming.[22] The conference, in which game theorists met experimenters associated with the Rand Corporation, was promoted by the Ford Foundation and Michigan University under the title "The Design of Experiments in Decision Processes." Among the nineteen essays published in Thrall *et al.* (1954), two deserve mention for their influence on subsequent developments, especially in experimental methodology: the essay by Estes (1954) and that by Kalisch *et al.* (1954). Estes's essay, which aimed to show that learning could be represented as a converging stochastic process, triggered off a debate on the informational and computational capacities of economic agents. This debate featured Flood (1954), Simon's *Models of Man* (1957), and Sidney Siegel, who in the early 1960s carried out a laboratory experiment proving the importance of monetary incentives to the behavior of experimental subjects.[23] "Some Experimental n-Person Games" by Kalisch *et al.*, although far from conclusive, showed how game theory allowed translation of the hypotheses to be tested into simple and precise models. The experimental subjects were first instructed in the main principles of game theory and then submitted to the experiment with predetermined time; payments consisted of tokens that were converted into dollars at the end of the experiment. The discussion of results took into account both informational and environmental conditions and the different personalities of the subjects.

These contributions did not go unnoticed by Morgenstern, who attended the conference. In his presentation, Morgenstern (1954, p. 484) argued that performed experiment should have a place in the economist's toolbox:

> I do believe that there exist great opportunities for direct experiments now and in the future. I am thinking of the actual, *physical*, experiment, i.e., one in which physical reality is being subjected to desired conditions, as distinguished from the so-called "thought-experiment."

Morgenstern's confidence in the usefulness of experimentation in social sciences was strengthened by the ensuing experimental activity, which he correctly perceived as an outgrowth of game theory. In the 1950s there were two different approaches to experimentation: the socio-psychological one, represented by experimental gaming, and the economic-managerial approach, expressed by business games.[24] These approaches shared two main features: firstly, the object of verification was usually represented by games in normal form; secondly, their theoretical background related to economics only indirectly, with other social sciences having the lion's share.[25] In business games, in particular, a multiplicity of factors (like the subjects' intellectual ability, patterns of learning, personality traits, and social origin) matched a variety of types of economic behavior to be considered "rational" or "maximizing." As Hoggatt (1959, pp. 192–195) put it: "we focus on using game situations as a research tool for studying the behavior of human beings in conflict situations ... [our aim is] to observe how the subject's actual behavior compares with various types of maximizing behavior as these are visualized in economic theory."

Hoggatt's business game was intended to point out the complexity of human behavior, which did not conform to neoclassical maximizing behavior but was shown to be the outcome of a mixture of conflicting maximum problems. This conclusion closely resembled the view Morgenstern made explicit in the first chapter of *Theory of Games*:

> If two or more persons exchange goods with each other, then the result for each one will depend in general not merely upon his own actions but on those of the others as well. Thus each participant attempts to maximize a function (his above-mentioned "result") of which he does not control all variables. This is certainly no maximum problems, but a peculiar and disconcerting mixture of several conflicting maximum problems. Every participant is guided by another principle and neither determines all variables that affect his interest.
>
> (von Neumann and Morgenstern 1944, p. 11)

Therefore, it is not surprising that, in 1962, Morgenstern, in assessing the meaning of the interplay between game theory and business, noted that

"game theory has clearly established the experimental character of economics. Although in a certain sense a by-product only, this nevertheless heralds a new outlook, a new attitude from which economics in general cannot fail to profit" (Morgenstern 1962, p. 11). The 1972 passage quoted in opening this essay, where Morgenstern acknowledges that experiments were basic both to the establishment and application of new economic theories, amounted to a reference to this development.

The tangle of claims and assumptions from which game theory originated is relevant to this story. The collaboration with von Neumann had a momentous effect on Morgenstern's approach to economics. When the two authors met in Princeton, they shared the view that neoclassical theory was not rigorous enough. The "Austrian" Morgenstern embraced von Neumann's mathematical rigor, but, at the same time, went further than von Neumann in arguing for the application of game theory to economics (Mirowski 1992; Rellstab 1992; Schotter 1992; Leonard 1995). Taking advantage of his pre-war criticism of neoclassical theory, Morgenstern claimed that game theory had the potential for a radical change in both the mathematical and the theoretical foundations of economic science. In particular, game theory could remove the simplifications of the Walrasian system. But, while in the 1950s economists promptly assimilated von Neumann's new mathematical concepts, they neglected Morgenstern's claim that the neoclassical postulate of maximization had to be either weakened or abandoned.[26] The application of game theory to economics was characterized by an emphasis on formal aspects until the 1970s, at least, when the identification of game theory with the theoretical analysis of competitive markets weakened. The heterodox potentialities of game theory have been developed only recently, chiefly through the rejection of that crucial neoclassical postulate, the coincidence between rational choice and the solution of a well-defined maximization problem. In the 1950s, Morgenstern was already aware that game theory was incompatible with the principle of constrained maximization. Dispensing with this essential hypothesis would lead economists to focus on empirically meaningful models of behavior.

In Morgenstern's view, game theory represented the appropriate foundation for a new analysis of the empirical processes of economic exchange. Building on the basis of von Neumann's mathematical apparatus of *Theory of Games*, game theory could provide both an abstract environment for empirical and experimental analysis and counterexamples leading to a re-examination of the basic postulates of the neoclassical approach. In the papers of the 1930s discussed in the previous section, Morgenstern maintained that neoclassical theory made improper use of mathematics. In particular, the formulation of problems was too inaccurate to render their translation into axiomatic terms possible, statements were often treated as proofs, and references to actual economic life were confused and offered no clue as to the relation between theory and empirical evidence.[27] In order to correct these methodological flaws, Morgenstern (1936) proposed the introduction of Hilbertian logic into economics. He intended this type of logic as a formal tool capable of identifying all implications of any proposition with exactitude. As already mentioned, *Theory of Games and Economic Behavior* presented what Morgenstern meant to be the first step of the procedure outlined in the previous section – the definition of economics as an exact science. In arguing in favor of a "process of mathematization" of economics, von Neumann and Morgenstern (1944, p. 4) maintained that the chief reason why mathematics had not been widely used in economics was that "economic problems are not formulated clearly and are often stated in such vague terms as to make mathematical treatment *a priori* appear hopeless." Clearly enough, Morgenstern's agenda did not change over the decades, with the need for empirical analysis intertwining with that for axiomatization, and both stemming from a comprehensive critique of neoclassical economics.

A reconstruction of Morgenstern's contribution to *Theory of Games* that stresses continuity with his previous work might appear to overemphasize Morgenstern's intellectual consistency.[28] However, our point here is that the formal framework emerging from the collaboration with von Neumann constitutes one possible alternative (indeed, the main alternative; see Myerson 1999) to general equilibrium with perfect foresight, and that it signals the definitive turn of modern economic theory towards axiomatics, as Morgenstern advocated. But, at the same time, both a call for methodological "modesty"[29] and the preponderant role of von Neumann's mathematics had the effect of postponing progress in the analysis of the empirical content of economic theory. Nevertheless, it remains true that von Neumann and Morgenstern's (1944, pp. 4, 5) contention is that "the empirical background of economic science is definitely inadequate," and that though "the aim of this book lies not in the direction of empirical research," it may be hoped that "as a result of the improvement of scientific technique and of experience gained in other fields, the development of descriptive economics will not take as much time as the comparison with astronomy would suggest."

Morgenstern followed this line of inquiry in his post-*Theory of Games* activity, starting with his 1950 volume *On the Accuracy of Economic Observations*, an investigation into the reliability of economic data. The book aimed to found a new research area on the measurement of error; its second chapter listed many sources of errors in economic statistics, and the first of these sources was the lack of designed experiments.[30] This deficiency was considered as the main cause of an essential difference between natural and social sciences: while, in the natural sciences, the theorist could confidently rely on the methods by which data were collected, in the social sciences, this confidence was lacking, because the collector and user of data were often different people (Morgenstern 1950, p. 17). Consequently, given the goal of making the interplay between theory and data

collection in economics as close as it was in physics, it was necessary to add performed experiments to the tools commonly employed by economists for the empirical verification of their models.

In 1954, Morgenstern's took up the point on experiment in "Experiment and Large Scale Computation in Economics," which addressed two issues: "first the occasional appearance of strictly planned experiments and second the ability to compute on large scale (with the aid of electronic computers) by making use of currently available theory" (Morgenstern 1954, p. 493). Both these new tools suggested the failings of thought experiments:

> [The thought experiment] is legitimate but exceedingly difficult to handle, hence the numerous times when it has given rise to poor results. It is often restricted to qualitative considerations. When one thought-experiment follows another new difficulties may arise. Length of chains of deduction can itself become a serious logical problem as can be seen from the difficulty of deciding in some mathematical proofs whether the proof is correct or not, if only because it is of "great" length.
>
> (Morgenstern 1954, p. 484)

It is noteworthy that while in the 1930s Morgenstern took issue with the logical consistency of the Walrasian system by means of a typical thought experiment, in the 1950s, he questioned the efficacy of the method of thought experiment through a criticism of the feature previously resorted to, namely, the length of chains of conjectural reactions and counter-reactions, carried *ad infinitum*. This was employed to show, in the 1930s, that perfect foresight prevented any definitive equilibrium resolution, and, in the 1950s, that the use of thought experiments involved serious logical problems. If, in the 1930s, Morgenstern's escape from the indeterminacy of equilibrium was an arbitrary decision breaking the chain of conjectures, in the 1950s, the way out of the indefinite deductive procedure of thought experiment was to perform a laboratory experiment.[31]

To summarize, Morgenstern made an effort to implement a more realistic attitude in economics. Besides methodological pronouncements and analytical groundwork, this effort consisted of writings on temporal data series, statistical errors, and dynamic growth models. Within this framework, he regarded performed experiments as one of the ways to improve the empirical side of economic analysis. But, in his view, scientific enterprise should also have targeted a revision of the theoretical models with which analysis begun. In fact, the conventional conception of economic behavior proved sharply different from that entailed by game theoretical models. This discrepancy has been demonstrated by laboratory experimentation to a degree that goes well beyond Morgenstern's original insights; collecting evidence in simple choice settings like games has disclosed, in Morgenstern's words (1954, p. 496), "some properties of a nature hitherto unknown."

4.5 Empirical research in deductive terms: Hayek's position

In Morgenstern's case, his intention to uncover the critical points of neoclassical theory, in combination with his eagerness to provide economics with fresh empirical content, resulted in an inductive approach, which explains his advocacy of experimental methods of analysis. On the contrary, Hayek's search for the empirical content of economic theory after "Economics and Knowledge" shows lasting confidence in the deductive approach. Starting with the debate on socialist calculation, though, Hayek's work became much more wide-ranging than the pure economic analysis he had pursued in the early 1930s. In this respect, the 1942–44 essay "Scientism and the Study of Society" is of crucial importance, because it can be argued that nearly all of Hayek's mature methodological views are put forward in this essay (Caldwell 1994).

Hayek's essay on scientism lays the basis for a new argument in favor of the deductive method. The main point is that scientism in social sciences, defined as the application of the method and language of natural sciences, is fruitless because the subject matter of the two forms of scientific knowledge is fundamentally different. Namely, the human beliefs studied by the social sciences cannot be reduced to any "objective fact" about the external world, though they constitute a fundamental component of the "external" world to be analyzed by a single individual:

> the facts of social sciences are merely opinions, views held by the people whose actions we study. They differ from the fact of physical sciences in being beliefs or opinions held by particular people, beliefs which as such are our data ... and which we can recognise from what they do and say merely because we have ourselves a mind similar to theirs.
>
> (Hayek 1942–44, p. 47)

As a result:

> It is only by the systematic and patient following up of the implications of many people holding certain views that we can understand, and often even only learn to see, the unintended and often uncomprehended results of the separate and yet interrelated actions of men in society.
>
> (Hayek 1942–44, p. 58)

As anticipated through the excerpt from "Economics and Knowledge" quoted near the end of section 4.3, even this evolved version of Hayek's

methodology lends no support to empirical analysis, as the term is commonly intended. Hayek's deductive method still consisted in obtaining, by means of an effort of imagination, what empirical analysis could, allegedly, not provide by itself. In this sense, thought experiments had a "purely pedagogic" purpose, as Moss (1997, p. 157) points out. But they also represented a tool designed to fill the gap between theory and reality, according to an approach that Hayek would define more clearly in the 1950s and 1960s, building on the basis of the essay on scientism. Actually, the full development of this methodological conception had to await Popper's contribution.[32]

Hayek (1955) holds that the aim of any model is confined to the definition of a certain range of phenomena, supposedly produced by the type of situation under consideration. According to this account, "the selection and application of the appropriate theoretical scheme thus becomes something of an art where success or failure cannot be ascertained by any mechanical test" (Hayek 1955, p. 18). Such a vision of scientific work distanced Hayek from the inductive method based on experiments. According to Hayek, in the social sciences, theories are particularly difficult to prove or disprove. The multiplicity of factors determining any situation prevents the validity of deductive reasoning from being established by direct observation. Therefore, deduction remains the single way to limit the range of phenomena to expect. Deduction relies upon the application of our existing knowledge, which is also a base for new knowledge: "that certain conclusions are implied by what we know already does not necessarily mean that we are aware of these conclusions, or are able to apply them whenever they would help us to explain what we observe" (Hayek 1955, p. 7).[33]

Later, Hayek (1964) coined a specific word to define this kind of theorizing. He argued that the complexity of social phenomena required that analysis concentrated on the recurrence of abstract patterns:

> Such a theory destined to remain "algebraic," because we are in fact unable to substitute particular values for the variables, ceases then to be a mere tool and becomes the final result of our theoretical efforts. Such a theory will, of course, in Popper's terms, be one of small empirical content, because it enables us to predict or explain only certain general features of a situation which may be comparable with a great many particular circumstances.... The advance of science will thus have to proceed in two different directions: while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena.
>
> (Hayek 1964, pp. 28–29)

From our perspective, this means that, if theory tackles subjects so abstract and many-sided that they exclude meaningful predictions, the only viable alternative is to rely upon imagination to distinguish between what is possible and what is not. Hayek's corollary, as expressed in the quotation that opens this essay, was that the usefulness of performed experiment was invalidated by the consideration that it could be employed only for facts "already known to the observer" (Hayek 1968, p. 180).

Indeed, Hayek based his analysis of the market process and of competition as a discovery procedure mostly on deduction and thought experiments. A substantial continuity characterizes his approach, as is exemplified by its relationship to the Walrasian equilibrium model. In the 1930s, Hayek pointed out its conceptual and methodological flaws for the analysis of the competitive process in conditions of dispersed knowledge. In the perspective of the "Scientism" essay, as well as of the following methodological essays, a Walrasian system of equations is not capable of mastering the patterns which emerge when certain conditions are satisfied, because it is designed to provide "point explanations" like those provided by the natural sciences; but the subject matter of economic analysis is, by its very nature, different from that of the natural sciences. In fact, when complex phenomena are investigated, only "explanations of the principle" can be sensibly formulated. As a result, the notion of spontaneous order (Hayek 1968) is offered as an alternative to Walrasian general equilibrium for the analysis of pattern coordination through the market process. Its purpose is to elucidate the very general conditions under which coordination holds. But its application necessarily entails overlooking the particular circumstances that determine pattern coordination. In this abstract environment, thought experiments can perform their distinctive function, that is, discovering empirical information through experiments that cannot be carried out, but only imagined.[34]

4.6 Concluding remarks

This essay has analyzed how two Austrian economists, Hayek and Morgenstern, dealt with the relationship between thought- and performed experiments. We have maintained that, prior to the 1940s, both Hayek and Morgenstern applied the deductive procedure of thought experiment. On such a basis, they claimed that economics could improve its methodological status if it developed according to a two-step procedure. The first step was to become an exact science thanks to the analytical power guaranteed by the adoption of the axiomatic method; the second step amounted to a careful and precise definition of the empirical content of economics.

After the 1940s, and in particular after the advent of game theory, Hayek and Morgenstern dealt with the empirical content of economics in different ways. On the one hand, Hayek relied on his own definition of

empirical analysis as a purely deductive investigation into the process by which individual knowledge changes. Experiments designed to test "artificially created real situations" would not work, because the phenomena of economic life were complex and irreducible to "objective data." On the other hand, Morgenstern promoted the introduction of experimental methods into economics, to take place through the adoption of laboratory procedures transposed from the natural sciences. This change of perspective was a consequence of the flourishing of experimental activities stimulated by game theory. An overview of the early years of experimental economics shows, first, that game theory allowed translation of models into verifiable hypotheses concerning simple choices, and, second, that it created an appropriate setting for experiments, disclosing properties of human behavior that challenged certain basic assumptions of neoclassical economics. These two considerations explain why Morgenstern, contrary to Hayek, changed his mind about thought experiments, and encouraged performed experiments as a useful tool to carry out the empirical step of economic inquiry.

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Notes

1 The place of Morgenstern in the history of economics is the subject of very different, and sometimes opposite, assessments. For instance, Boettke (1994, p. 2), who considers Mises and Hayek as the main representatives of the Austrian school, and views it as an alternative approach to mainstream economic theory, argues that "individuals like Schumpeter, Haberler, Machlup, Morgenstern and Robbins would carve out their own unique place within economics for their theoretical nuances (due in large part to Austrian themes of imperfect knowledge, dynamic market process, the importance of time and methodology), but they were still viewed by most other economists, and most importantly by themselves, as mainstream neoclassical economists." In the same volume, Schotter (1994, p. 556) opens his essay on social institutions and game theory by observing that "even a casual reading of the introduction of their book indicates that von Neumann and Morgenstern viewed game theory as a unifying theory for the social sciences and not as a narrow replacement for neoclassical economic theory." That Morgenstern's main contribution to economics is the abandonment of the orthodox conception of economic behavior is argued for in Innocenti (1995). Morgenstern's contribution to the theory of games is dealt with in Leonard (1995).

2 This is well exemplified by Mises's "evenly rotating economy." In Mises's work (1963, pp. 244–250), the role of entrepreneurship emerges through a contrast with individuals operating in a timeless, static economy in equilibrium.

3 For a recent, similar viewpoint on equilibrium models see Hahn (1996). The relation between general equilibrium theory and experimentation is discussed at length in Rizvi's chapter.

4 As will be shown, this holds true in spite of both Morgenstern's contribution to the axiomatization of economics in *Theory of Games and Economic Behavior* and Hayek's empirically oriented analysis of alternative institutional environments.

5 We are referring to their different political positions, underlying personal rivalry. On this aspect see Leonard (forthcoming, Ch. 3). It is likely that political views helped to shape their different attitudes towards economics since the 1930s. Since this essay focuses on methodological themes strictly intended, these questions will not be considered in what follows.

6 For instance, Hayek, in the tradition of Wieser, repeatedly referred to an imaginary centrally planned economy in order to highlight the beneficial effects of having decisions decentralized among individual agents in actual economies (in particular, see Hayek 1945).

7 On the relationships between the two circles, and especially on Morgenstern's late dissatisfaction with Mayer's causal-genetic approach, see Böhm (1992). On the Austrian circles in the inter-war period see also Craver (1986) and Kirzner (1994).

8 In the 1935 essay, Hayek's main goal was to show how a theory of business cycle could be based on "expectations inevitably doomed to disappointment." The essay was a reply to Myrdal's allegation that there was no role for expectations in Hayek's trade-cycle theory. This focus on trade-cycle theory may explain why Hayek did not elaborate further the notion of "correct foresight." When the essay was translated into English (1939), Hayek added a footnote to the paragraph just quoted, remarking that "Economics and Knowledge," published in 1937, contained a more elaborate and "partly revised" analysis of the relationship between equilibrium and foresight. As will be argued later, this footnote originated from Morgenstern's attack on this point.

9 Morgenstern quoted the entire passage in the text of his article. Since Morgenstern's article was translated into English (by F. Knight) before the English version of Hayek's article was released, there are some minor differences between the passage as cited from Hayek (1935) and the version given in Morgenstern (1935).

10 Moreover, in Morgenstern's view, the implicit assumption that "there is *identity* between *foresight and the expectation of the future*" shows that those economists were unaware of the difficulties stemming from the introduction of the expectation element. In this connection, Hayek's 1935 essay is quoted once more as a (negative) example.

11 It is appropriate to remark that the term "demi-gods" is largely equivalent to both those of "superoptimizer," as in Winter (1985), and "homo-rationalis," as in Aumann (1985). These terms are used to indicate the knowledge abilities attributed to individual agents in current general equilibrium theory and game theory respectively.

12 The paradox concerns the paralyzing effect on actual action of thinking strategically in a two-person game. The example is that of Moriarty's attempt to induce Holmes to leave London in order to catch him in Dover. Holmes's option to get off the train to Dover at an intermediate stop gives birth to a chain of mental reactions to the expected behavior of the other player. However, Morgenstern's reasoning does not consider the possibility of using mixed strategies. On this point see Bicchieri (1993).

13 For a society, then, we *can* speak of a *state* of equilibrium at a point in time – but it means only that the different plans which the individuals composing it have made for action in time are mutually compatible. And

> equilibrium will continue, once it exists, so long as the external data correspond to the common expectations of all members of the society.
>
> (Hayek 1937, p. 41)

14 In modern equilibrium theory, this point has been made explicit by Hahn (1984). It has been convincingly argued that Hahn's definition of equilibrium, which still sets the standard in current equilibrium theory, is nothing but a stochastic version of Hayek's definition (Littlechild 1982). On Hayek's notion of equilibrium as a state of mutual compatibility of plans, see Zappia (1996).

15 Morgenstern's endorsement of the axiomatic method figures one year later in the essay "Logistic and the Social Sciences:" "Beside the axiomatic method there is the genetic method which may even have a higher didactic value. But in order to gain rigorous insight into the state of any science, the use of the axiomatic method cannot be dispensed with" (1936, p. 396). It hardly needs observing that this shift of approach culminates in the introductory chapter of *Theory of Games and Economic Behavior*.

16 To assess the relevance of this task to current economic theory, compare Arrow's remarks (1996, pp. xiii–iv), introducing a recent volume on the status of the rationality hypothesis:

> Interactive rationality is relevant when the payoff of any agent depends on the action of others. In general, then, the best choice of action by A depends on the actions of B and *vice versa*. But how can A know the actions of B? Is it possible to have knowledge (even probabilistic knowledge) of the actions of another? Competitive equilibrium theory provides an answer to this conundrum, game theory (Nash equilibrium) another, each with its own assumption. But the deeper logical question is, how do either of these equilibrium concepts come into being? And, of course, the second question is, are the answers empirically convincing?

17 Morgenstern (1935, p. 174) argues:

> The resulting events are so extremely complicated that only far-reaching employment of mathematics could help to suggest reciprocal dependencies. The relationship between human behaviours dependent on one another, even without the assumption of perfect foresight, is almost inconceivably complicated, and it requires cogent examination.

18 It has been noted that Morgenstern's 1935 essay anticipated the rationale of rational expectations hypothesis (amongst others, see Arrow 1986, and Schotter 1992). This is certainly correct if reference is made to the informal model just mentioned. Morgenstern (1935, p. 177) perceptively observes that if it is maintained that, in order to have equilibrium, "it is enough if every individual belonging to the economy concerned simply knows what the concrete situation will be on a certain future market," then this is incorrect because "it is also posited by the theory that individual acts *rationally*. However, this "'rationality' posits, in its turn, that the economic subjects themselves perceive the connections and dependencies – that they really see through the relationship to a certain degree." Nevertheless, Morgenstern seems unaware that the requirement of "rationality" attributed to Walrasian (and Paretian) equilibrium, which he understands as something more than individual rationality, is the equivalent of the perfect foresight assumption in a context of uncertainty. If the actions of individual agents vary continuously with foresight and the future realization of relevant variables is a continuous function of actions, it is possible to show that there exists a foresight that would cause itself to be true, as happens with a rational expectations equilibrium. Apart from the information requirement that the rationality of agents is common knowledge, this theorization is equivalent to Morgenstern's argument on rationality (Radner 1989). As a result, the logical impossibility of a rational expectations equilibrium cannot be argued on these grounds.

19 In an attempt, admittedly provisional, to clarify the meaning of "degree of foresight," Morgenstern proceeds by distinguishing between an individual's insight into mutual relationships, which he terms "technical foreseeability," and "effective foresight," that is "knowledge of individual historical events and occurrences" (1935, p. 179). But the discussion of these two different notions of foresight makes it clear that at this juncture Morgenstern did not have a probabilistic notion of perfect foresight, which he actually considered as a synonym for effective foresight. True, a genuinely probabilistic view of the introduction of time into equilibrium theory cannot equally be found in other contemporary authors, such as Hayek, Hicks, and Myrdal, to mention those referred to by Morgenstern. However, in Morgenstern's case, this absence has damaging implications because he does not seem to grasp that, in the 1930s, the generally accepted meaning for perfect foresight is more similar to "technical" than to "effective" foresight. A better understanding of the requirements for equilibrium over time emerges from Hayek's (1937, p. 42) definition of "correct foresight," to which consideration has already been given. This point is dealt with in detail in Zappia (1999).

20 The achievements and shortcomings of historical empiricism and inductivism, as opposed to Austrian deductivism, have been recently assessed in Grimmer-Solem and Romani (1999).

21 One point should be made before moving on. Hayek's deductivism is substantially different from Mises's apriorism. Mises based his aversion to empirical testing on the methodological viewpoint that the fundamental postulates of economic behavior are to be considered true independently of real experience, that is, they are Kantian synthetic a priori propositions. Hayek, on the contrary, does not deny that a priori propositions by and large reflect structures and connections among objects of economic reality; nevertheless, he maintains that these propositions can be discovered as a result of a mostly deductive theoretical effort (see Smith 1994).

22 For the history of experimental economics, see Smith (1992), Roth (1993, 1995), and Rizvi's essay in this volume. A detailed analysis of the Santa Monica conference can be found in Dimand's essay.

23 The influence of Siegel on experimental economics is discussed in Smith (1992).

24 Experimental games were performed mainly by social scientists (psychologists, sociologists, philosophers and decision theorists) belonging to a closely knit community, chiefly financed by the military. Deutsch (1958), Flood (1958), Loomis (1959), and Scodel *et al.* (1959) tested the Prisoner's Dilemma; Vinacke and Arkoff (1957) verified the coalition theory proposed in *Theory of Games and Economic Behavior*; Mosteller and Nogee (1951), Allais (1953), Edwards (1953), Flood (1955) and Davidson *et al.* (1957) conducted experimental tests of the utility function proposed by von Neumann and Morgenstern in *Theory of Games*; Stone (1958) and Schelling (1958, 1959) verified Nash's bargaining theory and the theory of focal points. Business games, on the other hand, were developed by business-school economists as tools for training and selecting managers. The first business game was performed by a group of economists and managers directed by Richard Bellman, and it was published in 1957. Later, Andlinger and Greene set up a "business management game" (Andlinger 1958), and a group of IBM researchers organized a laboratory to make experiments in decision analysis (International Business Machines 1958a and 1958b). Finally, Hoggatt (1959) used a business game to test Cournot's model.

25 The first feature has been documented by an exhaustive review of experimental gaming in the 1950s (Rapoport and Orwant 1962). The article discusses over forty experiments and shows how each of them can be represented as the verification of a game in normal form. The second feature has been corroborated by an authoritative observer, Herbert Simon: "I do not think that the impetus for experimentation within a game-theoretical framework initially came from economists, but rather from psychologists (particularly those who had begun to build mathematical learning theory), statisticians, and interdisciplinary types close to cybernetics and management science" (quoted in Smith 1992, pp. 253–254).

26 On Morgenstern's attitude towards this issue, see Innocenti (1995). Von Neumann's way forward in the process of abandonment of the neoclassical postulate is discussed at length in Mirowski (2002).

27 See in particular Morgenstern's (1941) harsh review of Hicks's *Value and Capital*.

28 From both a theoretical point of view (e.g., the static, un-Austrian nature of von Neumann's solution to the problem of individuals' interaction), and a biographical one (Morgenstern's moving to the United States due to the degenerating political situation in Austria), there occurred dramatic changes which are not accounted for in our reconstruction.

29 > The field covered in this book is very limited, and we approach it in the sense of modesty. We do not worry at all if the results of our study conform with views gained recently or held for a long time, for what is important is the gradual development of a theory, based on a careful analysis of the ordinary everyday interpretation of economic facts.
> (von Neumann and Morgenstern 1944, p. 7)

30 Among these sources, Morgenstern (1950) indicates deliberate hiding of information, low training of observers, failure of questionnaires, aggregation of data, lack of definition or classification, errors of instruments, consideration of discrete rather than continuous intervals of time, and interdependence or stability of errors.

31 In the 1954 article, Morgenstern stresses that the validity of direct experiments rest on their similarity with the experiments performed in physical sciences. In this light, while Chamberlin's (1948) early experiment can be useful "for pedagogical purposes" only, both Mosteller and Nogee's (1951) and Edwards's (1953) experiments on gambling situations are praised because they can provide "a theory of utility of a truly scientific character, removed from the realm of pure speculation." And these experiments, Morgenstern (1954, p. 502) contends, "are on a borderline of economics; they connect with fields where experience with experiments has already been obtained." Again, it was a comparison between economics and natural sciences that led Morgenstern to consider performed experiments as a useful tool for economists.

32 However, see Caldwell (1992) for a different viewpoint.

33 Cartwright's essay in this volume discusses the rationale and limits of the use of deduction in mainstream economic theory. In particular, her discussion of the way economic models can teach us "genuine truths about empirical reality" (p. 137) is reminiscent of Hayek's.

34 Boettke (2000) has argued that Hayek's interest in the impact of alternative institutional environments on the process of coordination of individual plans can be interpreted as an empirical investigation in its own right. In Boettke's view, the fact that Hayek is not in principle averse to both prediction of "patterns" and the empirical recognition of economic regularities allows for a comparison between the implications of a theory and the observation of these regularities.

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